Incarceration *versus* probation?
Long-run evidence from an anticipated reform

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Abstract

How do individuals convicted to incarceration fare in terms of later crime and labor market outcomes compared to those who receive a non-custodial sentence? 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: we observe a significant selection in the nature of the cases tried before and after the reform. To measure its impact, we 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 commit more crimes and have weaker ties to the labor market than those placed on probation. The effects are particularly strong among young offenders. Our findings suggest that economic precariousness is an important mechanism explaining subsequent criminal behavior.

JEL Codes: K14, K42, J24

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1. Introduction

Over 10.74 million individuals were detained in penal institutions throughout the world in 2018 (Walmsley, 2018), a number which has been steadily increasing over the last four decades. Nowadays, the average worldwide prison population rate is around 145 per 100,000, a figure that is close to the OECD average. However, since the early 2000s, several OECD countries have started implementing policies aimed at lowering incarceration rates, such as the introduction of electronic monitoring or the increased use of probation time. Yet, evidence on the relative impact of incarceration compared to less severe legal sanctions remains very limited. Regarding electronic monitoring, Di Tella and Schargrodsky (2013), Marie (2015) and Henneguelle et al. (2016) all find that this alternative sanction reduces recidivism compared to incarceration, in very different contexts (Argentina, England and Wales, and France respectively). A recent strand of the literature uses the severity of randomly assigned judges in order to identify the causal effect of incarceration relative to less severe sanctions, generally probation, on a variety of outcomes, and reaches mixed conclusions (see for instance Kling 2006; Aizer and Doyle, 2015; Mueller-Smith, 2015). However, most of this literature builds on countries where incarceration conditions are particularly poor (first and foremost the US[1]), and evidence remains close to absent in settings where prison population is lower[2], detention conditions are better, and rehabilitation programs play a greater role in prison. One notable exception is the recent Norwegian study by Bhuller et al. (2020), finding that imprisonment discourages further criminal behavior due to rehabilitation programs.

Our paper contributes to filling this gap by providing robust evidence on the relative impact of custodial and non-custodial sentences in Denmark, where incarceration conditions are considered to be particularly advantageous.[3] To do so, we study a large-scale reform of the Danish legislation implemented in 2000, whereby jail time (a custodial sentence) was replaced by a probation period (a non-custodial sentence) for drunk-driving crimes.[4] This allows us to measure the relative impact of

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[1] The US is a clear outlier in the OECD. Its prison population rate is the highest in the world, with 622 per 100,000. By contrast, with an incarceration rate of 235 per 100,000, Lithuania is the first European country.

[2] In 2018, while the US prison population was at 655 per 100,000 inhabitants, it remained 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).

[3] See for instance Lappi-Seppälä (2007), Pratt (2008), Pratt and Eriksson (2011), and Ward et al. (2013).

[4] 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).

probation compared to incarceration on a large group of relatively mild offenders, which accounted for a quarter of all custodial sentences promulgated at the time. Importantly, our analysis is carried out on offenders who did not exhibit a strong alcohol abuse problem, which mitigates the external validity concerns arising from drunk-drivers' specificities.

We first document the presence of important selection in cases tried just before the reform, which precludes us from comparing offenders tried before and after the reform. Instead, we use a novel instrumental variable approach exploiting two features of the justice system: the significant case processing time and the fact that, in Denmark, individuals tried after a reform for a crime committed prior to it must be tried under the most lenient of the two laws. Combined together, these institutional features generated exogenous variation in the probability of being incarcerated among individuals arrested before the reform depending on the time lapse between the date of their crime and the date when the reform entered into force: the closer to the reform a crime was committed, the more likely a defendant was to be tried under the new law and placed on probation instead of being incarcerated.[5]

Using this instrumental variable, we find that non-custodial sentences significantly decrease offenders' involvement in subsequent criminal activities, relative to custodial sentences. Overall, while we do not find any impact on offenders' probability of committing another crime, we find that probation significantly decreases the average number of crimes offenders subsequently commit. Hence, although incarceration does not increase the number of reoffenders, it intensifies their subsequent criminal activities. After 8 years, incarceration increases the average number of convictions by 0.623 crime – representing a 30.3% increase at the sample mean. This overall effect is not driven by an increase in the number of drunk-driving crimes but rather by a rise in the number of other crimes. This suggests that the criminogenic effects custodial sentences were found to have on other offenders (Cullen et al., 2011; Aizer and Doyle, 2015) can affect a broad range of offenders, including subsets who may exhibit relatively low proclivity for criminal behaviors, such as drunk drivers. Interestingly, we find that this increase in other crimes is steered by a boost in the number of

[5] Our approach is loosely related to the one used in Drago et al. (2009). In their study, they used 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 prison population of Italy 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.

economically motivated crimes, suggesting that incarceration may significantly increase offenders' precariousness.

Our subsequent analysis of offenders' post-sentencing labor market attachment confirms that custodial sentences increase the difficulties faced by offenders in the labor market as compared to non-custodial ones. Indeed, we find 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 kroners – corresponding to a 15.2% decrease at the mean, or one and a half year worth of income. The timing and nature of the effects suggest that difficulties in the labor market may explain part of the observed rise in criminal activities. More generally, these results also provide additional evidence that the social cost of incarceration can span beyond the mere period of incarceration, and suggest that the implementation of accompanying post-release measures may be necessary to mitigate these costs.

Finally, we find substantial heterogeneity in our results based on offenders' age and past labor market attachment as we show that our results are primarily driven by young offenders and, in particular, those who had a job at the end of the year preceding their crime. Presumably, these offenders were those who had the most to lose from being incarcerated at a time when they should have been strengthening their professional network and accumulating experience. The negative effects of custodial sentences (relative to non-custodial ones) are more limited on older offenders, as well as on those who were already struggling on the labor market.

Our findings contribute to the broad literature studying the social and economic effects of incarceration, both at the intensive margin (duration of prison time) and, more recently, at the extensive margin (incarceration versus alternative sanctions).[6] Although longer incarceration spells may deter post-release criminal behavior (Drago et al., 2009), spending time in prison may also serve as a "school of crime" through exposure to other criminals, thus increasing future crime outcomes (Bayer et al., 2009; Stevenson, 2017; Piil Damm and Gorinas, 2020). Regarding labor market outcomes, incarceration has also been found to harm future employment prospects (see for instance Kling, 2006 or Mueller-Smith, 2015). Outside of the US, the literature is more limited and fails to reach a clear consensus. In a recent paper using Norwegian data, Bhuller et al. (2020) exploit the random allocation of criminal cases to judges with varying incarceration propensities and find that

[6] See Chalfin and McCrary (2017) for an extensive review of the literature.

former inmates recidivate less and face improved labor market outcomes when they were not employed prior to incarceration. However, alternative studies focusing on other Nordic countries and relying on a similar identification strategy reach contrasting conclusions that do not support the rehabilitative role of prison. In Sweden, Dobbie et al. (2018) find incarceration to have little impact on the probability for offenders to commit another crime, but a large negative impact on employment. In Denmark, Michel et al. (2019) find that incarceration increases the number of crimes offenders commit and reduces their labor market attachment. In these three Nordic studies, the results are identified on a very specific subsample of defendants at the margin of being incarcerated. By contrast, our paper focuses on a broader range of offender types arrested for a very widespread crime, *i.e.* drunk driving, thus enabling us to make a significant contribution to this debate in the context of a Nordic country. In this respect, our paper also relates to the literature looking at the effect of incarceration conditions, as we focus on a context where incarceration conditions are viewed as particularly exceptional by American and European standards (see for instance Pratt, 2008). Several studies, such as Chen and Shapiro (2007) for the US or Drago et al. (2011) for Italy, reveal that harsher prison conditions (measured by the intensity of security, the degree of social isolation, or inmates' death rate for instance) increase post-release criminal activity. The recent study by Bhuller et al. (2020) in Norway rather suggests that when incarceration conditions are favorable, the rehabilitative effect of incarceration can dominate. Our results are instead surprisingly similar to those found in the US and imply that incarceration can remain a harmful experience with long term negative consequences even in Nordic countries.

Our study also makes an important side contribution as we show that individuals, especially wealthier ones, can anticipate the consequences of a reform and act upon it. Analyzing how the reform was implemented, we find evidence that offenders anticipated that they could avoid prison by postponing their trial until 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), as well as a linear decrease in the share of defendants receiving a custodial sentence. We also show that wealthier defendants were more likely to "game the system" and avoid prison. To our knowledge, we are the first to provide such striking evidence in the context of a justice reform. 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. Incidentally, our findings also question the degree of fairness with which cases can be handled in times of legislative changes and add 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).

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.

2. The legislative change

2.1. Context of 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. 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. In 1999, drunk driving was the crime responsible for the largest number of custodial sentences promulgated, accounting for 24.82% of them.

In 1999, the vast majority of individuals tried for a drunk-driving crime were convicted and incarcerated, as displayed in *Table 1*.[7] In total, only 1.3% of the defendants tried were acquitted and 71.9% received an unconditional prison sentence. 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 incarceration spell remained relatively short: in 95.3% of the cases, it remained below 60 days. Additional sanctions, such as fines and suspensions of the driving license, were also frequently imposed on the defendant – in 27.8% and 30.8% of the trials respectively. Conversely, conditional prison sentences (probation) and community work were seldom used to sanction drunk drivers.

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 unconditional prison sentence could be commuted to a non-custodial sentence involving a two-year probation period and mandatory

[7] In Denmark, all individuals facing a prison sentence are tried in a court of justice.

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 an unconditional prison sentence (71.9%).[8]

Offenders who served their prison sentence usually did so in one of the country's open prisons.[9] These prisons, which account for about a third of the total Danish prison capacity, are low-security facilities where fences, walls, and barriers are minimal, offering softer detention conditions for the inmates. Generally speaking, these detention facilities are meant to support the principle of *normalization*, officially introduced in Denmark in the early 1970s, according to which life in prison should reflect life outside and correspond as much as possible to conditions in the general community.

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 mandatory 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.).[10,11] 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).[12]

The choice between community service or mandatory 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. 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

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[8] Our variable indicating whether or not an individual was incarcerated is a dummy variable which captures 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.

[9] Individuals serving a prison sentence inferior to four years and presenting no security threats to prison staff and other inmates are usually incarcerated in open prisons. In practice, offenders incarcerated for 60 days or less cannot apply for early release on parole.

[10] 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.

[11] Generally speaking, offenders placed on probation see their prison sentence suspended on the condition that they do not reoffend and that they observe any conditions that may be imposed.

[12] The cost of the non-custodial sentence includes the costs associated with offender supervision and 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 individuals' 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.

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 was substituted to the former sentences at the following rate: 30 hours for 10 to 14 days of imprisonment, 40 hours for 20-30 day sentences, and 60 hours for 40 to 50 days in jail.[15]

As displayed in *Table 1*, the share of offenders who received an unconditional prison 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 received a conditional prison sentence and were placed on probation rose from 0.7% to 59.0%. As the reform did not change the punishments 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),[16] 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 detailed 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, even when they are incarcerated in an open prison. In particular, Basberg Neumann, a sociologist specializing in Nordic prisons and emphasizes the fact that inmates' perception of prison conditions is largely determined by their frame of reference, namely the generous Scandinavian welfare system and institutions (Basberg Neumann, 2012). Importantly for the present study, social stigma upon release can play a major role in the rehabilitation process. In particular, suspended prison sentences remain on an individual's criminal record for 3 years from the conviction

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[15] 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.

[16] Generally speaking, the reform applied to all offenders but extreme repeat drunk drivers and offenders facing extreme aggravating circumstances. Also, it 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 already on probation when they were apprehended for a drunk-driving crime.

date, while unconditional prison sentences stay on the record for 5 years from the date of release from prison.

3. Defendants anticipating the reform and gaming the system

As is often the case for 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).

In this context, an important feature of Danish legislation lies in that it 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 – irrespective of the date of their crime. In practical terms, it 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 would be placed on probation if tried after. Thus, to the extent that defendants anticipated the reform and its consequences, they faced a clear incentive to try and postpone the date of their trial until after the reform in order to avoid prison. Below, we provide clear-cut evidence that this is precisely what happened with a large share of the drunk drivers postponing the date of their trial until after the reform. Crucially for our analysis, we further reveal that the characteristics of the individuals who gamed the system are not random and that wealthier defendants were more likely to do so.

3.1. Anticipation

First, we provide evidence that defendants anticipated the reform and modified their behavior from the moment the law was signed.

To do so, 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. In *Figure 1*, we show 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, we draw two dotted vertical lines marking 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 occurred in 2000. For the years 1999 and 2001, vertical lines were only drawn for comparison purposes.[17]

Strikingly, the evolution of these indicators reveals that the way in which drunk-driving cases were handled in district courts changed drastically in the months 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 (after the law was signed but before it entered into force) – representing a 69.5% decrease (*Figure 1.b*).[18] 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 or in the number of cases tried during the same period in adjacent years, 1999 and 2001 (*Figure 1.a*).[19] This suggests that stakeholders (courts of justice and/or defendants) anticipated the change in legislation and that, as a consequence, a large share of trials were postponed until after the reform. Hence, a group of offenders who should have been tried before the reform was tried after.[20]

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. Overall, the share of defendants receiving a custodial sentence decreased progressively 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% decrease. Interestingly, while the evolution of the share of offenders actually incarcerated exhibits a similar pattern, it started to decrease a year before the date of the reform, suggesting that the Prison and Probation Service in charge of enforcing the sanctions may have anticipated the reform even further. Another possible explanation lies in the waiting list system adopted in Denmark after a sharp rise in

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[17] For data confidentiality reasons, indicators c) and d) displayed in *Figure 1* are calculated as moving averages. For each year *y*, the value of these indicators is calculated as the average value of the indicators over years *y-1*, *y*, and *y+1*.

[18] 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.

[19] 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.

[20] 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 number of individuals who received an unconditional prison sentence. As a consequence, not all offenders served their prison sentence immediately after their trial.

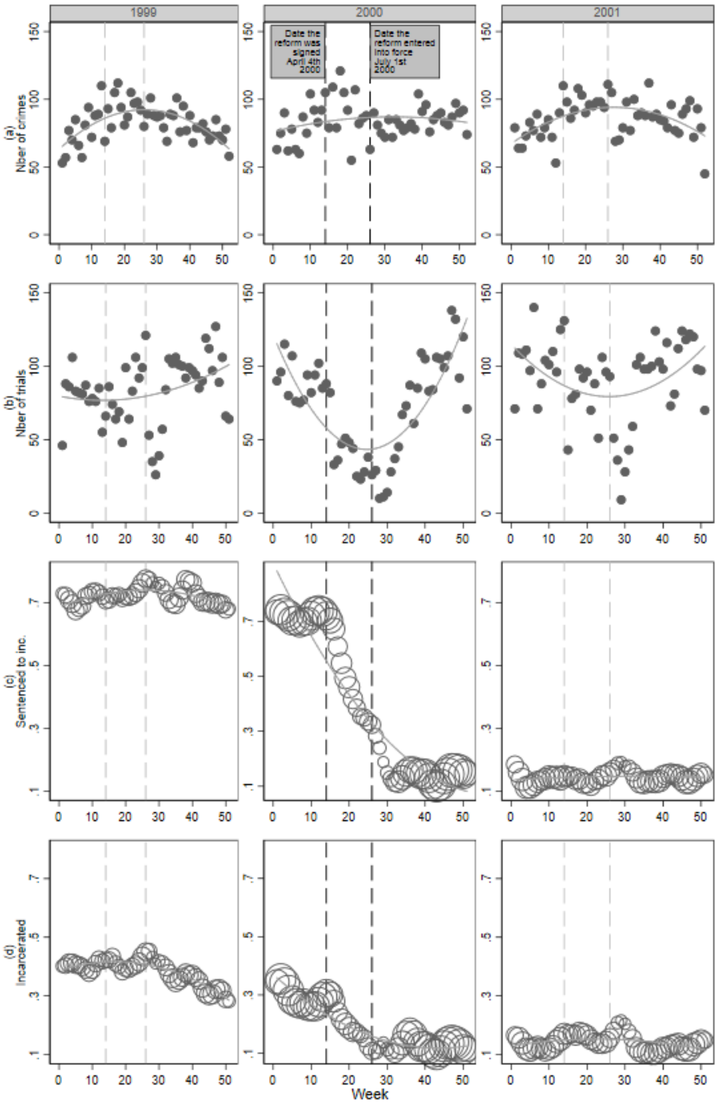

Figure 1 – Implementation of the drunk-driving legislation reform: The consequences of the reform are depicted here through the evolution of the following four indicators around the time of the change in legislation: a) the number of drunk-driving crimes resulting in a trial committed every week; b) 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 entered into force (week 26).

While we are not able to pin down the exact underlying mechanisms at play here, we believe that both defendants and judges had incentives to postpone drunk-driving cases until after the reform. As already discussed above, defendants had an incentive to ask for the postponement of their trial to avoid prison. Interestingly, judges had an incentive to let them do so to reduce the number of cases which might have to be retried. Indeed, Danish legislation also guarantees that defendants tried prior to the passing of a law lowering the sanction for the crime they were convicted of may request a retrial if they are still in prison (or on the waiting list to be incarcerated) when the reform enters into force.

3.2. Selection

Going further, we investigate the characteristics of the defendants who acted in anticipation of the reform and got their case postponed, and find evidence suggesting that the selection 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 on a constant, a year dummy indicating whether a case was tried in 2000, quarter fixed effects, the interactions between the year dummy and the quarter fixed effects, and a time trend. The coefficients associated with the year dummy thus capture differences in the characteristics of the defendants tried in the first quarter of 1999 and 2000, while the three interaction terms 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, the coefficients associated with the interaction of the year and 2nd quarter dummies allow us to capture 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, while in *Panel B*, we focus on the subset of offenders who received a prison sentence (whether conditional or unconditional) as the group of defendants who had the most to gain from postponing the date of their trial.

While we do not find evidence of any change in the nature of the cases tried in the first and fourth quarters between 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 we focus on the entire sample of defendants tried during the period, 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 postponing their trial. 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 postpone their case until after the reform than other defendants – presumably because they had access to better legal counsel.

We can rule out the possibility that this selection merely reflects an attempt to focus on offenders whose trial outcome did not depend on the timing of the trial during the transition period. For instance, we do not observe any change in the average number of drunk-driving crimes committed by offenders tried during the transition period, although the reform did not apply systematically to repeat drunk-drivers.[21] Overall, we actually find that defendants tried during the transition period tended to be more severe offenders. On average, individuals tried in the 2nd quarter of the year 2000 had been convicted and incarcerated a greater number of times for crimes other than drunk driving. Again, the magnitude of the differences is particularly important and significant for the subset of offenders who received a prison sentence. For instance, at the sample mean, they represent increases of 31.0% in the number of crimes and of 52.0% in the number of incarceration spells recorded by the defendants in the previous 5 years. These results further suggest that defendants tried during the transition period were in a more precarious situation.

Finally, we still 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, 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 the defendants tried before and after the reform. This also raises questions

[21] Offenders who had already been placed on probation for a drunk-driving crime more than once or who were on probation at the time of the crime for an alcohol-related crime.

with respect to the performance of traditional quasi-experimental estimators in the context of this reform and other similar ones.[22]

4. Empirical strategy

In order to measure the causal impact of the reform and bypass the selection problem documented above, we use a novel instrumental variable relying on variation in the probability for offenders to receive a custodial sentence. This variation, which we argue is plausibly exogenous, is generated by the time lapse between the date of their crime and the date when the reform entered into force.

4.1. The intuition behind the instrument

Our 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. It 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.

In *Figure 2*, we provide evidence of the strength of this approach. In order to do so, we organize the data 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 depict the following indicators: 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,

[22] Traditional quasi-experimental estimators raise additional selection problems which, although not discussed in details here, remain essential. In particular, one concern is that the entering 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 costs 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 lower 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*.[23]

The evolution of the first two indicators provides graphical support for our approach. Around the time of the reform, the period of time between the moment when a prosecutor would press charges against an alleged drink-driver and the moment when a district court rendered its decision was substantial. On average, the time gap was of 6 months for drunk-driving crimes committed in 1999 and it increased for crimes committed closer to the reform (*Figure 2.a*). 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 – although the decrease starts earlier (*Figure 2.d*).

[23] For data confidentiality reasons, indicators c) and d) displayed in *Figure 2* are calculated as moving averages. For each year *y*, the value of these indicators is calculated as the average value of the indicators over years *y-1*, *y*, and *y+1*. Furthermore, for any given week, the number of cases tried after the reform is normalized to 1 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 number 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).

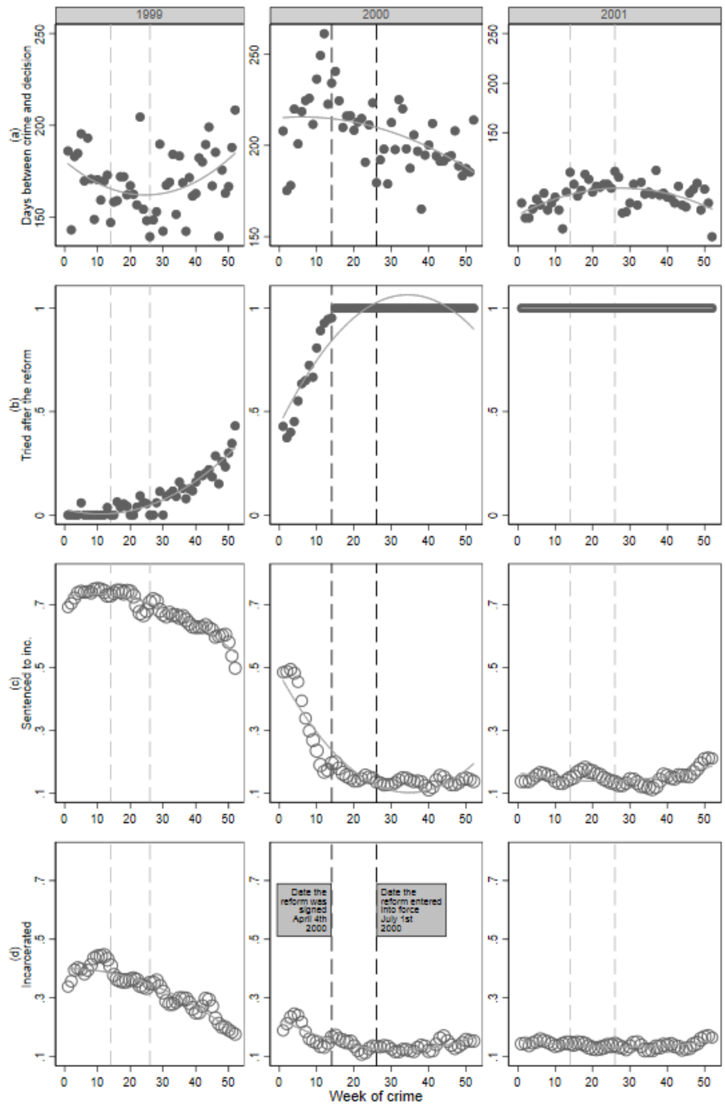

Figure 2 – Motivation for the instrumental variable approach: This figure depicts the evolution of the following four indicators around the time of the reform: a) the average time gap between the moment a crime is committed and the moment the decision of justice is rendered by a district court by *week 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 entered into force (week 26).

4.2. Sampling strategy

Our approach therefore compares individuals who committed their drunk-driving crime *before* the signature of the reform based on the date of their crime.

In what follows, 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).[24] 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 entering into force of the reform (between week 15 of 1998 and week 14 of 1999), these individuals are included in our sample as well so as to control for seasonal variations using both a time trend, 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.[25]

In *Table 3*, we provide a description of 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 are in a relationship (28.6%), and defendants born abroad and descendants of immigrants represent 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) derived from the estimation of the following equation:

$$y_{i,t} = \delta^{RF}(\overbrace{P_i * T_i}^{I_i}) + X_i\beta + \mu_1 T_i + \mu_2 P_i + \mu_m + \mu_c + \varepsilon_i \qquad \textbf{(1)}$$

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

[25] 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).

where $y_{i,t}$ is the outcome of interest for individual *i* measured at time *t*; T_i is a trend, increasing with time, which captures the time gap between the moment when the crime was committed and the date of the reform (the unit for this variable is 100 days);[26] P_i is a period dummy taking the value 1 if individual i's crime was committed in the 12-month period preceding the reform and 0 if it was committed earlier; 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); and X_i is a vector including all variables in the conditioning set detailed in *Appendix A.2*.[27] Because drunk-driving behavior may vary endogenously with the day of the week when the crime is committed (*e.g.* weekdays versus weekends), we also tried an alternative specification where we include day of the week fixed-effects.[28]

Our instrument, $I_i = (P_i * T_i)$, captures the differential effect of the T_i variable for crimes committed in the 12-month period preceding the day the reform entered into force, when compared to crimes committed in the 13 to 24 months before the reform. 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 moment the crime was committed and the entering into force of the reform in the 12-month period 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 we obtain by instrumenting $cust_i$, a dummy variable indicating whether individual *i* received a custodial sentence as part of their trial, by our instrument I_i using a Two-Stage-Least-Squares estimation procedure. Coefficients δ^{IV} measure the impact of receiving a custodial sentence (as opposed to a non-custodial one) on the *compliers*, the subset of defendants whose time of crime within the 12-month period preceding the

[26] 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.

[27] 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", "descendant 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 earnings (before tax and any social contributions). 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).

[28] The results are not reported here, but they are quite similar to those presented in section 5 and are available upon request.

entering into force of the reform had an impact on whether or not they received a custodial sentence – *i.e.* offenders who were sentenced to serve 1 to 60 days in prison.

Standard OLS approach

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

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

In this equation, the coefficient 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 and those who receive a non-custodial one differ significantly 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 and compliers' characteristics

In *Table 4*, we estimate the impact of having committed a drunk-driving crime closer to the signing of the law on the probability for a defendant to receive a custodial sentence (*Panel A*) and on the probability for a defendant to actually be incarcerated, as measured by our proxy (*Panel B*). In order to do so, we regress the binary variable indicative of the trial outcome 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 crimecommitted in the 12-month period preceding the entering into force 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 time gap between the day a defendant supposedly committed their crime and the moment when the law was signed is independent of their characteristics and those of their case.

Similarly, we find that 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 preceding the entering into force of the reform (*Panel B*). 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 and by the prison waiting list.

In *Appendix A.4*, we describe the characteristics of the offenders whose date of crime had an impact on whether or not they received a prison 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 very similar to those of the overall sample. This suggests that the selection described in section 3 does not affect the characteristics of the compliers.

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 further investigate the validity of this assumption, we study whether or not 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, we report the coefficient and standard error associated with the instrument 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.

[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.

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 reform allows us to mitigate the consequences of this potential problem by controlling for trend and seasonality effects.

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 it. 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 convictions, 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 problems arising due to non-monotonicity are probably limited as well.

5. Main Results

We measure the relative impact of custodial and non-custodial sentences on offenders' post-sentencing outcomes using the strategy described in the previous section. We investigate their impact on offenders' subsequent criminal behaviors and labor market attachment.

5.1. Impact on crime

Overall impact

We start by measuring the relative impact of custodial and non-custodial sentences on drunk drivers' post-sentencing involvement in criminal activities.[30] In *Figure 3*, we report on the differential effect of the two sentences as measured by our IV estimates. To do so, we compute the following two cumulative outcomes every 3 months from the date when the drunk-driving case was settled in court:

[30] 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.

a) the probability of being convicted of a crime by time *t* (the *extensive* margin); and b) the number of convictions committed by time *t* (the *intensive* margin). All subsequent convictions (for all types of crimes) are included in the calculation of these outcomes. A subset of coefficients is displayed in *Table 5*.

Although standard errors are large, several overall patterns emerge. At the extensive margin, our results suggest that custodial and non-custodial sentences are equally effective in preventing offenders from being reconvicted (*Figure 3.a*). Throughout most of the study period, point estimates are close to 0 and fail to be statistically significant at the 10% level. It is only towards the end of the period that estimates start to suggest that incarceration may become a little more effective in reducing the share of offenders who are reconvicted in the long run, as point estimates become statistically significant at the 10% level.

Turning to the intensive margin, we find that custodial sentences significantly increase the average number of convictions (*Figure 3.b*). At its peak, the magnitude of the effect is quite large: our results indicate that incarceration increases the average number of convictions by 0.623 crime after 8 years – representing a 30.3% increase at the sample mean. Because we do not find any effect on the extensive margin, the effect at the intensive margin is mechanically larger on reoffenders – individuals who were reconvicted at least once. Hence, it seems that while incarceration does not increase the number of reoffenders, it intensifies their subsequent criminal activities. It is also important to note that offenders' number of convictions following their trial is top-coded at the 99th percentile (see Appendix A.2.). Therefore, we are confident that having significant estimates at the intensive margin, but no significant effect at the extensive margin is not simply driven by extreme values.

Taking a closer look at the results, a first interesting pattern lies in the sudden drop experienced by point estimates one to two years after the trial, which suggests that incarceration is more effective in preventing criminal behaviors than probation in the very short run. The bottom is reached after 15 months, at which point the extensive margin coefficient is statistically significant at the 5% level and the intensive margin coefficient at the 10% level. We interpret this pattern as reflecting the incapacitation effect of custodial sentences and its late timing as the consequence of the waiting list system in effect at the time, which could delay offenders' incarceration up to several months after the promulgation of their sentence.

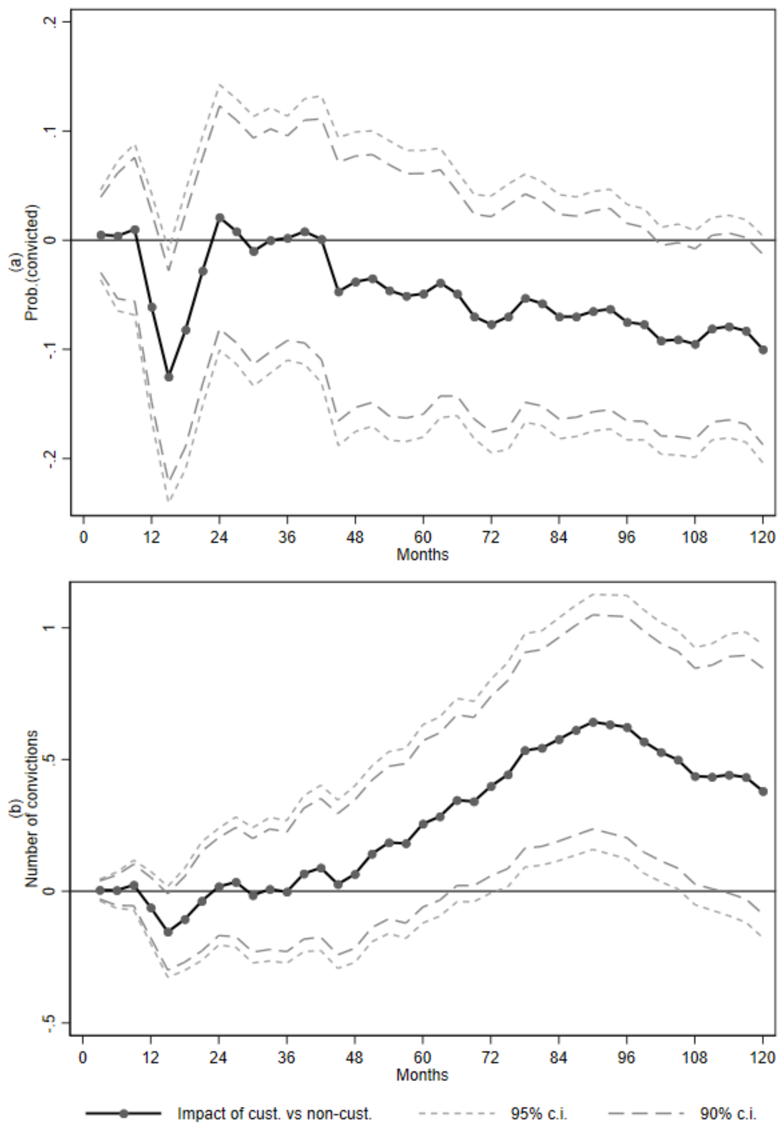

Figure 3 – Impact of custodial vs. non-custodial sentences on crimes: This figure depicts the cumulative impact of a custodial sentence (as measured by our IV estimates) on the following outcomes: a) the probability of being convicted of a crime; and b) the number of convictions. Crime outcomes are measured every 3 months from the date when the drunk-driving case was settled in court.

The timing of the effects during the rest of the study period is also quite revealing at the intensive margin. First, the impact of the two sanctions is remarkably similar during the following two years, with differences remaining small in magnitude and non-statistically significant. It is only from the 5th 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 non-custodial sentence and requirements associated with it. It is also interesting to note that conditional sentences remain on offenders' criminal record for 3 years from the date of their trial. We interpret this as a sign that non-custodial sentences represent an important disruption in individuals' lives. In particular, the stigma associated with having a criminal record (as revealed by Pager, 2003; Raphael, 2014; Agan and Starr, 2018 and Mueller-Smith and Schnepel, 2020) would disappear three years after trial for individuals placed on probation, but would last up to five years after release for those incarcerated, consistent with the patterns observed in the data. Finally, the difference in the effects of the two sanctions reaches its maximum 8 years after the decision of justice and starts diminishing from then on, implying that, relatively speaking, the negative effects of custodial sentences can dissipate. However, for most offenders this coincides with the start of the 2008 economic crisis and the rise in unemployment. This pattern will be commented further below, but these results suggest that the impact of custodial and non-custodial sentences may depend on the peculiarities of the legal sanctions, as well as on external factors.

In comparison with IV and reduced-form estimates, the standard OLS approach yields very different results. As displayed in *Table 5,* we find that OLS estimates suggest that there is a negative relationship between receiving a custodial sentence and crime, both at the extensive and intensive margins. Consistent with the deterrence theory, these coefficients suggest that custodial sentences decrease both the probability for a drink-driver to be subsequently convicted of any other crime, as well as the number of such crimes they commit. For instance, they suggest that custodial sentences decrease the probability for an offender to commit any other crime within the next 10 years by 5.0 percentage points (representing a 7.4% decrease at the sample mean) and reduces the number of such crimes they commit by 0.255 crime (representing a 10.7% decrease at the sample mean). Both results are statistically significant at the 1% level. The differences between standard OLS and IV results cast further doubt on the reliability of studies attempting to mitigate the differences across groups by controlling for observable confounding factors.

Overall, our results provide evidence that non-custodial sentences can be at least as effective as custodial ones to prevent subsequent crime. From policymakers' perspective, our results also provide supporting evidence that non-custodial sentences can be more cost-effective than custodial ones to reduce subsequent crime.

Rehabilitative or criminogenic effect?

The above results suggest that in the long run, probation leads to fewer crimes compared to incarceration. A natural question is whether this effect is driven by the positive impact of the rehabilitation program offered to some offenders placed on probation to deal with their alcohol abuse problem, or rather by a criminogenic effect of incarceration.

To answer this question, we split our overall crime outcomes between convictions for a drunk-driving crime and convictions for any other crime, and report on the relative impact of custodial and non-custodial sentences on these two types of convictions in *Figure 4*. We use the first outcome to test whether or not non-custodial sentences had any differential rehabilitative effect on offenders' alcohol abuse problem. We use the second outcome to test whether or not incarceration had a criminogenic effect. Again, we measure their relative impact on the extensive and intensive margins every 3 months from the date when the drunk-driving case was settled in court. A subset of coefficients is displayed in *Tables 6*.

We find no differential impact on convictions for drunk-driving crimes, suggesting that the increase in the number of convictions cannot be driven by the positive effect the rehabilitation program may have had on the alcohol abuse problem of offenders placed on probation. Indeed, the two sanctions appear to be equally effective in preventing offenders from being reconvicted for a drunk-driving crime (*Figure 4.a*), and their impact on the average number of convictions for drunk driving is also similar (*Figure 4.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 significant room for improvement. As displayed in *Tables 6*, the average number of reconvictions for a drunk-driving crime for individuals included in our sample is 0.6 after 10 years. To some extent, the absence of a rehabilitative effect may be explained by the fact that, as mentioned above, the rehabilitation program was only offered to a subset of offenders placed on probation (those who suffered from an alcohol abuse problem). Moreover, these offenders could already benefit from it prior to the reform (they only had to request it). As a consequence, the number of compliers who benefitted from the rehabilitation program may be too limited for its positive effect (if any) to materialize in our results.

In contrast, our results highlight the criminogenic effect of incarceration as we find that, compared to non-custodial sentences, custodial ones increase the average number of convictions for crimes other than drunk driving (*Figure 4.d*). At its peak, the magnitude of the effect is large: our results indicate that custodial sentences increase the average number of convictions by 0.630 crime 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 for a crime other than drunk driving (*Figure 4.b*), the effect on the intensive margin is again mechanically larger on offenders who were convicted at least once.

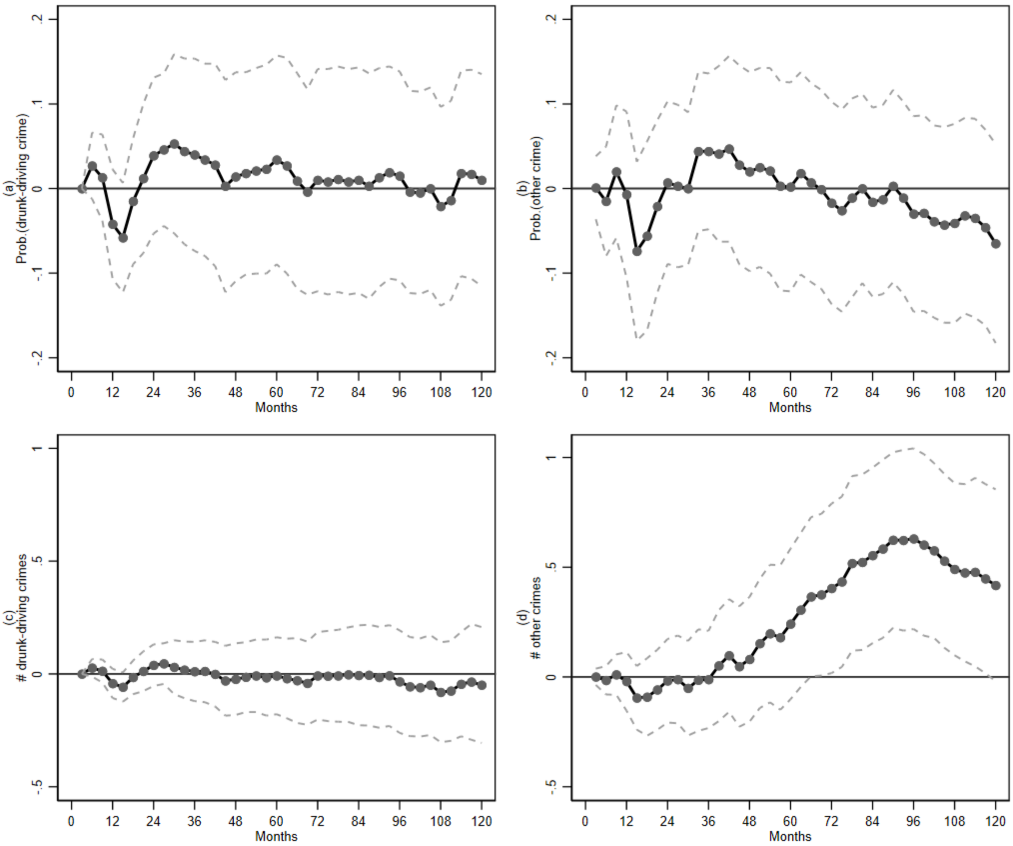

Figure 4 – Impact of custodial vs. non-custodial sentences on drunk-driving and other crimes: This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) on the following outcomes: a) the probability of being convicted of a drunk-driving crime ; b) the probability 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.

When further disaggregating the type of other crimes committed, we find that our results are driven by economically motivated crimes. In *Figure 5*, we report on the relative impact of custodial and non-custodial sentences on convictions for violent, property, and other crimes taken separately, leaving drunk-driving crimes out of this analysis. A subset of coefficients is displayed in *Tables 7*. We observe a strong and particularly significant increase in the number of convictions for property crimes. In contrast, we do not find any impact on the number of convictions for violent crimes. 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. Overall, these results suggest that incarceration may significantly weaken the labor market attachment of offenders who, to a larger extent, resort to crime to make a living – a theory we investigate further in the next section.

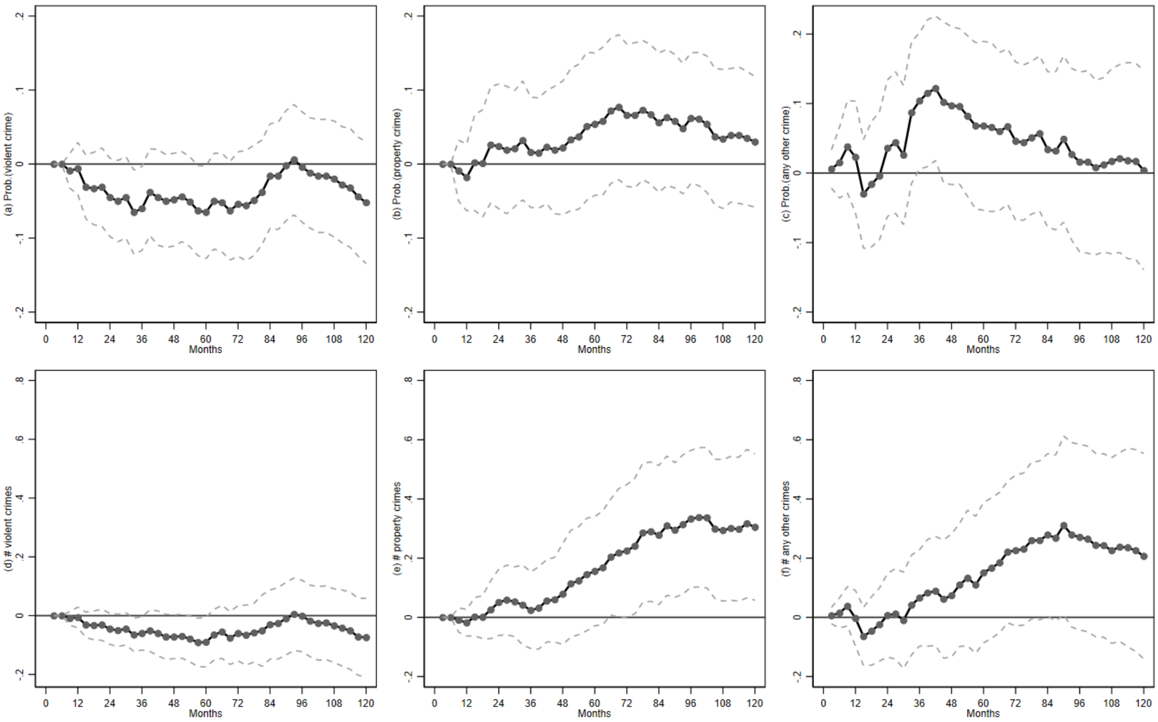

Figure 5 – Impact of custodial vs. non-custodial sentences on other crimes (decomposition by crime type): This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) on the following outcomes: a) the probability of being convicted of a crime; and b) the number of 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) property crimes; 2) violent crimes; and 3) other non-drunk-driving crimes.

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. The aim here is to understand the relative impact of these two sanctions on a key aspect of offenders' lives, as well as to shed some light on a possibly important mechanism behind the effects found on crime-related outcomes. 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*;[31] 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 6*. A subset of coefficients is also displayed in *Table 8*.

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

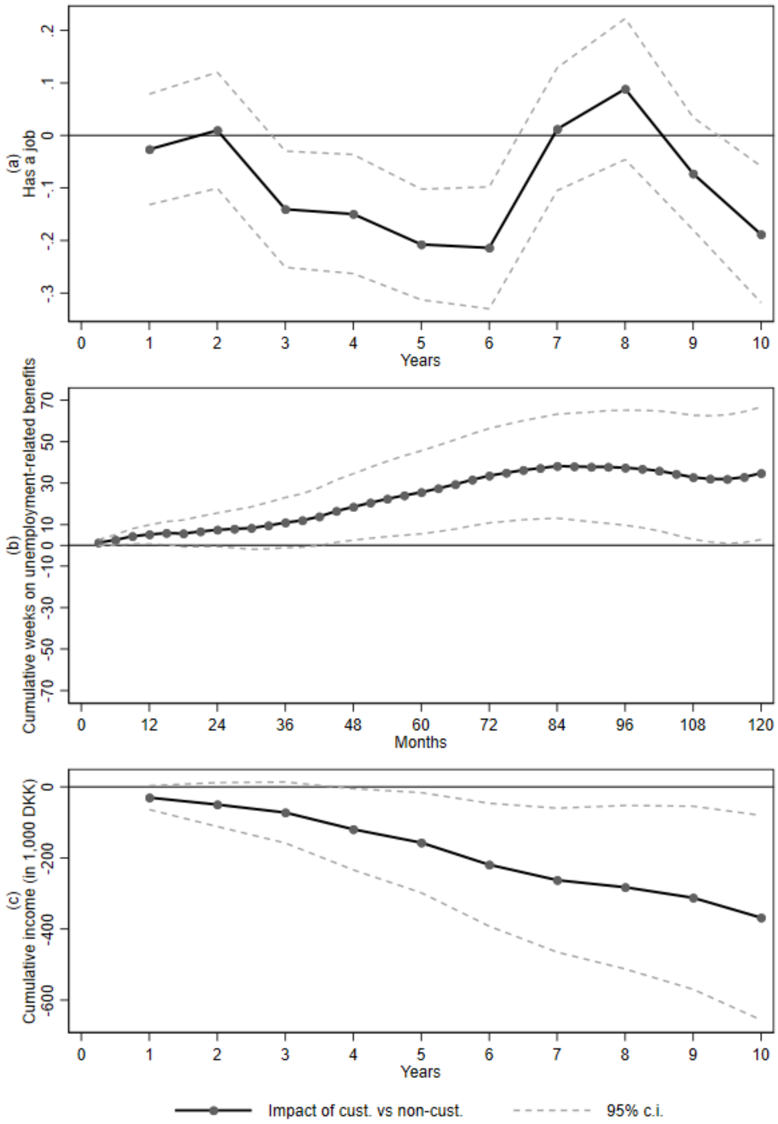

Figure 6 – Impact of custodial vs. non-custodial sentences on labor market attachment: This figure depicts the impact of custodial sentences (as measured by our IV estimates) on the following outcomes: a) the probability of having a job at the end of each year following the decision of justice; b) the 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.

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 who receive a custodial sentence are less likely to be employed after receiving their sentence than those who only receive a non-custodial sentence. As a consequence, they are more likely to rely on unemployment-related benefits. This result holds for almost every year following the decision of justice – except for a couple of years towards the end of the period. In both cases, the magnitude of the effects is very large. For instance, 10 years after the 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 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. After 10 years, incarceration represents a cumulative loss of 368,056 kroners – corresponding to a 15.2% decrease at the sample mean, or one and a half year worth of income.

Again, the timing of the effects (combined with the pattern of results discussed above) suggests that offenders' precarious post-incarceration employment situation may constitute an important mechanism for the increase in the number of subsequent crimes. While the impacts on crime and labor market outcomes follow similar patterns, the impact on labor market outcomes seems to precede the one on crime outcomes. Indeed, the negative impact of incarceration starts materializing from the 3rd year following the decision of justice, while differences in crime outcomes only start materializing from the 5th year after the decision of justice. In contrast, differences in labor market attachment between offenders who received a custodial sentence and those who received a non-custodial one remain small in the first years following the decision of justice. Again, this suggests that non-custodial sentences can constitute an important disruption in individuals' lives, which negatively affects their ties to the labor market.

It is also interesting to note that these differences between the impact of the two sanctions on offenders' labor market attachment seem to decrease towards the end of the period (around 7 years after the decision of justice), before worsening again at the end of the study period. Although more pronounced, this pattern echoes the one observed on crime outcomes. While this remains puzzling, it is interesting to note that, for most offenders in the sample, the 7th year after the decision of justice corresponds to 2008, which was marked by an economic crisis and the beginning of a sharp rise in the unemployment rate. In turn, these observations seem to suggest that the relative impact of custodial and non-custodial sentences can be influenced by societal factors, including the state of the economy.

5.3. Heterogeneity

Finally, we examine whether or not the effects of incarceration on crime vary across subgroups of offenders. More specifically, we compare the relative effect of custodial and non-custodial sentences for: a) offenders of different age groups; b) offenders who were employed at the end of the year preceding their crime and those who were not. In *Table 9*, we report on the relative impact of the two sanctions on offenders' overall crime outcomes 10 years after their drunk-driving case was settled in court, as well as on offenders' cumulative income 10 years after their drunk-driving case was settled in court.

First, we find that the effect of incarceration captured above is primarily driven by young offenders (below 30) and, in particular, those who were employed at the end of the year preceding their crime. Within this group, although custodial and non-custodial sentences do not have any differential impact on offenders' probability of committing a crime, custodial sentences increase the average number of crimes they commit by 2.4 10 years after their drunk-driving case was settled in court – representing a 75.5% increase at the sample mean. Cumulative income also decreases by 1,142,270 DKK – representing a 40.2% decrease at the sample mean.

Second, the negative impact of custodial sentences relative to non-custodial ones is less pronounced on older offenders. In particular, our evidence suggests that custodial sentences may actually decrease the probability of committing another crime for older offenders. Effects are also more limited on offenders who were already struggling on the labor market. Finally, we do not find important differences across offenders based on whether or not they had a job at the end of the year preceding their crime.

It is interesting to note that the effects are the most pronounced for younger offenders who were incarcerated at a time when they should have been strengthening their professional network while accumulating experience. Presumably, these individuals were those who had the most to lose from a period of incarceration. This further reinforces the idea that the weakening of offenders' labor market attachment following a period of incarceration can push them to commit more crimes.

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 were 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 and that, for instance, wealthier defendants were more likely to have their trial put off until after the reform. From a policy perspective, this important result suggests that 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 show that, in the context of the reform under study, this increase was not driven by drunk-driving crimes but by other crimes and, in particular, property crimes. Part of the explanation for this increase in offenders' criminal activities can be found in their greater precariousness. Indeed, we also 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. Moreover, the negative effects on offenders' labor market attachment seem to precede the rise in their criminal activities. These results are primarily driven by young offenders and, in particular, those who had a job at the end of the year preceding their crime and had the most to lose from being incarcerated.

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 in Danish open prisons. In this respect, our paper contributes to understanding the role of incarceration conditions on post release outcomes, and complements 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 be found in differences in methodologies and compliers' characteristics. While we focus on a broad range of mild offenders arrested for a single (yet important) 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.

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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

Variables	Whole sample #Obs.	Mean	S.d.	Instrument 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: Relative impact of incarceration on all types of subsequent crimes

	Whole sample			OLS			RF			IV					
				(1)			(2)			(3)			(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															
Within 2 years	8,353	0.341	0.474	-0.037	0.013	***	-0.003	0.009		0.026	0.064		0.019	0.062	
Within 4 years	8,353	0.511	0.500	-0.058	0.012	***	0.006	0.010		-0.033	0.072		-0.038	0.070	
Within 6 years	8,353	0.598	0.490	-0.057	0.011	***	0.011	0.009		-0.070	0.064		-0.077	0.060	
Within 8 years	8,353	0.647	0.478	-0.053	0.010	***	0.011	0.008		-0.068	0.059		-0.075	0.055	
Within 10 years	8,353	0.680	0.467	-0.050	0.009	***	0.015	0.008	*	-0.094	0.058		-0.100	0.053	*
Panel B: Number of convictions															
Within 2 years	8,353	0.523	0.875	-0.070	0.023	***	-0.003	0.017		0.044	0.123		0.020	0.113	
Within 4 years	8,353	1.095	1.525	-0.151	0.036	***	-0.009	0.025		0.096	0.196		0.065	0.171	
Within 6 years	8,353	1.632	2.148	-0.189	0.050	***	-0.057	0.031	*	0.439	0.263	*	0.393	0.207	*
Within 8 years	8,353	2.053	2.642	-0.205	0.057	***	-0.091	0.038	**	0.665	0.325	**	0.623	0.255	**
Within 10 years	8,353	2.384	3.017	-0.255	0.067	***	-0.055	0.042		0.442	0.364		0.378	0.284	
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 1); 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 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 1); 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.B: Impact on other crimes

	Whole sample			OLS		RF		IV			
				(1)		(2)		(3)		(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 1); 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 7.A: Impact on other crimes, decomposition (extensive margin)

	Whole sample			OLS			RF			IV					
				(1)			(2)			(3)			(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 2); Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 3); 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.B: Impact on other crimes, decomposition (intensive margin)

	Whole sample			OLS		RF		IV			
				(1)		(2)		(3)		(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 2); Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 3); 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 8: 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, I 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 2); 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 3); finally, I 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. Standard errors are clustered at the district court and individual levels. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 9.a: Heterogeneity, impact on crime outcomes

			Any crime at 10 years					
			Extensive Margin			*Intensive Margin*		
		N	Mean & sd	OLS	IV	Mean & sd	OLS	IV
Panel A: Impact on crime by age or prior employment status								
Age	≤ 30	2211	0.806	-0.055 ***	0.086	3.674	-0.245	1.883 **
			0.396	*0.021*	*0.116*	*3.742*	*0.160*	*0.907*
	> 30 & ≤ 40	2776	0.748	-0.044 **	-0.192 **	2.667	-0.215 *	0.007
			0.434	*0.018*	*0.097*	*3.015*	*0.123*	*0.569*
	> 40	3366	0.540	-0.065 ***	-0.126	1.303	-0.241 ***	-0.237
			0.498	*0.018*	*0.082*	*1.892*	*0.080*	*0.326*
Employ. status	Not employed	3030	0.703	-0.012	-0.124	2.817	-0.201	0.111
			0.457	*0.017*	*0.104*	*3.428*	*0.138*	*0.605*
	Employed	5323	0.666	-0.066 ***	-0.083	2.137	-0.269 ***	0.538
			0.472	*0.013*	*0.073*	*2.726*	*0.081*	*0.384*
Panel B: Impact on crime by age and prior employment status								
Not employed	≤ 30	680	0.865	0.027	-0.145	4.775	-0.048	-0.215
			0.342	*0.034*	*0.220*	*4.243*	*0.364*	*2.096*
	> 30 & ≤ 40	999	0.796	0.018	0.093	3.385	-0.005	0.620
			0.403	*0.034*	*0.203*	*3.473*	*0.268*	*1.298*
	> 40	1351	0.553	-0.053 *	-0.241 *	1.412	-0.185	-0.699
			0.497	*0.028*	*0.140*	*2.060*	*0.139*	*0.545*
Employed	≤ 30	1531	0.779	-0.082 ***	0.231	3.184	-0.228	2.404 **
			0.415	*0.026*	*0.164*	*3.385*	*0.206*	*1.174*
	> 30 & ≤ 40	1777	0.721	-0.069 ***	-0.323 **	2.263	-0.201	-0.330
			0.448	*0.022*	*0.147*	*2.641*	*0.141*	*0.695*
	> 40	2015	0.532	-0.057 **	-0.087	1.231	-0.250 ***	-0.025
			0.499	*0.024*	*0.097*	*1.766*	*0.083*	*0.414*

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 1); 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. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 9.b: Heterogeneity, impact on labor outcomes

			At 10 years					
			Has a job			*Cumulative income*		
	Subgroups	N	Mean & sd	OLS	IV	Mean & sd	OLS	IV
Panel A: Impact on crime by age or prior employment status								
Age	≤ 30	2211	0.51	-0.01	-0.47 ***	2563.96	-65.63	-950.65 ***
			0.50	*0.02*	*0.17*	*1224.39*	*46.43*	*341.13*
	> 30 & ≤ 40	2774	0.37	-0.01	-0.12	2471.41	-28.83	-142.77
			0.48	*0.02*	*0.11*	*1288.26*	*51.43*	*223.42*
	> 40	3366	0.22	-0.02	-0.11	2276.97	-71.54	-284.36
			0.42	*0.01*	*0.08*	*1398.47*	*48.64*	*227.52*
Employ. status	Not employed	3029	0.17	-0.01	-0.17 **	1788.91	-42.20	-284.41 *
			0.38	*0.02*	*0.08*	*909.43*	*40.07*	*163.65*
	Employed	5322	0.45	-0.02	-0.20 **	2775.40	-78.39 **	-386.06 **
			0.50	*0.01*	*0.09*	*1386.93*	*35.23*	*194.74*
Panel B: Impact on crime by age and prior employment status								
Not employed	≤ 30	680	0.31	0.05	0.01	1933.76	-8.77	-488.63
			0.46	*0.05*	*0.31*	*1005.36*	*94.98*	*567.15*
	> 30 & ≤ 40	998	0.18	0.01	-0.11	1813.38	28.72	95.88
			0.39	*0.03*	*0.19*	*805.03*	*71.06*	*298.65*
	> 40	1351	0.09	-0.03	-0.18 **	1697.91	-71.75	-431.86
			0.28	*0.02*	*0.08*	*921.36*	*56.89*	*276.78*
Employed	≤ 30	1531	0.60	-0.03	-0.60 ***	2843.87	-122.31 *	-1142.27 **
			0.49	*0.03*	*0.22*	*1209.11*	*67.57*	*447.22*
	> 30 & ≤ 40	1776	0.47	-0.01	-0.11	2841.34	-7.24	-106.77
			0.50	*0.03*	*0.14*	*1359.58*	*66.15*	*318.01*
	> 40	2015	0.31	-0.01	-0.02	2665.21	-107.91	-168.92
			0.46	*0.02*	*0.11*	*1524.10*	*72.52*	*301.53*

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 1); 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. 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 “a strong need for rehabilitation” who were sentenced to no more than 40 days could benefit from a *pardon scheme.* In this case, their unconditional prison 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 an unconditional prison 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[32]

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).[33] 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, conditional and unconditional prison sentence length, etc.).

Unfortunately, whether or not an offender was actually incarcerated and the length of their incarceration spell is only imprecisely observed. That is why we focus on whether or not an individual *received* a custodial sentence in what follows – and not on whether or not they were actually

[32] 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).

[33] 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).

incarcerated. However, we construct a proxy for whether or not an individual was actually incarcerated and obtain results similar to those displayed below when using it as our variable of interest.[34,35] Finally, drivers' blood alcohol content at the time of their arrest is not available in the datasets.

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. Then, in order to investigate any criminogenic effect of incarceration, 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 earnings before tax and any social contributions (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.

[34] Our proxy for whether or not an individual was incarcerated captures whether an individual spent at least 10 days in prison – 10 days being the minimum duration of prison sentences requested for a drunk-driving crime.

[35] The only difference is that, logically enough, the results are larger in magnitude. Results are available upon request.

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, other road traffic crimes, and non-road traffic crimes), marital status, highest educational achievement, type of job held, and annual earnings (before tax and any social contributions).

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

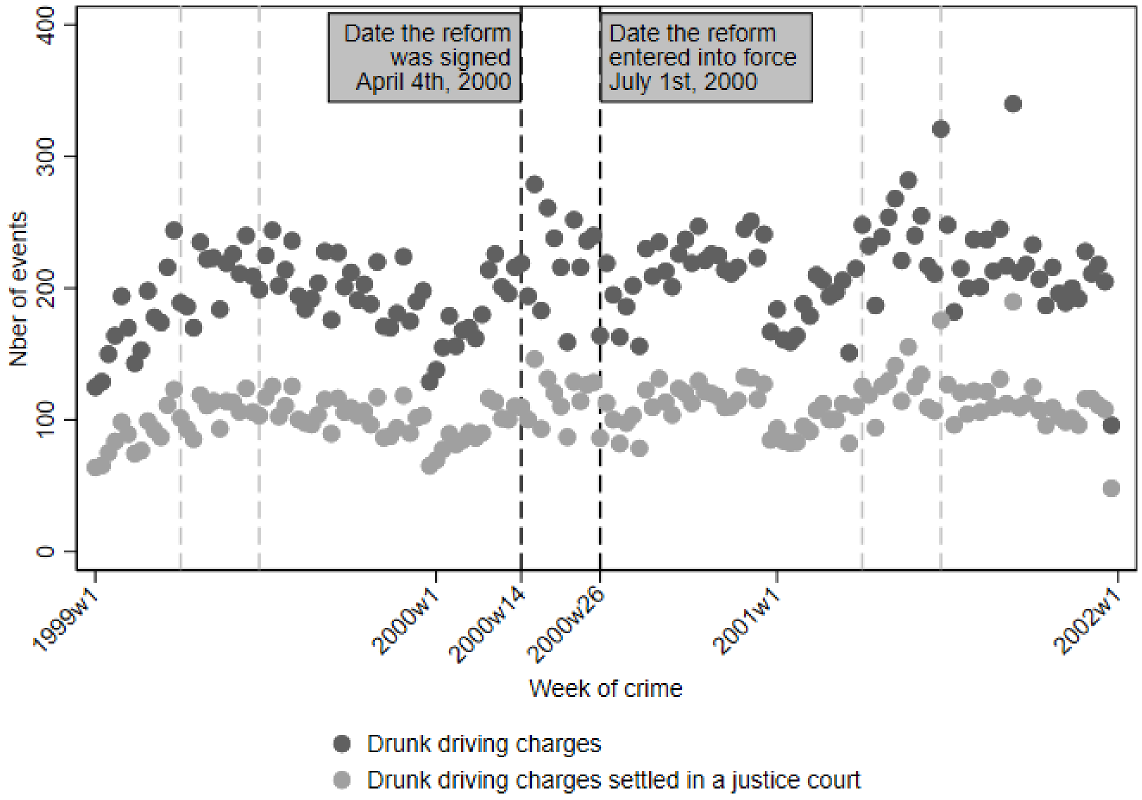
Variables	Description *N.B.: Names in italics refer to variables made available by Statistics Denmark. More information can be found on each of these variables on their website: https://www.dst.dk*
Offender's number of drunk-driving convictions in the 5 years preceding the crime	Computed from the date of crime. Top-coded at the 99th percentile. Variable source: we calculated this information myself 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 road traffic 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 myself using the exhaustiveness of the registers. Information on the date of the crime was retrieved from the *SIG_GER1DTO* variable.
Offender's number of non-road traffic convictions prior to the 5-year period preceding the crime	Computed from the date of crime. Top-coded at the 99th percentile. Variable source: we calculated this information myself 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** *<u>N.B.:</u> 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 99^{th} percentile. Variable source: we calculated this information myself 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 99^{th} 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, I 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 my 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: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.