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**Custodial *versus* non-custodial sentences:  
Long-run evidence from an anticipated reform**

**Bastien Michel\***  
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**JEL Codes: K14, K42, J24**

**Keywords: Crime, Employment, Incarceration, Recidivism.**



# Custodial *versus* non-custodial sentences: Long-run evidence from an anticipated reform

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## Abstract

We study the relative impact of custodial and non-custodial sentences on later crime and labor-market outcomes in Denmark, a country where detention conditions are particularly good. To do so, we take advantage of a large-scale reform of the Danish legislation implemented in 2000, whereby incarceration was replaced by a non-custodial sentence for most drunk-driving crimes, which represented a quarter of the custodial sentences inflicted prior to the reform. Our first key finding is that stakeholders anticipated the consequences of the reform: around the time of the reform, the number of cases tried dropped and the nature of the cases changed significantly. To measure the relative impact of incarceration, we therefore resort to a novel instrumental variable approach exploiting quasi-exogenous variation in the probability of being tried after the reform, and therefore incarcerated, based on the crime date. We find that incarcerated offenders commit more crimes and have weaker ties to the labor market after release. The pattern of results suggests that part of the explanation for this increase in offenders' criminal activities can be found in their greater precariousness.

**JEL Codes:** K14, K42, J24

**Keywords:** Crime, Employment, Incarceration, Recidivism

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## 1. Introduction

Although the world's prison population has grown steadily over the past four decades to reach more than 10.8 million people (Fair and Walmsley, 2021), the effectiveness of incarceration is increasingly being questioned. In particular, although one of its main objectives is to deter criminals from reoffending (the so-called *specific* deterrent effect), available evidence suggests that custodial sentences are not more effective in preventing recidivism than non-custodial sentences such as probation or electronic monitoring (see Villettaz et al. (2015), Chalfin and McCrary (2017), and Loeffler and Nagin (2022) for reviews). However, most of the existing literature is based on the US and robust evidence remains very limited in settings where prison conditions are better.<sup>1</sup> In fact, recent studies from Finland and Norway, two countries with some of the most favorable prison conditions in the world, come to more mixed conclusions (Bhuller et al., 2020; Huttunen et al., 2020), suggesting that improving incarceration conditions can make custodial sentences more effective overall than non-custodial ones in combating recidivism.<sup>2</sup>

Our study contributes to this literature by providing robust evidence on the relative impact of custodial and non-custodial sentences in Denmark, another Scandinavian country with particularly good incarceration conditions (Lappi-Seppälä, 2007; Pratt, 2008; Pratt and Eriksson, 2011; Ward et al., 2013). To do so, we take advantage of a large-scale reform of the Danish legislation implemented in 2000, whereby incarceration was replaced by a probation period for drunk-driving crimes. The probation period was combined with a fine and community service or mandatory participation in a rehabilitation program. Drunk driving is a very common crime and has a high cost for society:<sup>3</sup> in countries where it is liable to imprisonment, it is often one of the most frequent charges resulting in custodial sentences. In Denmark, for example, it accounted for a quarter of all custodial sentences passed in 1999. Interestingly, note that while one may be concerned by the fact that drunk-drivers

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

<sup>2</sup> However, both papers show heterogeneity in the results. In Bhuller et al (2020), the specific deterrent effect of incarceration is driven by individuals who were not working prior to incarceration and whose employment and earnings also increase after their release. For previously employed individuals, there is no effect on recidivism but a lasting negative effect on employment. In Huttunen et al. (2020), the specific deterrent effect of incarceration is more significant for individuals who commit more serious crimes than for those who commit less serious ones).

<sup>3</sup> Drunk driving accounts for more than one million arrests each year in the US, and for more than 273,000 deaths every year throughout the world (Vissers, 2017). In the EU and the US, alcohol is estimated to have caused 25 to 30% of all road fatalities in 2015 (European Commission, 2015; National Center for Statistics and Analysis, 2017) – representing around 6,400 and 10,265 fatalities respectively. Drunk driving therefore 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).

may constitute a specific type of offenders, particularly prone to alcohol problems, our framework actually allows us to capture the impact of incarceration on offenders who do not have a serious alcohol abuse problem.

An analysis of how the reform was implemented leads to the first key conclusion, namely that stakeholders (defendants and courts of justice) anticipated the reform and modified their behavior in the weeks preceding its entering into force. In practice, these anticipations materialized through a sharp drop in the number of cases tried from the moment the law was signed (but before it actually entered into force), in line with stakeholders' incentives to postpone drunk-driving cases until after the reform: defendants to avoid incarceration and courts of justice to reduce the number of cases that might have to be retried. Importantly, we show that individuals who had their case postponed until after the reform were not selected at random, but had rather specific characteristics. In particular, wealthier defendants were more likely to have their trial delayed, thus avoiding prison. As a consequence, significant selection occurred in the nature of the offenders tried before and after the reform and our identification strategy cannot rely on the comparison of these two groups.<sup>4</sup>

To study the relative effects of custodial and non-custodial sentences in the context of this reform, we instead propose a novel instrumental variable approach which consists in comparing offenders arrested at different dates before the reform was signed. Indeed, we show that the closer to the reform a crime was committed, the more likely a defendant was to be tried after the reform under the new law and placed on probation instead of being incarcerated. This variation in the probability of being incarcerated stems from the combination of two general features of Danish legislation. First, the case processing time in courts of justice was significant at the time of the reform (6 months on average). Thus, variation in the date of the crime generated variation in the probability of offenders being tried after the reform: the closer to the reform the crime was committed, the higher this probability was. Second, Danish legislation guarantees that individuals tried after a reform for a crime committed prior to it must be tried under the most lenient of the two laws. Hence, defendants tried after the reform benefitted from the new law and were therefore placed on probation. As a result, people arrested for drunk driving three months before the date of the reform were more likely to be tried after the reform and avoid incarceration than people arrested for a similar offense six months earlier. Crucially, we show that this source of variation in offenders' probability of incarceration is plausibly exogenous.

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<sup>4</sup> In contrast, previous studies of this reform (Andersen, 2015; Wildeman and Andersen, 2017) have relied on comparisons between offenders tried before and after the reform, which calls for a cautious interpretation of their results.

Our results show that, relative to non-custodial sentences, custodial ones significantly increase offenders' involvement in subsequent criminal activities. While we do not find any impact on the number of subsequent drunk-driving crimes, incarceration significantly increases the average number of other crimes. At its peak, eight years after the trial, incarceration increases the average number of other convictions by 1.4 (statistically significant at the 1% level) – representing an 89.8% increase at the sample mean. However, we do not observe any significant effect on the probability of an offender committing another crime after being released, whether it be a drunk-driving crime or any other crime. Hence, although incarceration does not increase the number of reoffenders relative to probation, it intensifies reoffenders' subsequent criminal activities. Investigating further, we find that the increase in the number of convictions is driven by a rise in economically motivated crimes.

Consistent with these findings, we observe that custodial sentences increase the difficulties faced by offenders in the labor market as compared to non-custodial ones. Indeed, our analysis of offenders' post-sentencing labor market attachment indicates that incarceration significantly reduces offenders' probability of having a job, increases their reliance on unemployment-related benefits, and, ultimately, reduces their income. After ten years, incarceration represents a cumulative loss of 599,874 kroner (statistically significant at the 5% level) – which is sizeable as it corresponds to a 45.9% decrease at the sample mean. A close examination of the timing of the effects on labor market outcomes provides suggestive evidence consistent with the idea that individuals with a criminal record are subject to stigma that hinders their employment opportunities after release – in line with evidence found in Pager (2003), Raphael (2014), Agan and Starr (2017, 2018), Mueller-Smith and Schnepel (2021), and Rose (2021). Overall, our results suggest that labor market difficulties are likely to explain some of the observed increase in criminal activity.

Our findings contribute to the literature studying the impact of incarceration (see Villettaz et al. (2015), Chalfin and McCrary (2017), and Loeffler and Nagin (2022) for reviews).<sup>5</sup> In particular, it complements existing studies on Scandinavian countries (Dobbie et al., 2018; Bhuller et al., 2020; Huttunen et al., 2020)<sup>6</sup> which, by focusing on the subset of offenders on the margin of being

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

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

incarcerated,<sup>7</sup> have so far tended to suggest that custodial sentences may be more effective overall to reduce recidivism than non-custodial ones in countries with good detention conditions. We actually draw more mixed conclusions based on a large subset of defendants in Denmark, namely drunk drivers, for whom the outcome of the trial depends much less on the severity of the judge than on the law and result of the blood alcohol test at the time of arrest. The fact that comparable results can be found in Denmark, where incarceration conditions are as good as in other Scandinavian countries, and in the United States, where prison conditions are much worse, prompts caution about the link between prison conditions and the relative effectiveness of custodial and non-custodial sentences suggested so far by cross-country comparisons. To some extent, these results echo the literature on the impact of prison conditions in the United States, which has found mixed results to date (Katz et al., 2003; Bedard and Helland, 2004; Chen and Shapiro, 2007).

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

The rest of the article 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.

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<sup>7</sup> All three studies use random assignment of criminal cases to judges of different severity levels to measure the causal impact of incarceration. In this setting, its impact is identified on defendants at the margin of incarceration, *i.e.* defendants for whom the judge's severity has an impact on whether or not they are incarcerated.

## 2. The legislative change

### 2.1. Context prior to the reform

In the last quarter of the 20<sup>th</sup> century, legislations on drunk-driving crimes were gradually hardened throughout the world in an attempt to reduce the number of road fatalities. As a result, drunk driving has become a very common crime<sup>8</sup> and, in some countries, a major source of custodial sentences.

In Denmark, individuals arrested for drunk driving have been facing a prison sentence since the establishment of a first administrative blood alcohol threshold in 1976, lowered from 0.8 to 0.5g/L in 1998. These changes led to a sharp increase in the number of custodial sentences passed for drunk driving: in 1999, drunk driving was the main reason for receiving a custodial sentence, accounting for 24.8% of all custodial sentences. As displayed in *Table 1*, 71.9% of defendants tried for a drunk-driving crime during that year received a custodial sentence.<sup>9</sup> Although the severity of the sentence varied depending on the characteristics of the offense (*e.g.* driver's level of impairment and existence of aggravating circumstances) and the number of prior drunk-driving convictions, the length of the prison sentence was relatively short: in 95.3% of cases, it remained below 60 days. Moreover, while fines and suspensions of the driving license were also frequently imposed on defendants – in 27.8% and 30.8% of trials respectively –, probation and community service were rarely used. Finally, it should be noted that because drivers are charged with impaired driving based on the results of blood tests that are provided to the court, only 1.2% of those charged were acquitted.

For what follows, it is important to note that offenders suffering from an alcohol abuse problem who received a prison sentence of no more than 60 days could then ask to benefit from a *pardon scheme* described in *Appendix A.1*. As part of this scheme, their custodial sentence could be commuted to a non-custodial sentence, including a two-year probation period and mandatory participation in a yearlong rehabilitation program. This explains why the share of offenders who were actually incarcerated (37.8%) is lower than the share of those who received a custodial sentence (71.9%).<sup>10</sup>

Offenders who served their prison sentences benefited from particularly advantageous conditions of incarceration. These stem from the authorities' wish to ensure that life in prison mirrors life on the

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<sup>8</sup> For instance, more than 1 million drivers were arrested for driving under the influence of alcohol or narcotics in the US in 2019 as per the Federal Bureau of Investigation's Uniform Crime Reporting Program.

<sup>9</sup> In Denmark, all individuals facing a prison sentence are tried in a court of justice.

<sup>10</sup> Our variable indicating whether or not an individual was incarcerated is a dummy variable capturing whether an individual has spent at least 10 days in prison – 10 days being the minimum duration of prison sentences requested for a drunk-driving crime.



outside as much as possible, in order to prepare prisoners for their release and facilitate their reintegration into society.<sup>11</sup> To avoid harmful interactions, drunk drivers (as well as other minor offenders) were kept separate from more serious criminals.

## 2.2. Details of the new law

In 2000, a reform was passed introducing cheaper, more lenient sentences against drunk drivers. As part of it, custodial sentences of no more than 60 days were replaced by a two-year probation period and a fine, combined with either community service or participation in a yearlong rehabilitation program (identical in every way to the one offered as part of the pardon scheme mentioned above).<sup>12</sup> The reform systematically applied to all offenders except those who had already been placed on probation for a drunk-driving crime more than once or who were on probation at the time of the offense for an alcohol-related crime. The reform reduced the burden for taxpayers, as the average cost of a custodial sentence was DKK 15,800 per offender convicted of drunk driving, against DKK 8,300 per offender for the alternative non-custodial sentence studied here (Nielsen and Kyvsgaard, 2007).<sup>13</sup>

The choice between community service or participation in a rehabilitation program was left to the judge based on whether or not the offender suffered from an alcohol abuse problem, the rehabilitation program being reserved for offenders exhibiting such a problem.<sup>14</sup> As part of this program, offenders had to take a drug causing acute sensitivity to ethanol and to participate in an alcohol treatment program.<sup>15</sup> Offenders were monitored throughout the duration of the treatment and the rest of the probation period.<sup>16</sup> Probation officers were in charge of ensuring that the terms of the probation were being respected and, in particular, of controlling offenders' drug intake and participation in the alcohol treatment program during the first phase of the scheme. Community service was to be requested against offenders who did not exhibit such an alcohol abuse problem and was substituted

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<sup>11</sup> This so-called *normalization* principle was officially introduced in the early 1970s.

<sup>12</sup> Generally speaking, offenders placed on probation see their prison sentence suspended on the condition that they do not reoffend and that they comply with any conditions that may be imposed. In case of mild violation(s) of the probation terms, the Prison and Probation Service decides whether or not to enforce the custodial sentence. In case of more serious violation(s), judges are responsible for making the most appropriate decision.

<sup>13</sup> The cost of a non-custodial sentence includes the costs associated with offender supervision and the rehabilitation program.

<sup>14</sup> 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.

<sup>15</sup> In practice, this program could take a variety of forms (ranging from group sessions at a clinic to individual meetings with general practitioners) and could vary in intensity depending on the individual's location, needs, and motivation (Nielsen and Kyvsgaard, 2007).

<sup>16</sup> During the first two months of the two-year program, offenders would usually meet with their probation officer every 2 weeks, and only once a month thereafter – unless arguments in favor of a more intensive monitoring prevailed.

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

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

As documented in the next section, the reform was perceived by offenders as a softening of the legislation. 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. Importantly, probation sentences remain on an individual's criminal record for three years from the date of conviction, while custodial sentences remain on the criminal record for five years from the date of release from prison. This can be a significant disadvantage in trying to reintegrate into society, especially because Danish employers have the right to ask for an extract from the criminal record during the hiring process.

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

### 3. Implementation of the reform: anticipation and selection

In order to determine how to measure the impact of the reform, we start by analyzing how it was implemented.

#### 3.1. Anticipation

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

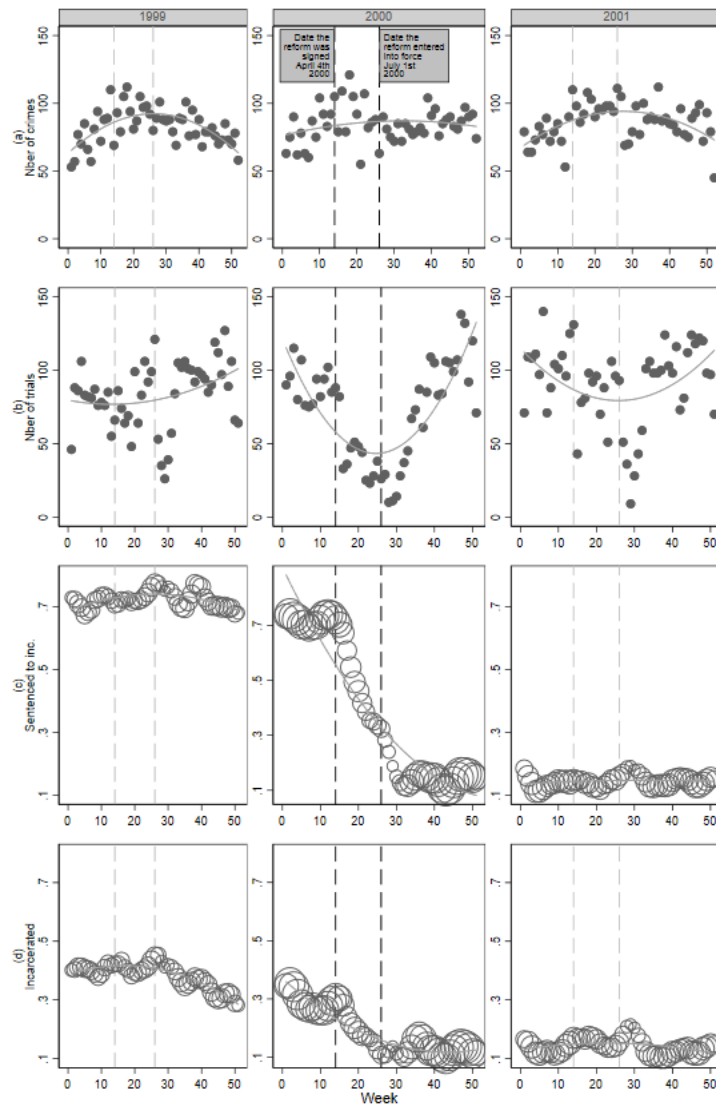
To do so, we use administrative data containing information on the universe of drunk-driving crimes committed or tried around the time of the reform to describe how the reform was implemented. *Figure 1* shows the evolution of the following four indicators between 1999 and 2001: a) the number of alleged drunk-driving crimes resulting in a trial committed every week; b) the number of drunk-driving cases tried every week in district courts; c) the share of defendants tried for drunk driving who received a custodial sentence by week of trial; d) the share of defendants tried for drunk driving who were actually incarcerated by week of trial. For each year, two dotted vertical lines are drawn to mark week 14 (the week when the law was signed in 2000) and week 26 (the week when it entered into force in 2000). The only reform implemented during these three years is the one studied in this article, which occurred in 2000. For the years 1999 and 2001, vertical lines are only drawn for comparison purposes.<sup>17</sup>

Strikingly, the evolution of these indicators reveals that the way in which drunk-driving cases were handled in district courts changed drastically in the three-month transition period following the signature of the reform and preceding its entering into force. Indeed, the number of cases tried each week dropped significantly from 91.7 cases on average in the three weeks preceding the signing of the law to 28.0 cases on average during the transition period – representing a 69.5% decrease (*Figure 1.b*).<sup>18</sup> This is the case despite the fact that there was no similar variation in the number of alleged

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<sup>17</sup> For data confidentiality reasons, indicators c) and d) displayed in *Figure 1* are calculated as moving averages. For each week  $w$ , the value of these indicators is calculated as the average value of the indicators over weeks  $w-1$ ,  $w$ , and  $w+1$ . Furthermore, for any given week, the number of individuals who received a prison sentence and the number of individuals who were incarcerated are normalized to 0 if equal to or lower than 3. These manipulations are not driving the trends observed in the data.

<sup>18</sup> The number of cases tried in the week following July 1<sup>st</sup> 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.



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

crimes resulting in a trial committed in the preceding months (*Figure 1.a*) or in the number of cases tried during the same period in adjacent years, 1999 and 2001 (*Figure 1.b*).<sup>19</sup> This suggests that stakeholders (courts of justice and/or defendants) anticipated the change in legislation and that, as a

<sup>19</sup> 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 then progressively returned to its pre-reform level.

consequence, a large share of trials were postponed until after the reform. Essentially, this means that a group of offenders who should have been tried before the reform was tried after.<sup>20</sup>

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. Interestingly, this time, the decline did not take the form of a sharp discontinuity but rather of a linear decrease, suggesting that judges gradually began to implement the reform before it came into effect.<sup>21</sup> This also suggests the existence of inequalities between defendants based on their trial date, since the likelihood of receiving a custodial sentence decreases progressively as the trial date gets closer to the date the reform entered into force. Overall, the share of defendants who received a custodial sentence decreased gradually from around 73.6% on average in the three weeks preceding the signing of the law to 34.5% on average in the three weeks preceding the date of the reform – representing a 53.1% drop. The evolution of the share of offenders actually incarcerated exhibits a similar pattern, and this despite the fact that the Danish Parliament had adopted a special regime to limit the number of offenders who would begin serving prison sentences for drunk driving during the transition period.<sup>22</sup>

While it is not possible to pin down the exact underlying mechanisms at play here, a closer look at the stakeholders' incentives suggests that both defendants and judges had good reasons for wanting drunk-driving cases to be postponed until after the reform: the former to avoid prison, the latter to reduce the number of cases which might have to be retried. Indeed, a feature of Danish legislation guarantees that defendants tried after a reform for a crime committed prior to it must be tried under the more lenient of the two laws. This means that individuals tried for a crime committed prior to the reform at hand faced the risk of being incarcerated if tried before the reform, while they merely faced

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<sup>20</sup> 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 percent 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), namely 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 percent of what would have been the total number of drunk-driving cases tried during that period.

<sup>21</sup> If the decrease in the share of defendants sentenced to incarceration simply reflected a compositional effect related to the decrease in the number of cases observed in *Figure 1.b*, then the decrease would have been a sharp decline similar to that shown in *Figure 1.b* rather than the linear decrease actually observed.

<sup>22</sup> With this special regime, the parliament hoped to avoid unequal treatment of offenders. However, *Figure 1* clearly shows that some offenders were incarcerated during the transition period for a drunk-driving crime while others were not. More information on this special regime can be found here (in Danish): <https://www.retsinformation.dk/eli/mt/2000/133> (accessed in June 2022).

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

the risk of being placed on probation if tried after. The same feature also guarantees that defendants tried prior to the passing of a law lowering the sanction for the crime they were convicted of may request that their case be retried if they are still in prison when the reform enters into force.<sup>23</sup>

### 3.2. Selection

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

To do so, 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 1, 1999 and December 31, 2000, we regress different variables indicative of their criminal priors and labor market attachment ( $y_i$ ) on a constant, a year dummy indicating whether a case was tried in 2000 ( $Y_i^{2000}$ ), quarter fixed effects ( $Q_i^j$ ), the interactions between the year dummy and the quarter fixed effects, and a time trend ( $T_i$ ):

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

In this equation, the coefficient  $\nu_1$  associated with  $Y_i^{2000}$  captures differences in the characteristics of the defendants tried in the first quarter of 1999 and 2000, while the coefficients associated with the three interaction terms,  $\theta_j$ , capture differential changes in the characteristics of the defendants tried in the first quarter and those tried in the second, third, and fourth quarters respectively. Because the transition period corresponds exactly to the second quarter,  $\theta_2$  captures differential changes occurring during the transition period (again, starting the week when the reform was signed and ending the week when it entered into force). Standard errors are clustered at the district court and individual levels.

The corresponding estimates are reported in *Table 2*. In *Panel A*, our sample includes all defendants tried during the period. In *Panel B*, we exclude from the sample those who did not receive a prison sentence, either conditional or unconditional, and, as such, were not affected by the reform. This

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<sup>23</sup> The two mechanisms may have reinforced each other. Firstly, judges may have been more willing to accede to defendants' requests to postpone their trial during the transition period. Secondly, by internalizing the judges' incentives, defendants may have been more inclined to request a postponement of their trial if initially scheduled during the transition period.

allows us to focus on those defendants who were most likely to have something to gain from having their trial postponed until after the reform.

While we do not find evidence of any change in the nature of the cases tried in the first, third,<sup>24</sup> and fourth quarters of 1999 and 2000 (columns 1 and 4), we find strong evidence of such a change occurring during the second quarter of the year 2000 (column 2). Indeed, compared to defendants tried in the same quarter in 1999, we observe that defendants tried during the transition period, especially those who received a prison sentence, had weaker ties to the labor market: they had lower income and were more likely to receive benefits, particularly unemployment-related benefits. While the effects are diluted when looking at the entire sample of defendants tried during the period (*Panel A*), the magnitude of the differences is particularly important and significant for the restricted subset of defendants who received a prison sentence and had something to gain from having their trial postponed (*Panel B*). For instance, at the sample mean, the earnings of the defendants who received a prison sentence dropped by 17.9% during the transition period. Overall, this suggests that wealthier individuals were more often able to have their case postponed until after the reform than other defendants.

In this context, an important question is whether or not this selection merely reflects an attempt by the courts to focus on offenders whose trial outcome did not depend on the timing of the trial during the transition period. If this was the case, offenders who were not eligible to the reform would represent a greater share of the individuals tried during the transition period.<sup>25</sup> We find no such evidence. In particular, we do not observe any change in the average number of drunk-driving crimes committed by offenders tried during the transition period. While defendants tried during the transition period had been convicted and incarcerated a greater number of times for crimes other than drunk driving, these characteristics did not in themselves constitute grounds for ineligibility. Hence, although these patterns may reflect the fact that the courts are biased against more serious criminals, we find no evidence that the observed selection was the result of sorting between eligible and ineligible offenders.

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<sup>24</sup> We 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*. Only one of these coefficients is statistically significant at the 1% level.

<sup>25</sup> Again, the reform did not systematically apply to offenders who had already been placed on probation for a *drunk-driving* crime more than once or to those who were on probation at the time of the crime for an *alcohol-related* crime.

Overall, these findings question the degree of consistency with which drunk-driving cases were handled in district courts around the time of the reform, as well as the level of fairness with which defendants were treated by the justice system during the transition period. From a methodological point of view, our results indicate that the way drunk-driving cases were handled in district courts during the transition period generated differences in the nature of defendants tried before and after the reform. Importantly, as the nature of the selection is relevant to the analysis of the impact of the reform on subsequent crime and labor market outcomes, it poses important concerns for identification and calls for a strategy that does not rely on the comparison of individuals tried before and after the reform. More generally, this also raises questions with respect to the performance of traditional quasi-experimental estimators when applied to the study of significant contextual changes (such as legislative reforms, program expansions, etc.) with consequences that can be anticipated by their stakeholders.<sup>26</sup>

## **4. Empirical strategy**

### **4.1. Intuition behind the instrument**

In order to measure the causal impact of the reform and to bypass the selection problem documented above, we use a novel instrumental variable approach. This approach relies on two features of the justice system which, when combined together, create exogenous variation in the probability for offenders to receive a custodial sentence. The first of these features is the fact that, as already mentioned above, Danish legislation guarantees that defendants tried after a reform for a crime committed prior to it must be tried under the more lenient of the two laws. This means that individuals tried after July 1, 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

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<sup>26</sup> Traditional quasi-experimental estimators raise additional selection problems which, although not discussed in detail here, remain essential. In particular, one concern is that the coming into force of the new law might have been accompanied (at least for a time) by more frequent police controls to compensate for the reduction in the expected cost of the punishment by increasing the probability of being caught drunk driving. Moreover, another concern is that potential offenders might have modified their behavior around the time of the reform. For instance, they might have anticipated the above-mentioned increase in road traffic controls and behaved more carefully in the weeks following the entering into force of the reform, thus reducing the overall number of drunk-driving crimes. Furthermore, conditional on individuals internalizing changes in the legislation, the reform could also have induced a modification in the characteristics of the individuals arrested for a drunk-driving crime after the law was passed. Indeed, the lowering of the cost associated with drunk-driving crimes should mechanically have led a new range of individuals to commit drunk-driving crimes (those reaping fewer benefits from committing a crime and/or incurring higher costs if caught), thereby increasing the overall number of drunk-driving crimes.



committed, the more likely the offender was to be tried after the reform under the new law, and therefore to avoid prison.

*Figure 2* provides visual support for this approach. It organizes 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 (as in *Figure 1*), and represents 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 April 4, 2000 (the date when the reform was signed) 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*.<sup>27</sup>

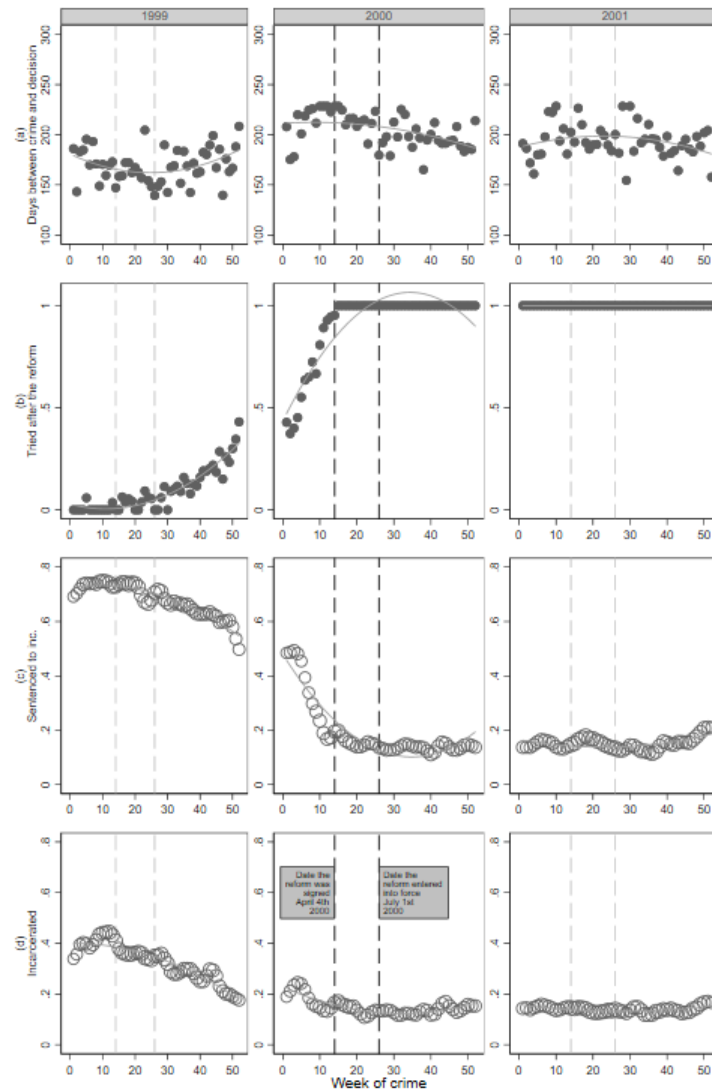
The first two indicators show that there is significant variation in the probability of being tried after the reform among individuals arrested for a drunk-driving crime in the 12-month period preceding the signing of the law. The first indicator shows that, around the time of the reform, the time gap between the moment when a prosecutor would press charges against an alleged drunk-driver and the moment when the district court rendered its decision was substantial. On average, this time gap was of six months for drunk-driving crimes committed in 1999. It can be noted that this gap tends to increase as crimes are committed closer to the reform and peaks for crimes committed in the weeks preceding the signature of the law (*Figure 2.a*). Note that this time gap was almost entirely driven by the case processing time in district courts. Possible implications for our design are discussed in *Section 4.4* below. The second indicator shows that the significant time lag resulted in a gradual increase in the probability of being tried after the signing of the reform in the 12 months preceding it: 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*).

The last two indicators confirm that variation in the probability of being tried after the reform translated into a corresponding variation in the probability of being sentenced to custody among individuals tried for a drunk-driving crime committed in the 12-month period *preceding* the signing of the reform, based on the date of their crime. Indeed, the share of defendants who received a

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<sup>27</sup> For data confidentiality reasons, indicators b), c), and d) displayed in *Figure 2* are calculated as moving averages. For each week  $w$ , the value of these indicators is calculated as the average value of the indicators over weeks  $w-1$ ,  $w$ , and  $w+1$ . Furthermore, for any given week, the share of cases tried after the reform is normalized to 0 if the actual number of cases tried *after* is equal to or lower than 3 (in total, this normalization was carried out for 16 weeks), and the share of cases tried after the reform is normalized to 1 if the actual number of cases tried *before* is equal to or lower than 3 (in total, this normalization was carried out for 4 weeks).

custodial sentence by week of crime started going down from July 1999 from slightly less than 80% to less than 20% right after the reform (*Figure 2.c*). The same pattern is observed for the share of defendants who were actually incarcerated following their trial (*Figure 2.d*).



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

## 4.2. Sampling strategy<sup>28</sup>

Inspired by the above observations, our approach consists in comparing individuals who committed their drunk-driving crime at different dates *prior* to the signature of the reform and therefore faced varying probabilities of incarceration, as discussed above. We focus on all individuals charged for a drunk-driving crime committed in the 24-month period preceding the signing of the law (between April 4, 1998 and April 3, 2000). Two characteristics of our sampling strategy should be highlighted. First, by focusing on crimes committed before the reform was signed, we avoid any selection problems that could arise due to an announcement effect. Second, while *Figure 2* shows that our instrument exhibits no variation among individuals charged for a drunk-driving crime committed 13 to 24 months before the signing of the reform (between week 15 of 1998 and week 14 of 1999), including these individuals in our sample allows us to control for seasonal variations using a time trend as well as year and month fixed effects.<sup>29</sup> Restricting our sample to defendants *tried* in the country at that time, we obtain a sample of 8,377 cases, corresponding to 7,982 distinct defendants.<sup>30</sup>

*Table 3* describes the characteristics of the defendants included in our sample. They are predominantly males in their late thirties. While close to 63.7% of them were holding some type of job at the end of the year preceding the date of their crime, 72.3% 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 five years (18.0% in the previous two years). Few of them were in a relationship (28.7%), and defendants born abroad and descendants of immigrants represented 4.9% and 0.4% of the sample respectively – slightly less than their actual share in the overall population in 2000, which was 5.6% and 1.5% respectively (DST, 2000).

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<sup>28</sup> *Appendix A.2.* provides detailed information on the administrative datasets used in this paper.

<sup>29</sup> We do not include individuals tried for a drunk-driving crime committed more than 24 months before the signing of the 2000 reform because the reform which lowered the administrative blood alcohol threshold from 0.8 to 0.5g/L entered into force on March 1<sup>st</sup>, 1998.

<sup>30</sup> 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).

### 4.3. Econometric specifications

#### *Reduced form and instrumental variable approaches*

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

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

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 when the law was signed (the unit for this variable is 100 days);<sup>31</sup>  $P_i$  is a period dummy taking the value 1 if individual  $i$ 's crime was committed in the 12-month period preceding the signing of the law and 0 if it was committed earlier (13 to 24 months prior to the reform); and  $X_i$  is a vector including all variables in the conditioning set detailed in *Appendix A.2*. More specifically, we control for various trial characteristics, such as whether the defendant was a juvenile at the time of the crime and the nature of the main charge (using a detailed seven-digit drunk-driving charge code). We also include defendants' background information, such as their gender, age at the time of the trial, immigration status,<sup>32</sup> their past criminal activity (the number of convictions for other drunk-driving crimes and non-road traffic crimes in the 5-year period preceding their crime), marital status, highest educational achievement, type of job held, and annual income. Unless specified otherwise, all baseline background characteristics included in the conditioning set were measured at the end of the year preceding the crime and are available for the vast majority of the offenders in our sample (the variables included in the conditioning set are all available from 1986). Finally,  $\mu_m$  and  $\mu_c$  are fixed effects indicating the calendar month when the crime was committed and the district court where the case was tried (there were 84 of them at the time of the reform).

Our instrument,  $I_i = (P_i * T_i)$ , thus captures the differential effect of the trend variable  $T_i$  for crimes committed in the 12-month period preceding the day the reform was signed, compared to crimes committed 13 to 24 months before the signature. The parameter of interest is  $\delta^{RF}$ , 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

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<sup>31</sup>  $T_i$  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.

<sup>32</sup> We use Statistics Denmark's typology: "immigrants", "descendants of immigrants", or "rest of the population."

time gap between the date of the crime and the signing of the reform in the 12-month period preceding it. In contrast, *Figure 2* suggests that there is no particular reason to expect  $\mu_1$  to be statistically different from 0.

Our main focus is on the IV estimates, which are obtained by instrumenting a dummy variable indicating whether individual  $i$  was incarcerated by  $I_i$  using a Two-Stage-Least-Squares (2SLS) estimation procedure. The resulting coefficients measure the impact of being incarcerated (as opposed to being placed on probation) on the *compliers*, *i.e.* the subset of defendants whose date of crime within the 12-month period preceding the signature of the reform had an impact on their being incarcerated.

#### *Standard OLS approach*

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

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

In this equation,  $inc_i$  is a dummy variable indicating whether individual  $i$  was incarcerated following their trial and  $\delta^{OLS}$  is the parameter of interest. However, the  $inc_i$  variable is likely to be endogenous in this specification as offenders who are incarcerated differ significantly from those who are not. As a consequence, unless all differences across these two groups are controlled for (which seems unlikely to occur), these OLS estimators are likely to yield biased estimates.

#### **4.4. Instrument validity**

##### *First-stage estimates and compliers' characteristics*

First-stage estimates are displayed in *Table 4*, measuring the impact of having committed a drunk-driving crime closer to the signing of the law on the probability of a defendant being incarcerated.<sup>33</sup> To do so, we regress our outcome variable on our instrument and an increasingly exhaustive set of control variables. From column 1 to column 4, we enrich the set of control variables by adding the following covariates successively and incrementally: a time trend, period, month-of-crime and district court fixed effects (column 1), dummy variables indicative of the nature of the drunk-driving charge

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<sup>33</sup> Our variable indicating whether or not an individual was incarcerated is a dummy variable capturing whether an individual has spent at least 10 days in prison – 10 days being the minimum duration of prison sentences requested for a drunk-driving crime.

(column 2), information about the criminal case (column 3), and defendant characteristics (column 4).

As expected, we find that, in the 12-month period preceding the signing of the reform, having committed a crime closer to the moment when the law was signed substantially reduced the probability of being incarcerated. Indeed, within that period, delaying their drunk-driving crime by 100 days would have reduced defendants' probability of being incarcerated by 7.2 percentage points. Note that both the magnitude and significance level of these estimates are robust to the inclusion of covariates in the regression, suggesting that, in the 12-month period preceding the signing of the law, the instrument is independent of both defendant and case characteristics.

In *Appendix A.4*, we describe the characteristics of the compliers, *i.e.* offenders whose date of crime had an impact on whether or not they received a custodial sentence. To do so, we use the methodology followed by Pinotti (2017), which consists in eliciting compliers' characteristics by the 2SLS regression of the product of the individual characteristics and the endogenous variable on the endogenous variable using  $I_i$  as an instrument. We find that the characteristics of the first group are roughly similar to those of the overall sample and, in particular, to those who received a prison sentence (whose characteristics are displayed in the first columns of *Table 2, Panel B*). This suggests that the selection described in *Section 3* does not sufficiently affect the characteristics of the compliers to constitute a threat to the external validity of our results – most likely because the selection only concerns a subset of offenders scheduled to be tried during the transition period, which ultimately represents a small subset of our entire sample.

#### *Independence, exclusion, and monotonicity*

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

The independence assumption implies that the instrument is independent of defendants' background characteristics and potential outcomes (once a time trend, period, month-of-crime and district court fixed effects are controlled for). In order to investigate the validity of this assumption, we study whether defendants' pre-crime characteristics are correlated with the instrument. We do so by regressing each of the background variables displayed in the left column of *Table 3* on the instrument, the time trend, as well as period, month-of-crime and district court fixed effects. For each regression, the coefficient and standard error associated with the instrument are reported in *Table 3*. We find that

the coefficients associated with the instrument are systematically small and are rarely statistically significant (out of the 30 investigated covariates, only two are statistically significant at the 10% level), indicating that the independence assumption is likely to be met. This also suggests that the reform was not anticipated by potential offenders prior to the date of its signature. 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 trial). One concern is that the risk of recidivism and/or prospects of employment might vary across defendants based on the timing of their crime or the date of their sanction. However, the inclusion in our sample of individuals tried for a drunk-driving crime committed 13 to 24 months before the signing of the reform allows us to mitigate the consequences of this potential problem by controlling for trend and seasonality effects. Another potential concern lies in the fact that, as displayed in *Figure 1*, the waiting time for trials slightly increased as crimes were committed closer to the date of the reform, which could be an issue since longer trial waiting times may theoretically foster subsequent crime outcomes. Given that offenders who received a custodial sentence did not have to wait as long as those who received a non-custodial sentence (because the latter were tried closer to the date of the reform), it is possible that our results (showing that custodial sentences foster crime compared to non-custodial ones) *underestimate* the negative impact of incarceration. However, two elements allow us to minimize the extent of this concern. First, the percentage increase in trial waiting time in the 12 months prior to the signing of the legislation remains limited. Second, while theoretical arguments suggest that punishment celerity should deter recidivism, empirical evidence now converges to say that it has no impact (see Pratt and Turanovic (2018) for a review of the evidence).<sup>34</sup>

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

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<sup>34</sup> In Pratt and Turanovic's own words: "*While exceptions exist [...] the general pattern revealed in this body of work is that celerity effects of punishment are nonexistent, and that even when present it can be difficult to disentangle such effects from other potentially confounding influences, like the perceived (or actual) certainty or severity of punishment. So again, the pattern is pretty clear that faster punishments appear to have little to no consistent, independent effect on one's future criminal behavior.*". While a few studies on the impact of the Hawaii Opportunity Probation with Enforcement (HOPE) project fostering swift-and-certain punishments found significant positive effects (Hawken and Kleiman, 2009; Kilmer et al., 2013), replication studies carried out in other states found no impact (see, for example, the experimental studies by Lattimore et al. (2016) and O'Connell et al. (2016)). *Criminology & Public Policy* devoted an issue to this topic, see for example Nagin (2016) and Cullen et al. (2016), in addition to the articles just cited.

preceding its signature. We investigate the validity of this assumption by estimating the first-stage equation for various subgroups of the sample: males, females, individuals aged below 30, individuals aged above 30, individuals with prior drunk-driving convictions, individuals without any prior drunk-driving conviction, etc. The coefficients and standard errors associated with each of the subgroups are reported in *Appendix A.5*. We find that the coefficients are all positive and statistically significant (as well as very similar in magnitude), which suggests that issues of non-monotonicity are probably limited as well.

## 5. Main Results

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

### 5.1. Impact on crime

We start by measuring the relative impact of custodial and non-custodial sentences on drunk drivers' post-sentencing involvement in criminal activities.<sup>35</sup> In *Figure 3*, we report on the differential effect of the two types of sentences on drunk-driving crimes (left panel) and other crimes (right panel), as measured by our IV estimates. To do so, the following two *cumulative* outcomes are computed every three months for both types of crime from the date when the drunk-driving case was settled in court: the probability of being convicted of a crime by time  $t$  (the *extensive* margin), displayed in the top panel; and the number of convictions by time  $t$  (the *intensive* margin), displayed in the bottom panel. A subset of the coefficients is displayed in *Table 5.A.* for drunk-driving crimes and in *Table 5.B.* for other crimes.

We find no differential impact on subsequent drunk-driving convictions, suggesting that custodial and non-custodial sentences have similar effects on this type of crime. Indeed, the two sanctions appear to be equally effective in preventing offenders from being reconvicted for a drunk-driving crime (*Figure 3.a*), and their impact on the average number of subsequent convictions for drunk driving is also similar (*Figure 3.c*). In both cases, point estimates are relatively small in magnitude and systematically fail to be statistically significant at the 5% level. This is so despite the fact that

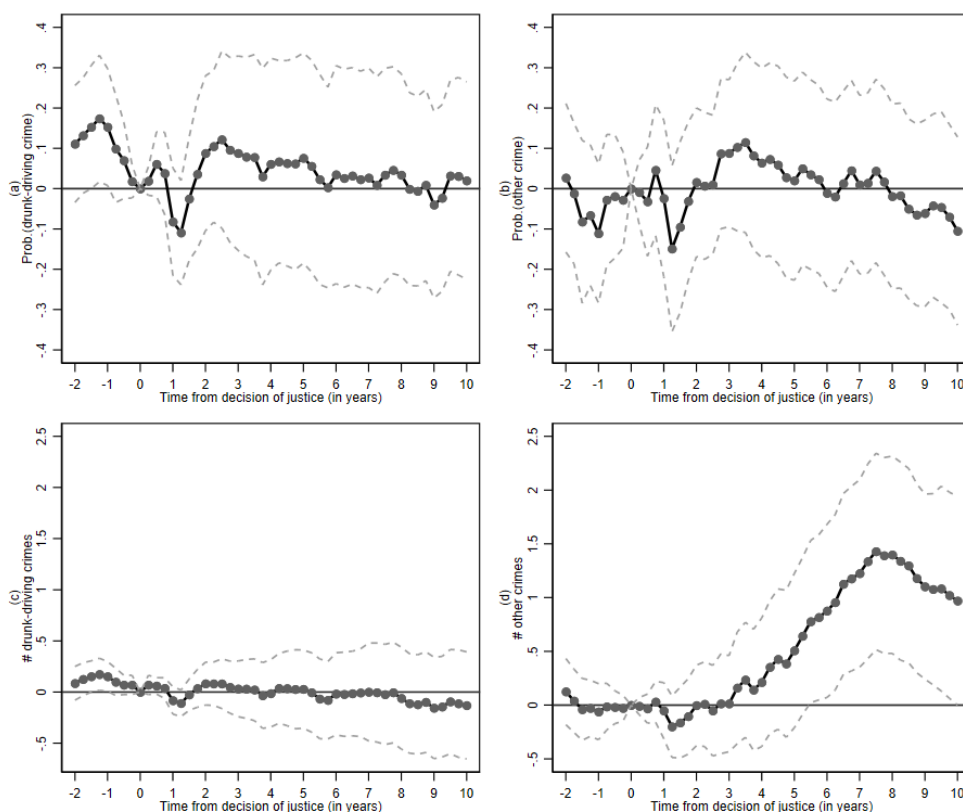
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<sup>35</sup> In order to measure the *net* impact of incarceration, we exclude from the calculation of these outcomes any crime registered under the same case ID or related to any other crime committed prior to the decision of justice considered in this study.



descriptive statistics show that there is significant room for improvement in terms of recidivism: as documented in *Table 5.A*, the average number of subsequent convictions for a drunk-driving crime among individuals included in our sample is 0.6 after ten years. At first glance, this result is somewhat surprising given that some of the offenders who received non-custodial sentences were required to participate in an alcohol rehabilitation program. However, one should keep in mind that, among the compliers, the number of defendants with a serious problem of alcohol dependence is probably limited because of the pardon scheme to which they were entitled before the reform.

In contrast, we find that, compared to non-custodial sentences, custodial ones increase the average *number* of convictions for crimes other than drunk driving (*Figure 3.d*). At its peak, the magnitude of this effect is large: our results indicate that custodial sentences increase the average number of convictions by 1.4 after eight years – representing a 89.8% increase at the sample mean. After ten years, the figures are 1.0 and 53.6% respectively. Because custodial and non-custodial sentences



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

appear to be equally effective in preventing individuals from being convicted of a crime other than drunk driving (*Figure 3.b*), the effect on the intensive margin is in fact mechanically larger (almost twice as large) on offenders who were convicted at least once. This suggests that the criminogenic effects custodial sentences were found to have on other offenders (Cullen et al., 2011; Aizer and Doyle, 2015) can also affect offenders exhibiting relatively low proclivity for criminal behavior, such as drunk drivers.

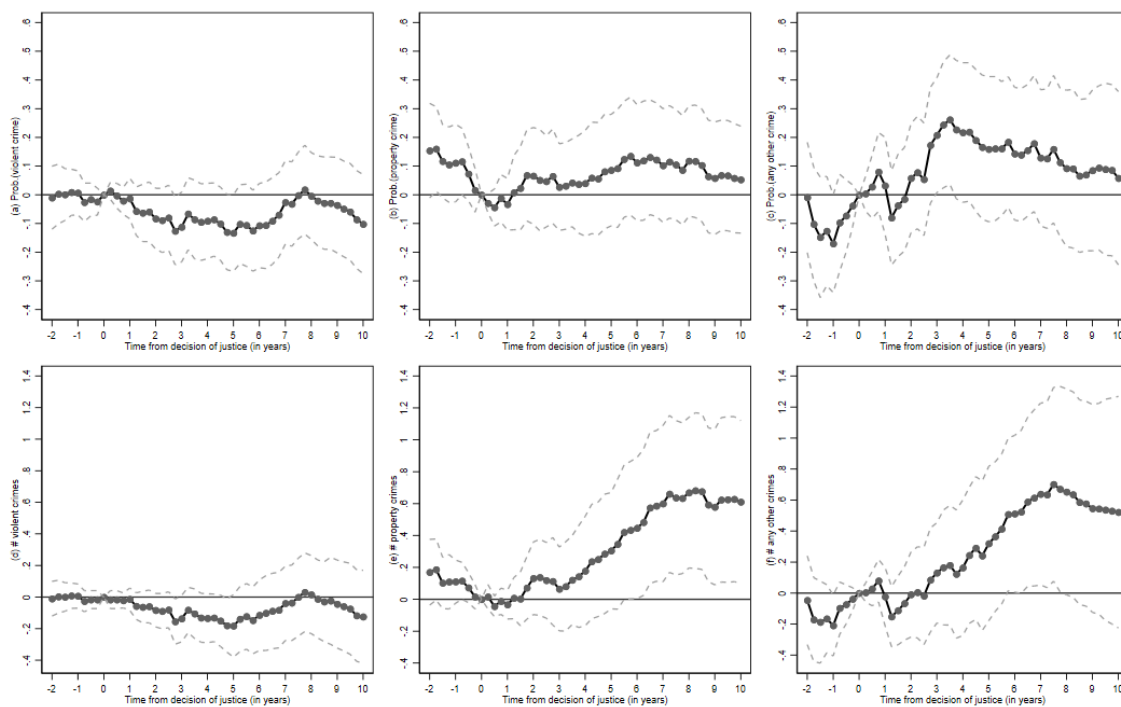
The timing of the effects is very telling in a number of ways. First of all, it is interesting to note that point estimates experience a brief and sudden drop 12 to 15 months after the trial, which is consistent with an incapacitation effect. Although not statistically significant, this suggests that, as expected, as long as offenders are kept behind bars, custodial sentences are more effective than non-custodial ones in preventing criminal behavior.<sup>36</sup> Then, once the incapacitation effect is no longer at play, the negative impact of incarceration after release becomes more and more pronounced over time, up to eight years after the initial decision of justice. This negative effect then seems to diminish slightly (although not sufficiently to become statistically different from the level reached after eight years), suggesting that it may not be irremediable. This turning point will be discussed further in *Section 5.3* below.

While all estimates (standard OLS approach, RF, and IV) reach similar conclusions on drunk-driving outcomes, the naive OLS approach yields different results than those just discussed for drunk-driving crimes and crimes other than drunk driving. As displayed in *Tables 5.A* and *5.B*, the standard OLS estimates suggest that custodial sentences decrease both the probability for a drunk-driver to be subsequently convicted of any other crime, and the number of such crimes they commit. For instance, these estimates suggest that custodial sentences tend to increase the probability of an offender being convicted of another drunk-driving crime within the next ten years by 2.3 percentage points and increase the number of such convictions by 0.1. Both results are statistically significant at the 5% level. In contrast, we find no statistically significant effect on the probability of an offender being convicted of any other crime or on the number of such convictions.

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<sup>36</sup> The reason why this incapacitation effect does not materialize immediately after the trial at time 0 lies in the fact that we focus on convictions (and not crimes) and that there was a significant time lag between the time of the alleged crime and the time of the verdict. This can also be explained by the waiting list system in place at the time, which could delay the incarceration of offenders for up to several months after the verdict was announced.

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



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

We perform several placebo tests to check the robustness of the results. First, we examine whether the instrument is correlated with the pretrial values of our outcome variables and show that our results

cannot be explained by a differential trend. Second, we replicate the same analysis on earlier 24-month periods in which no reforms were implemented around the same date (from April 4 1995 to April 3 1997, from April 4 1996 to April 3 1998, and from April 4 1997 to April 3 1999) (*Appendix A.6*). Again, the results suggest that our main findings cannot be explained by our quasi-experimental design and, in particular, by the fact that our identification method is based on comparing individuals arrested at different points in time. Finally, to investigate potential spurious correlations between our instrument and future labor market outcomes during the study period, we repeat the same analysis on individuals arrested for a drunk-driving crime in the 24-month period preceding the signing of the law but whose cases were not transferred to a court (*Appendix A.7*). We do not find any evidence that would compromise the integrity of our approach.

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

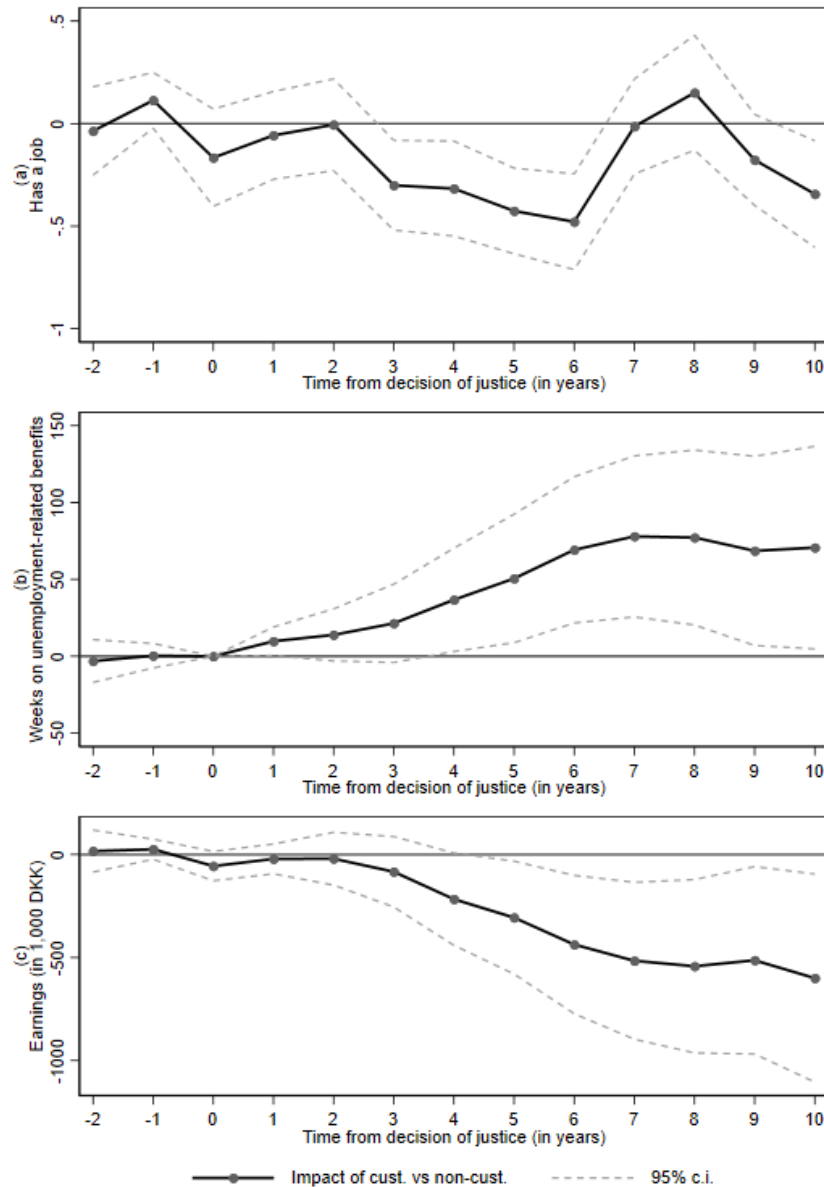
## **5.2. Impact on labor market attachment**

We now turn to the relative impact of custodial and non-custodial sentences on offenders' labor market outcomes, an important dimension of offenders' lives after release. To do so, we compute the following outcomes every year from the date when the drunk-driving case was settled in court: a) the probability of having a job in year  $t$ ;<sup>37</sup> b) the cumulative number of weeks during which offenders received unemployment-related benefits by time  $t$ ; c) the cumulative income by time  $t$ . IV estimates measuring the differential effect of the two sentences on these various labor market outcomes are reported in *Figure 5*. A subset of coefficients is also displayed in *Table 7*.

Overall, our results suggest that, compared to non-custodial sentences, custodial ones significantly weaken individuals' labor market attachment, and that they do so on a long-term basis. Indeed, despite the limited length of the incarceration spells, offenders sentenced to custody are less likely to be employed after receiving their sentence than those who only received a non-custodial sentence (top panel of *Figure 5*). As a consequence, they are more likely to rely on unemployment-related benefits (middle panel of *Figure 5*) and have lower earnings (bottom panel of *Figure 5*). This result holds for almost every year following the decision of justice – except for a couple of years towards the end of

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<sup>37</sup> Measured by Statistics Denmark at the end of the month of November.



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

the period (we comment further on this turning point below). In both cases, the magnitude of the effects is very large. For instance, ten years after the initial decision of justice, the probability of having a job is reduced by 34.3 percentage points for individuals who received a custodial sentence compared to those who received a non-custodial one (statistically significant at the 1% level) – representing a 99.1% decrease at the sample mean. As a consequence, incarceration represents a

cumulative loss of 599,874 kroner after ten years (statistically significant at the 5% level) – corresponding to a 45.9% decrease at the sample mean. Therefore, our findings suggest that incarceration has a dramatic impact on individuals’ attachment to the labor market. Note that these results are of the same order of magnitude as those obtained in Bhuller et al. (2020), although they go in the opposite direction.

Overall, these negative labor market consequences of incarceration may seem surprising, given that the periods of incarceration considered in this study are quite short (between 10 and 60 days, with an average of 18 days) and that drunk drivers are generally incarcerated in open prisons, in which offenders are allowed to leave the grounds during the day to go to work (although the suspension of their driver’s license may limit their ability to do so). Note, however, that the negative differential effect of incarceration on labor market outcomes materializes in the medium term, not at the time of incarceration. This suggests that this effect is not explained by a greater likelihood of losing one’s job after conviction, but rather by greater labor market difficulties thereafter.

Again, the timing of the effects is particularly revealing and crucial in this regard. First of all, the impact of the two sanctions on the probability of having a job (*Figure 5.a*) is remarkably similar during the two years following the trial, with differences remaining small in magnitude and non-statistically significant. It is only from the third year after the decision of justice that the negative effect of incarceration really begins to materialize. This may be so for a number of reasons, including the length of the probation period associated with non-custodial sentences (two years). It is also interesting to note that this turning point coincides with the moment when convictions cease to appear on the criminal record of those placed on probation (three years after the trial), while they remain on the criminal record of individuals sentenced to incarceration, whose record is only cleared five years after the date of their release. This suggests that there may be considerable stigma associated with having a criminal record, which weakens offenders’ post-release labor market attachment, in line with previous findings (Pager, 2003; Raphael, 2014; Agan and Starr, 2017, 2018; Mueller-Smith and Schnepel, 2021, Rose, 2021). After the seventh year, the difference between the two types of sanctions starts decreasing, echoing the pattern of results observed on crime outcomes. Interestingly, this coincides with the time when the conviction is removed from the criminal record of offenders sentenced to incarceration.<sup>38</sup> Towards the end of the period, the difference in the probability of having

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<sup>38</sup> As observed in *Section 5.1*, the incapacitation effect materializes about a year after conviction, meaning that the criminal record of incarcerated offenders is wiped clean five years later, *i.e.* six years after conviction.

a job becomes more pronounced again. This coincides with the rise in unemployment from 2009 onwards in the wake of the 2008 economic crisis and suggests that incarceration and the difficulties it creates for offenders' more immediate labor market attachment can make individuals more vulnerable to economic downturns, possibly as a result of their more erratic professional history. Overall, these observations suggest that the relative impact of custodial and non-custodial sentences can be heavily influenced by certain features of the sanction (*e.g.* number of years a conviction remains on the criminal record), as well as by the state of the economy, and in particular local labor market conditions, as recently shown by Yang (2017), Schnepel (2018), and Galbiati et al. (2020).

We perform the same placebo tests as those described in the previous section to check the robustness of the results and reach similar conclusions (*Appendices A.6 and A.7*).

### **5.3. Discussion: understanding the criminogenic effect of incarceration through the lens of labor market precariousness**

So far, our results indicate that incarceration increases the average number of subsequent crimes committed by drunk-driving offenders, compared to non-custodial sentences. These results are at odds with standard theoretical predictions that harsher sanctions should reduce crime.

The analysis of the relative impact of custodial and non-custodial sentences on labor market participation suggests the existence of an important mechanism at play regarding the criminogenic effect of incarceration: upon release, incarcerated offenders may use crime as a means of subsistence to compensate for the increased difficulties they encounter in the labor market because of the time they spent in prison. This explanation, in line with Becker (1968) and Ehrlich's (1973) theory of crime, is supported by several arguments in the context of this study. First, as discussed in *Section 5.2*, custodial sentences considerably weaken offenders' labor market attachment compared to non-custodial ones. Indeed, incarceration significantly reduces offenders' probability of having a job, increases their reliance on unemployment-related benefits, and, ultimately, reduces their income. This could explain the need to rely on criminal activities to earn money. Second, the increase in crime is mainly due to a surge in property crimes, which are essentially economically motivated crimes: burglary, fraud, handling of stolen goods, theft, robbery, shoplifting, etc. Third, the plot of the curve representing the relative impact of custodial and non-custodial sentences on property crimes (*Figure 4.e*) shows similarities with that of the curve representing the effect on labor market attachment (*Figure 5.a*). In particular, they show similar inflection points both in the short and in the longer term. Moreover, a close look at these estimates suggests that the deterioration of offenders' labor market

attachment slightly precedes the increase in the number of crimes committed.<sup>39</sup> Indeed, the impact on labor market attachment starts to materialize from the 3<sup>rd</sup> year (around the time when the criminal record of those who received a non-custodial sentence is expunged), while the effect on crime appears from the fourth year. Similarly, the detrimental impact of prison on labor market attachment begins to dissipate from the sixth year (around the time when the criminal record of those who received a custodial sentence is expunged), while the increase in the number of crimes committed comes to a halt from the eighth year.

Other traditional mediators can also explain part of the criminogenic effect of custodial relative to non-custodial sentences, but are less convincing in the context of this study. For instance, the detrimental effect of custodial sentences on offenders' mental health or human capital accumulation may help explain part of the increase in the number of crimes committed by incarcerated offenders, but not why the effect reaches a plateau or declines in the longer run. This is also the case for the mechanism whereby incarceration fosters criminal network formation and, in doing so, crime. Moreover, in the particular setting of this study, drunk-driving offenders were only incarcerated with minor offenders (precisely to avoid prison acting as a school of crime),<sup>40</sup> which limits the scope of this crime network mechanism. Nonetheless, we also compare the relative impact of custodial and non-custodial sentences on the likelihood that a drunk driver will subsequently be convicted of a crime committed with one or more other criminal(s), as well as the number of such crimes committed, and find no evidence to support the idea that prison expands these offenders' criminal network.<sup>41</sup>

## 6. Conclusion

In this paper, we investigate the long-term effects of incarceration relative to non-custodial sentences on the criminal behavior and labor market attachment of relatively mild offenders in a context where prison conditions are particularly good. To do this, we use a large-scale reform of Danish legislation, whereby a custodial sentence was replaced by a non-custodial sentence for most drunk driving offenses. The study reaches several important 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

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<sup>39</sup> One should keep in mind that our variables of interest are convictions, and that these occur with a certain delay in relation to crimes.

<sup>40</sup> Drunk-driving offenders benefited from a special regime reserved for minor offenders, called "hæfte", which guaranteed, among other things, that minor offenders would not be incarcerated with more serious criminals.

<sup>41</sup> Results are available upon request.



in line with their best interest. In the case of the change in the drunk-driving legislation studied here, we show that the way drunk-driving cases were handled in district courts changed drastically in the months preceding the entering into force of the reform (from the moment the law was signed) and that a large share of the cases which should have been tried before the reform was actually tried after. This means that a group of offenders who should have been tried before the reform was tried after and, as a consequence, avoided prison. Furthermore, we show that the identity of the individuals who had their case postponed was not random: in particular, wealthier defendants were more likely to have their trial put off until after the reform. From a policy perspective, this important result suggests that 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 significantly increase offenders' involvement in subsequent criminal activities. We find evidence that, in the context of the reform under study, this increase was not driven by drunk-driving crimes but by other crimes, especially property crimes. While it is not possible to pin down the exact mechanisms at play, part of the explanation for this increase in offenders' criminal activities can probably be found in their greater precariousness. In particular, we find that, compared to non-custodial sentences, custodial ones significantly weaken offenders' labor market attachment as they decrease both their probability of having a job and their total income. A close examination of the timing of the effects on labor market outcomes supports the notion that individuals with a criminal record are subject to stigma that hinders their employment opportunities after release – in line with evidence found in Pager (2003), Raphael (2014), Agan and Starr (2017, 2018), Mueller-Smith and Schnepel (2021), and Rose (2021).

Our study thus contributes to understanding the role of incarceration on post-release outcomes and complements the existing literature on the topic in a Scandinavian context (Dobbie et al., 2018; Bhuller et al., 2020; Huttunen et al., 2020), which have so far tended to suggest that custodial sentences may be more effective overall to reduce recidivism than non-custodial ones in countries with good detention conditions. Understanding what drives the differences with previous studies is beyond the scope of this article, but we hypothesize that part of the explanation may lie in differences in methodologies and compliers' characteristics. While we focus on a broad range of mild offenders

arrested for a single (yet widespread) crime, previous studies conversely focus on a specific type of defendants (on the margin of being incarcerated) convicted of a broader range of crimes – using random assignment of cases to judges with varying levels of severity. 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, and suggest that accompanying offenders following their release could help mitigate the costs of incarceration to offenders and to society. In this regard, reducing the length of time a conviction remains on the criminal record seems potentially effective in the context of this study, and worthy of further study.

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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 (any)	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
<i>Length in days (incl. 0s)</i>	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
<i>Length in days (incl. 0s)</i>	4,249	18.129	25.402	3,645	11.296	25.178	4,643	6.652	25.932
<i>Length between 1 and 60 days</i>	4,249	0.685	0.465	3,645	0.362	0.481	4,643	0.113	0.316
<i>Length over 60 days</i>	4,249	0.034	0.182	3,645	0.028	0.164	4,643	0.029	0.169
-Incarcerated	4,249	0.378	0.485	3,645	0.196	0.397	4,643	0.138	0.345
Other sentences (any)	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
<i>Amount in DKK (incl. 0s)</i>	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
<i>Length in months (incl. 0s)</i>	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**

Variables	All		Year 2000		Weeks 14-26		Weeks 27-39		Weeks 40-52	
	Mean	S.d.	$\Delta T1$		$\Delta T2-\Delta T1$		$\Delta T3-\Delta T1$		$\Delta T4-\Delta T1$	
			Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: all offenders</i>										
Has a job	0.641	0.480	0.005	0.078	-0.027	0.031	-0.012	0.035	-0.031	0.028
Income	257.290	140.786	8.854	20.720	-14.464	8.202 *	-18.180	9.784 *	-3.822	8.991
Earnings	161.606	159.416	7.970	25.187	-26.127	10.250 **	-17.413	12.914	-10.028	11.098
Weeks of social transfers	22.669	21.538	-0.860	3.796	1.477	1.336	0.817	1.609	0.694	1.264
Weeks of labour market-related transfers	13.192	18.444	3.877	3.261	1.340	1.151	1.273	1.280	-0.153	1.088
Weeks of health-related transfers	3.500	9.184	1.300	1.638	-0.561	0.502	0.034	0.518	0.754	0.626
Weeks of self-supporting transfers	0.741	5.110	-0.099	0.679	0.309	0.370	0.226	0.365	0.270	0.415
Weeks of pension-related transfers	5.237	15.503	-5.938	2.899 **	0.389	1.073	-0.716	1.129	-0.178	0.979
Nber of crimes in past 5 years	1.688	2.089	-0.261	0.404	0.276	0.177	0.073	0.130	-0.080	0.131
Nber of DD crimes in past 5 years	0.414	0.650	0.020	0.100	-0.032	0.042	-0.036	0.047	0.014	0.034
Nber of other crimes in past 5 years	1.277	1.949	-0.320	0.401	0.315	0.176 *	0.100	0.132	-0.084	0.132
Nber of inc. spells in past 5 years	0.418	0.873	-0.026	0.138	0.066	0.053	-0.048	0.049	-0.008	0.046
<i>Panel B: offenders who received a prison sentence (whether it be a conditional or an unconditional one)</i>										
Has a job	0.638	0.481	0.094	0.099	-0.056	0.042	-0.011	0.040	0.013	0.032
Income	263.483	136.818	40.278	27.111	-25.197	11.524 **	-20.627	12.252 *	1.596	9.690
Earnings	165.429	159.680	48.125	33.104	-29.673	13.391 **	-17.373	13.322	-3.524	11.319
Weeks of social transfers	23.206	21.570	-2.782	4.451	3.818	1.782 **	1.880	1.686	0.212	1.378
Weeks of labour market-related transfers	13.352	18.440	1.964	4.092	3.015	1.659 *	1.247	1.396	-1.166	1.310
Weeks of health-related transfers	3.701	9.427	0.239	1.819	-0.227	0.689	-0.135	0.702	0.937	0.730
Weeks of self-supporting transfers	0.670	4.900	0.170	0.895	0.182	0.374	0.131	0.336	0.555	0.451
Weeks of pension-related transfers	5.483	15.824	-5.155	3.017 *	0.848	1.439	0.638	1.375	-0.114	1.211
Nber of crimes in past 5 years	1.710	2.054	-0.570	0.426	0.514	0.213 **	0.028	0.158	0.100	0.137
Nber of DD crimes in past 5 years	0.537	0.699	0.028	0.130	0.047	0.056	0.065	0.055	0.004	0.038
Nber of other crimes in past 5 years	1.176	1.861	-0.655	0.413	0.481	0.201 **	-0.053	0.156	0.110	0.134
Nber of inc. spells in past 5 years	0.504	0.937	-0.030	0.173	0.254	0.085 ***	0.034	0.065	0.039	0.058

*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.

Number of observations: 7,894 in Panel A and 5,597 in Panel B.

Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 3: Sample description and balance checks**

Variables	All		Instrument	
	Mean	S.d.	Coeff.	s.e.
Female	0.087	0.282	0.002	0.007
Age, when the decision was rendered	38.268	10.944	-0.130	0.231
<i>Immigration status</i>				
Immigrant	0.049	0.216	-0.003	0.005
Descendant	0.004	0.061	-0.001	0.001
Rest of the population	0.947	0.224	0.004	0.005
<i>Family status</i>				
Single	0.491	0.500	0.006	0.012
In partnership	0.287	0.452	0.001	0.008
Separated	0.196	0.397	-0.011	0.008
Widow	0.014	0.119	-0.001	0.002
Unknown	0.012	0.108	0.004	0.002 *
<i>Education status</i>				
Primary education	0.524	0.499	0.012	0.012
Secondary education	0.378	0.485	-0.007	0.011
Higher education	0.062	0.242	-0.001	0.005
Unknown	0.035	0.184	-0.005	0.003
<i>Attachment to the labor market</i>				
Has a job	0.637	0.481	-0.013	0.009
Earnings	160.746	158.608	-4.629	2.989
Any social transfers	0.723	0.447	-0.015	0.009
Weeks of social transfers	22.676	21.518	0.104	0.438
Weeks of labour market-related transfers	13.213	18.437	0.356	0.379
Weeks of health-related transfers	3.404	8.993	-0.064	0.171
Weeks of self-supporting transfers	0.530	3.492	-0.036	0.064
Weeks of pension-related transfers	5.385	15.680	-0.135	0.288
<i>Criminal priors</i>				
Any crime in past 2 years	0.423	0.494	-0.010	0.010
<i>Any DD crime in past 2 years</i>	0.180	0.384	0.001	0.007
Nber of crimes in past 2 years	0.720	1.084	-0.003	0.021
<i>Nber of DD crimes in past 2 years</i>	0.199	0.445	0.006	0.008
Any crime in past 5 years	0.650	0.477	-0.021	0.009 **
<i>Any DD crime in past 5 years</i>	0.343	0.475	-0.013	0.009
Nber of crimes in past 5 years	1.692	2.085	-0.024	0.040
<i>Nber of DD crimes in past 5 years</i>	0.419	0.648	-0.001	0.012

*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.

Number of observations: 8,377.

Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 4: First-stage**

	(1)	(2)	(3)	(4)
Instrument	-0.072*** (0.009)	-0.071*** (0.009)	-0.071*** (0.009)	-0.072*** (0.009)
R-squared	0.059	0.067	0.070	0.079
Mean	0.337			
Trend	YES	YES	YES	YES
Period FE	YES	YES	YES	YES
Month-of-crime & 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 the impact of the 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 offenders' probability of being incarcerated. 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 detailed 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.

Number of observations: 8,377.

Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 5.A: Impact on drunk-driving crimes**

	All		OLS		RF		IV			
			(1)		(2)		(3)		(4)	
	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Probability of being convicted again for drink-driving</i>										
2 years before	0.157	0.364	0.010	0.008	-0.008	0.005	0.111	0.085	0.111	0.074
Within 2 years	0.106	0.308	0.013	0.009	-0.007	0.007	0.082	0.094	0.092	0.097
Within 4 years	0.205	0.403	0.021	0.010 **	-0.004	0.010	0.052	0.134	0.061	0.134
Within 6 years	0.278	0.448	0.010	0.010	-0.002	0.010	0.035	0.142	0.035	0.138
Within 8 years	0.323	0.468	0.013	0.010	-0.002	0.009	0.034	0.132	0.034	0.128
Within 10 years	0.358	0.479	0.023	0.011 **	-0.001	0.009	0.016	0.128	0.016	0.125
<i>Panel B: Number of drink-driving convictions</i>										
2 years before	0.171	0.414	0.010	0.009	-0.006	0.006	0.087	0.097	0.086	0.084
Within 2 years	0.118	0.356	0.012	0.010	-0.006	0.008	0.078	0.102	0.087	0.106
Within 4 years	0.247	0.520	0.033	0.013 **	0.001	0.012	-0.030	0.170	-0.013	0.168
Within 6 years	0.376	0.689	0.035	0.018 *	0.001	0.015	-0.034	0.211	-0.020	0.204
Within 8 years	0.477	0.827	0.040	0.021 *	0.004	0.019	-0.075	0.258	-0.060	0.254
Within 10 years	0.551	0.901	0.054	0.023 **	0.010	0.020	-0.152	0.274	-0.135	0.267
Trend			YES		YES		YES		YES	
Period FE			YES		YES		YES		YES	
Month-of-crime & district court FE			YES		YES		YES		YES	
Additional Cov.			YES		YES		NO		YES	
F-test										64.777

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

Number of observations: 8,377.

Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 5.B: Impact on other crimes**

	All		OLS		RF		IV			
	Mean	S.d.	(1)		(2)		(3)		(4)	
			Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Probability of being convicted again for any crime other than drunk driving</i>										
2 years before	0.309	0.462	-0.010	0.009	-0.002	0.007	0.090	0.103	0.027	0.094
Within 2 years	0.273	0.445	0.014	0.009	-0.001	0.007	0.025	0.101	0.016	0.094
Within 4 years	0.418	0.493	0.018	0.012	-0.005	0.009	0.066	0.123	0.064	0.120
Within 6 years	0.499	0.500	0.007	0.011	0.001	0.009	-0.008	0.127	-0.011	0.119
Within 8 years	0.547	0.498	0.009	0.010	0.001	0.009	-0.020	0.123	-0.019	0.117
Within 10 years	0.578	0.494	0.007	0.011	0.007	0.009	-0.105	0.124	-0.105	0.119
<i>Panel B: Number of convictions other than drunk-driving convictions</i>										
2 years before	0.482	0.874	-0.032	0.016 **	-0.009	0.011	0.259	0.203	0.126	0.157
Within 2 years	0.403	0.784	-0.008	0.015	0.000	0.014	0.033	0.208	-0.002	0.191
Within 4 years	0.836	1.362	0.014	0.031	-0.015	0.023	0.276	0.348	0.213	0.305
Within 6 years	1.244	1.916	-0.016	0.043	-0.062	0.031 **	0.955	0.503 *	0.867	0.411 **
Within 8 years	1.558	2.322	-0.025	0.053	-0.100	0.033 ***	1.460	0.578 **	1.399	0.468 ***
Within 10 years	1.812	2.665	-0.062	0.059	-0.069	0.036 *	1.065	0.619 *	0.971	0.495 *
Trend			YES		YES		YES		YES	
Period FE			YES		YES		YES		YES	
Month-of-crime & district court FE			YES		YES		YES		YES	
Additional Cov.			YES		YES		NO		YES	
F-test										64.777

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

Number of observations: 8,377.

Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

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

	All		OLS		RF		IV			
	Mean	S.d.	(1)		(2)		(3)		(4)	
			Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Probability of being convicted again for a violent crime</i>										
2 years before	0.046	0.209	-0.012	0.006 *	0.001	0.004	0.012	0.051	-0.010	0.056
Within 2 years	0.042	0.201	-0.005	0.004	0.006	0.004	-0.078	0.054	-0.083	0.054
Within 4 years	0.081	0.273	-0.003	0.007	0.007	0.005	-0.081	0.065	-0.091	0.067
Within 6 years	0.107	0.309	0.000	0.007	0.008	0.005	-0.109	0.081	-0.114	0.075
Within 8 years	0.129	0.335	0.001	0.009	0.000	0.006	-0.008	0.084	-0.004	0.078
Within 10 years	0.142	0.349	0.002	0.009	0.007	0.006	-0.104	0.095	-0.102	0.088
<i>Panel B: Probability of being convicted again for a property crime</i>										
2 years before	0.119	0.323	0.006	0.007	-0.011	0.006 **	0.172	0.085 **	0.154	0.084 *
Within 2 years	0.086	0.280	0.002	0.006	-0.005	0.006	0.078	0.089	0.066	0.086
Within 4 years	0.148	0.355	0.015	0.009	-0.003	0.007	0.050	0.102	0.040	0.094
Within 6 years	0.192	0.394	0.012	0.010	-0.008	0.008	0.118	0.110	0.111	0.103
Within 8 years	0.217	0.412	0.015	0.012	-0.008	0.007	0.111	0.107	0.118	0.098
Within 10 years	0.234	0.423	0.008	0.011	-0.004	0.007	0.050	0.107	0.053	0.095
<i>Panel C: Probability of being convicted again for any other crime (excluding drunk driving)</i>										
2 years before	0.209	0.407	-0.016	0.009 *	0.001	0.007	0.046	0.098	-0.009	0.098
Within 2 years	0.194	0.395	0.005	0.009	-0.004	0.007	0.063	0.099	0.058	0.097
Within 4 years	0.316	0.465	0.010	0.012	-0.015	0.009 *	0.220	0.127 *	0.217	0.124 *
Within 6 years	0.396	0.489	-0.006	0.011	-0.010	0.009	0.152	0.123	0.143	0.118
Within 8 years	0.449	0.497	-0.006	0.012	-0.007	0.010	0.102	0.141	0.092	0.139
Within 10 years	0.482	0.500	-0.007	0.012	-0.004	0.011	0.066	0.153	0.058	0.154
Trend			YES		YES		YES		YES	
Period FE			YES		YES		YES		YES	
Month-of-crime & district court FE			YES		YES		YES		YES	
Additional Cov.			YES		YES		NO		YES	

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

Number of observations: 8,377.

Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

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

	All		OLS		RF		IV			
			(1)		(2)		(3)		(4)	
	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Number of violent crimes</i>										
2 years before	0.046	0.209	-0.012	0.006 *	0.001	0.004	0.012	0.051	-0.010	0.056
Within 2 years	0.042	0.201	-0.005	0.004	0.006	0.004	-0.078	0.054	-0.083	0.054
Within 4 years	0.099	0.355	-0.005	0.008	0.010	0.006	-0.121	0.081	-0.134	0.079 *
Within 6 years	0.152	0.493	-0.004	0.011	0.009	0.008	-0.111	0.124	-0.121	0.114
Within 8 years	0.189	0.557	-0.001	0.014	-0.001	0.009	0.016	0.138	0.016	0.126
Within 10 years	0.224	0.656	0.001	0.016	0.009	0.011	-0.122	0.165	-0.127	0.151
<i>Panel B: Number of property crimes</i>										
2 years before	0.150	0.435	0.005	0.009	-0.012	0.007 *	0.199	0.116 *	0.170	0.105
Within 2 years	0.105	0.364	0.003	0.006	-0.009	0.008	0.145	0.120	0.132	0.116
Within 4 years	0.234	0.662	0.034	0.017 **	-0.013	0.013	0.201	0.198	0.178	0.175
Within 6 years	0.340	0.871	0.032	0.024	-0.032	0.017 *	0.469	0.246 *	0.446	0.226 **
Within 8 years	0.414	1.023	0.038	0.029	-0.048	0.018 ***	0.682	0.267 **	0.669	0.241 ***
Within 10 years	0.474	1.156	0.031	0.033	-0.044	0.019 **	0.635	0.290 **	0.611	0.260 **
<i>Panel C: Number of other crimes (excluding drunk driving)</i>										
2 years before	0.273	0.596	-0.028	0.014 **	0.003	0.011	0.042	0.148	-0.046	0.146
Within 2 years	0.248	0.563	-0.007	0.012	0.001	0.010	0.009	0.147	-0.009	0.140
Within 4 years	0.490	0.869	-0.012	0.022	-0.012	0.016	0.193	0.229	0.163	0.223
Within 6 years	0.727	1.153	-0.037	0.026	-0.036	0.019 *	0.553	0.292 *	0.508	0.258 **
Within 8 years	0.929	1.413	-0.062	0.032 *	-0.047	0.024 *	0.688	0.384 *	0.653	0.339 *
Within 10 years	1.089	1.599	-0.092	0.034 ***	-0.038	0.027	0.568	0.428	0.525	0.382
Trend			YES		YES		YES		YES	
Period FE			YES		YES		YES		YES	
Month-of-crime & district court FE			YES		YES		YES		YES	
Additional Cov.			YES		YES		NO		YES	

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

Number of observations: 8,377.

Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 7: Impact on labor market attachment**

	All		OLS		RF		IV			
			(1)		(2)		(3)		(4)	
	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Has a job</i>										
2 years before	0.635	0.481	0.004	0.008	0.003	0.008	0.108	0.149	-0.036	0.110
At 2 years	0.530	0.499	-0.040	0.010 ***	0.000	0.008	0.050	0.120	-0.005	0.114
At 4 years	0.465	0.499	-0.034	0.012 ***	0.023	0.009 ***	-0.262	0.125 **	-0.317	0.118 ***
At 6 years	0.464	0.499	-0.040	0.011 ***	0.034	0.009 ***	-0.430	0.130 ***	-0.478	0.119 ***
At 8 years	0.442	0.497	-0.016	0.011	-0.011	0.010	0.175	0.142	0.151	0.143
At 10 years	0.346	0.476	-0.024	0.010 **	0.025	0.009 ***	-0.325	0.125 ***	-0.343	0.133 ***
<i>Panel B: Cumulative number of weeks spent on unemployment-related benefits</i>										
2 years before	27.443	34.775	2.189	0.662 ***	0.220	0.516	-5.357	9.604	-3.078	7.087
Within 2 years	30.565	36.890	5.777	0.787 ***	-0.996	0.643	12.083	9.507	13.920	8.642
Within 4 years	59.943	68.095	11.645	1.348 ***	-2.629	1.268 **	32.767	18.715 *	36.748	17.147 **
Within 6 years	86.947	97.170	17.245	2.009 ***	-4.952	1.767 ***	62.752	25.501 **	69.217	24.252 ***
Within 8 years	106.539	121.270	21.661	2.519 ***	-5.523	2.133 ***	68.811	30.568 **	77.203	29.008 ***
Within 10 years	123.205	141.845	25.464	3.017 ***	-5.056	2.491 **	61.139	35.770 *	70.666	33.594 **
<i>Panel C: Cumulative earnings (in 1,000 DKK)</i>										
2 years before	316.54	301.01	9.96	4.49 **	-1.21	3.80	107.85	81.79	16.95	52.06
Within 2 years	275.69	301.04	-7.87	6.10	1.44	4.82	30.87	73.98	-20.12	65.36
Within 4 years	534.75	574.34	-17.43	12.60	15.50	8.28 *	-121.75	130.70	-216.64	114.33 *
Within 6 years	794.45	850.16	-28.22	18.35	31.26	12.14 ***	-305.03	195.04	-436.99	171.32 **
Within 8 years	1066.43	1132.85	-39.20	24.37	38.79	15.59 **	-383.43	248.27	-542.25	214.66 **
Within 10 years	1306.09	1397.66	-47.21	31.15	42.92	18.85 **	-425.30	291.67	-599.87	257.74 **
Trend			YES		YES		YES		YES	
Period FE			YES		YES		YES		YES	
Month-of-crime & district court FE			YES		YES		YES		YES	
Additional Cov.			YES		YES		NO		YES	

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

Number of observations: 8,377.

Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.



## Appendix

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

Between 1990 and 1994, drunk-driving offenders with an alcohol abuse problem who were sentenced to no more than 40 days could benefit from a *pardon scheme*. In this case, their custodial sentence could be commuted to a two-year probation period and 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 then decided 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 the offender’s driving license. This sanction is identical to the one that was 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 only to offenders exhibiting a strong need for rehabilitation. Towards the end of the 1990s, between 75 and 80% of offenders enrolled in a rehabilitation program as part of this pardon scheme were eventually granted a pardon.

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

## Appendix A.2. Data sources<sup>43</sup>

### 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 contain 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 whom 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).<sup>44</sup> Finally, they also contain information about the outcome of every criminal case settled by the police, a prosecutor, or a judge. For each decision of justice, information is available on the identity of the defendants, the entity responsible for issuing the sanction, the date when the sanction was issued, as well as the nature of the sanction (a prison sentence, a fine, a withdrawal, an acquittal, etc.) and its severity (fine amounts, probation and custodial sentence length, etc.). Finally, drivers' blood alcohol content at the time of their arrest is not available in the datasets.

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<sup>43</sup> 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: <https://www.dst.dk/extranet/forskningvariabellister/Oversigt%20over%20registre.html> (accessed in June 2022).

We provide further information on each of these datasets in *Table A.2* (placed in the appendix).

<sup>44</sup> These crime and charge codes are recorded by the police using a detailed 7-digit hierarchical code (1,161 codes) – the last three digits often indicating the severity of the crime. For instance, 60 different codes can be used to categorize drunk-driving crimes and charges (29 of which were effectively encountered during the study period).

### *Information on labor market attachment*

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

### **B. Outcome variables**

Using these registers, we compute the two groups of outcome variables on which this study focuses: 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 were, the number of such crimes they committed. We also compute an outcome indicating whether or not individuals were convicted of any other crimes and, if they were, the number of such crimes they committed. We measure the impact of the reform on these outcomes from three months to ten 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 following the ruling. More specifically, we focus on the relative impact of custodial and non-custodial sentences on the annual number of days worked, as well as on annual income (which is inflated to 2015 prices using Statistics Denmark's Consumer Price Index). We measure the impact of the reform on these outcomes at different time horizons, from one to ten years from the date when the drunk-driving case was settled in court.

### **C. Control variables**

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

convictions in the 5-year period preceding their crime for other drunk-driving crimes and other crimes), marital status, highest educational achievement, type of job held, and annual income.

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

## VARIABLES DESCRIPTION

### A. Conditioning set

<b>Variables</b>	<b>Description</b> <i>N.B.: Names in italics refer to variables made available by Statistics Denmark. More information on each of these variables can be found on their website: <a href="https://www.dst.dk">https://www.dst.dk</a></i>
Detailed charge code	Variable source: information on the exact nature of the charge was retrieved from the <i>SIG_GER7</i> variable. The typology is available on the following webpage (accessed in June 2022): <a href="https://www.dst.dk/en/Statistik/dokumentation/nomenklaturer/overtraedelsestyper-i-kriminalstatistikken">https://www.dst.dk/en/Statistik/dokumentation/nomenklaturer/overtraedelsestyper-i-kriminalstatistikken</a>
Offender's number of drunk-driving convictions in the 5 years preceding the crime	Computed from the date of crime. Top-coded at the 99 <sup>th</sup> percentile.  Variable source: we calculated this information ourselves using the exhaustiveness of the registers. Information on the date of the crime was retrieved from the <i>SIG_GERIDTO</i> variable.
Offender's number of other convictions in the 5 years preceding the crime	Computed from the date of crime. Top-coded at the 99 <sup>th</sup> percentile.  Variable source: we calculated this information ourselves using the exhaustiveness of the registers. Information on the date of the crime was retrieved from the <i>SIG_GERIDTO</i> variable.
Number of times the offender was incarcerated in the 5 years prior to the crime	Computed from the date of crime. Top-coded at the 99 <sup>th</sup> percentile.  Variable source: we calculated this information ourselves using the exhaustiveness of the registers. Information on the date of the crime was retrieved from the <i>SIG_GERIDTO</i> 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 <i>FOED_DAG</i> variable and information on the date of the crime from the <i>SIG_GERIDTO</i> variable.
Offender's gender	Variable source: information on the gender of the offender was retrieved from the <i>AFG_KOEN</i> variable.
Offender's age at the time of the trial	Variable source: information on the gender of the offender was retrieved from the <i>AFG_AFGALD</i> variable.
Offender's immigration status	Dummy variables indicative of the following four groups of individuals:  - Immigrants

	<ul style="list-style-type: none"> <li>- Descendants of immigrants</li> <li>- Unknown status</li> <li>- Rest of the population</li> </ul> <p>Variable source: information on the immigration status of the offender was retrieved from the <i>IE_TYPE</i> variable.</p>
Offender's marital status	<p>Dummy variables indicative of the following five groups of individuals:</p> <ul style="list-style-type: none"> <li>- Single</li> <li>- In a partnership</li> <li>- Separated</li> <li>- Widow</li> <li>- Unknown status</li> </ul> <p>Measured at the end of the year preceding the offender's crime.</p> <p>Variable source: information on the marital status of the offender was retrieved from the <i>CIVST</i> variable.</p>
Offender's highest educational achievement	<p>Dummy variables indicative of the following four groups of individuals:</p> <ul style="list-style-type: none"> <li>- Primary education</li> <li>- Secondary education</li> <li>- Higher education</li> <li>- Unknown highest educational achievements</li> </ul> <p>Measured at the end of the year preceding the offender's crime.</p> <p>Variable source: information on the education status of the offender was retrieved from the <i>HFFSP2</i> variable.</p>
Offender's earnings	<p>Annual earnings before tax and any other social contributions. Top-coded each year at the 99<sup>th</sup> percentile. Missing values were given the value 0. Measured at the end of the year preceding the offender's crime.</p> <p>Variable source: information on the offender's earnings was retrieved from the <i>LOENMV</i> variable.</p>
Offender's job status	<p>Dummy variables indicative of whether or not an individual has a job. Missing values were given the value 0.</p> <p>Measured at the end of the year preceding the offender's crime.</p> <p>Variable source: information on the employment status of the offender was retrieved from the <i>PSTILL</i> variable.</p>

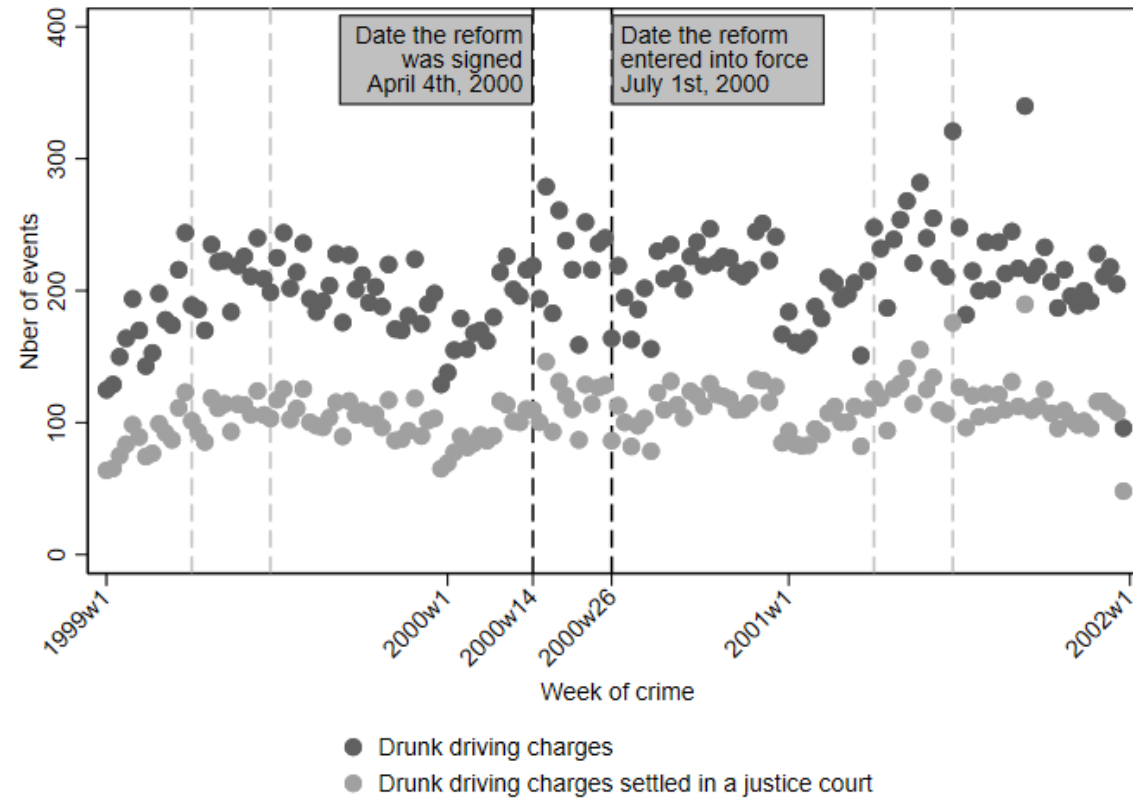
Offender's reliance on transfers	<p>Dummy variables indicative of whether or not an individual received any social transfer in the 12-month period preceding their crime. Missing values were given the value 0.</p> <p>Measured on a weekly-basis.</p> <p>Variable source: information on the offender's social transfers was retrieved from the <i>DREAM</i> database.</p>
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## B. Outcome variables

Outcome variables	<b>Description</b> <i>N.B.: Names in italics refer to variables made available by Statistics Denmark. More information on each of these variables can be found on their website: <a href="https://www.dst.dk">https://www.dst.dk</a></i>
Offender's number of convictions following their trial	<p>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.</p> <p>Top-coded at the 99<sup>th</sup> percentile.</p> <p>Variable source: we calculated this information ourselves using the exhaustiveness of the registers. Information on the date of each crime was retrieved from the <i>SIG_GERIDTO</i> variable.</p>
Whether or not an offender was convicted following their trial	<p>Computed from the above variable.</p>
Offender's earnings (cumulative)	<p>Annual earnings before tax and any other social contributions. Top-coded each year at the 99<sup>th</sup> percentile. Missing values were given the value 0.</p> <p>Measured at the end of each year, following the offender's trial.</p> <p>Variable source: information on the offender's earnings was retrieved from the <i>LOENMV</i> variable.</p>
Offender's reliance on unemployment-related transfers (cumulative)	<p>Annual number of weeks during which an individual received unemployment-related transfers</p> <p>Measured on a weekly-basis.</p> <p>Variable source: information on the offender's social transfers was retrieved from the <i>DREAM</i> database.</p>
Offender's job status	<p>Dummy variables indicative of whether or not an individual has a job. Missing values were given the value 0.</p> <p>Measured at the end of the year preceding the offender's crime.</p> <p>Variable source: information on the employment status of the offender was retrieved from the <i>PSTILL</i> variable.</p>



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



## Appendix A.4. Compliers' characteristics

Variables	All		Compliers
	Mean	S.d.	Coeff.
Female	0.087	0.282	-0.038
Age, when the decision was rendered	38.268	10.944	36.653
<i>Education status</i>			
Primary education	0.524	0.499	0.546
Secondary education	0.378	0.485	0.392
Higher education	0.062	0.242	0.056
Unknown	0.035	0.184	0.006
<i>Attachment to the labor market</i>			
Has a job	0.637	0.481	0.774
Earnings	160.746	158.608	216.603
Any social transfers	0.723	0.447	0.742
Weeks of social transfers	22.676	21.518	17.108
Weeks of labour market-related transfers	13.213	18.437	10.687
Weeks of health-related transfers	3.404	8.993	2.947
Weeks of self-supporting transfers	0.530	3.492	0.609
Weeks of pension-related transfers	5.385	15.680	2.749
<i>Criminal priors</i>			
Any crime in past 2 years	0.423	0.494	0.507
<i>Any DD crime in past 2 years</i>	0.180	0.384	0.201
Nber of crimes in past 2 years	0.720	1.084	0.731
<i>Nber of DD crimes in past 2 years</i>	0.199	0.445	0.194
Any crime in past 5 years	0.650	0.477	0.778
<i>Any DD crime in past 5 years</i>	0.343	0.475	0.457
Nber of crimes in past 5 years	1.692	2.085	1.709
<i>Nber of DD crimes in past 5 years</i>	0.419	0.648	0.472

*Notes:* In this table, we describe the characteristics (mean and standard deviation) of the defendants included in our sample, as well as those of the compliers. To do the latter, we follow the methodology described in Pinotti (2005). "DD" stands for "drunk-driving." Number of observations: 8,377.

## Appendix A.5. First-stage by subgroup

Subgroups	# Obs.	Probability of being incarcerated	
		Mean	Instrument
Gender	<i>Female</i>	727	0.274 (0.039)
	<i>Male</i>	7,656	0.342 -0.083*** (0.010)
Age	<i>Below 30</i>	2,218	0.310 -0.066*** (0.018)
	<i>Between 30 and 40</i>	2,789	0.360 -0.082*** (0.017)
	<i>Above 40</i>	3,376	0.334 -0.067*** (0.017)
Origin	<i>Immigrant</i>	410	0.280 -0.101* (0.060)
	<i>Descendant of immigrant(s)</i>	441	0.281 -0.068 (0.058)
	<i>Other</i>	7,941	0.340 -0.072*** (0.009)
Education	<i>Lower education</i>	4,397	0.342 -0.076*** (0.012)
	<i>Higher education</i>	3,691	0.331 -0.069*** (0.016)
Employment status	<i>Has a job</i>	5,340	0.327 -0.082*** (0.011)
	<i>Does not have a job</i>	3,043	0.353 -0.052*** (0.015)
Prior drunk driving	<i>No prior drunk driving</i>	3,427	0.301 -0.054*** (0.015)
	<i>Prior drunk driving(s)</i>	4,956	0.361 -0.085*** (0.012)
Prior incarceration	<i>No prior incarceration spell</i>	4,647	0.297 -0.071*** (0.012)
	<i>Prior incarceration spell(s)</i>	3,736	0.385 -0.077*** (0.014)

Notes: In this table, we estimate the first stage equation for various subgroups of the sample. More specifically, a dummy variable indicative of whether or not a defendant received a custodial sentence or was incarcerated is regressed on our instrument, a trend, a dummy variable indicating whether the crime was committed in the 12 month period preceding the signing 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.

## Appendix A.6. Placebo tests, different periods

### A. First stage

	(1)	(2)	(3)	(4)
<b>Panel A: 1995-1997</b>				
Instrument	-0.005 (0.009)	-0.004 (0.009)	-0.005 (0.009)	-0.005 (0.009)
R-squared	0.033	0.048	0.052	0.061
Mean	0.459			
Observations	7,817			
<b>Panel B: 1996-1998</b>				
Instrument	-0.001 (0.013)	-0.001 (0.013)	-0.000 (0.013)	0.000 (0.013)
R-squared	0.026	0.037	0.042	0.050
Mean	0.447			
Observations	7,869			
<b>Panel C: 1997-1999</b>				
Instrument	-0.004 (0.011)	-0.005 (0.011)	-0.006 (0.011)	-0.008 (0.010)
R-squared	0.023	0.035	0.040	0.049
Mean	0.424			
Observations	7,983			
Trend	YES	YES	YES	YES
Period FE	YES	YES	YES	YES
Month-of-crime & 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 perform placebo tests using as sample individuals who were tried for a drunk-driving crime committed between April 4, 1995 and April 3, 1997 (Panel A), between April 4, 1996 and April 3, 1998 (Panel B), and between April 4, 1997 and April 3, 1999 (Panel C) instead of individuals who were tried for a drunk-driving crime committed between April 4, 1998 and April 3, 2000. The reported coefficients capture the differential effect of the trend variable for crimes committed in the last 12 months of the period under consideration, compared to those committed in the first 12 months. For each period, 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 last 12 months of the period under consideration, and month of crime and district court fixed effects are added to the regression; in column (2) dummy variables indicative of the detailed 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: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## B. Results

### Drunk driving

	1995-1997				1996-1998				1997-1999			
	Sample		RF		Sample		RF		Sample		RF	
	Mean	S.d.	Coeff.	s.e.	Mean	S.d.	Coeff.	s.e.	Mean	S.d.	Coeff.	s.e.
<i>Panel A: Probability of being convicted again for drunk driving</i>												
2 years before	0.137	0.344	0.000	0.005	0.142	0.349	0.007	0.005	0.150	0.357	0.003	0.005
Within 2 years	0.080	0.272	-0.004	0.005	0.089	0.285	0.002	0.005	0.099	0.298	0.002	0.007
Within 4 years	0.155	0.362	0.001	0.008	0.171	0.376	-0.002	0.008	0.193	0.395	-0.002	0.009
Within 6 years	0.229	0.420	-0.004	0.010	0.247	0.431	-0.006	0.008	0.269	0.444	-0.007	0.010
Within 8 years	0.287	0.452	-0.009	0.011	0.308	0.461	-0.002	0.008	0.324	0.468	-0.007	0.010
Within 10 years	0.336	0.472	-0.014	0.011	0.349	0.477	0.010	0.009	0.356	0.479	-0.010	0.010
<i>Panel B: Number of drunk-driving convictions</i>												
2 years before	0.137	0.344	0.000	0.005	0.153	0.388	0.008	0.006	0.163	0.401	0.003	0.005
Within 2 years	0.080	0.272	-0.004	0.005	0.089	0.285	0.002	0.005	0.099	0.298	0.002	0.007
Within 4 years	0.179	0.444	0.004	0.009	0.202	0.472	-0.007	0.010	0.230	0.500	-0.002	0.012
Within 6 years	0.298	0.610	-0.004	0.014	0.329	0.647	-0.014	0.013	0.360	0.670	-0.005	0.015
Within 8 years	0.397	0.715	-0.008	0.016	0.443	0.789	-0.010	0.015	0.472	0.814	-0.008	0.017
Within 10 years	0.503	0.849	-0.010	0.018	0.532	0.880	0.002	0.018	0.545	0.892	-0.013	0.019

*Notes:* In this table, we perform placebo tests using as sample individuals who were tried for a drunk-driving crime committed between April 4, 1995 and April 3, 1997 (Panel A), between April 4, 1996 and April 3, 1998 (Panel B), and between April 4, 1997 and April 3, 1999 (Panel C) instead of individuals who were tried for a drunk-driving crime committed between April 4, 1998 and April 3, 2000. We report Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2). The reported coefficients capture the differential effect of the trend variable for crimes committed in the last 12 months of the period under consideration, compared to those committed in the first 12 months. Standard errors are clustered at the district court and individual levels.

Number of observations: 7,817 for 1995-1997; 7,869 for 1996-1998; and 7,983 for 1997-1999.

Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

### Other crimes

	1995-1997				1996-1998				1997-1999			
	Sample		RF		Sample		RF		Sample		RF	
	Mean	S.d.	Coeff.	s.e.	Mean	S.d.	Coeff.	s.e.	Mean	S.d.	Coeff.	s.e.
<i>Panel A: Probability of being convicted again for any crime other than drunk driving</i>												
2 years before	0.288	0.453	0.009	0.006	0.290	0.454	-0.002	0.007	0.299	0.458	0.010	0.007
Within 2 years	0.256	0.436	0.010	0.009	0.256	0.436	-0.018	0.009 **	0.261	0.439	0.009	0.010
Within 4 years	0.384	0.486	0.004	0.010	0.387	0.487	-0.001	0.009	0.397	0.489	-0.003	0.011
Within 6 years	0.461	0.498	0.001	0.010	0.465	0.499	-0.001	0.009	0.481	0.500	0.004	0.011
Within 8 years	0.511	0.500	-0.002	0.011	0.516	0.500	0.006	0.008	0.532	0.499	0.000	0.010
Within 10 years	0.547	0.498	-0.003	0.011	0.549	0.498	0.006	0.007	0.563	0.496	-0.001	0.009
<i>Panel B: Number of convictions other than drunk-driving convictions</i>												
2 years before	0.444	0.837	0.018	0.011	0.454	0.854	0.000	0.011	0.469	0.869	0.022	0.013 *
Within 2 years	0.367	0.740	0.007	0.015	0.358	0.696	-0.013	0.013	0.386	0.768	0.008	0.017
Within 4 years	0.739	1.245	0.013	0.027	0.753	1.251	0.019	0.022	0.796	1.333	-0.002	0.029
Within 6 years	1.086	1.739	0.024	0.037	1.120	1.766	0.032	0.027	1.187	1.876	0.025	0.035
Within 8 years	1.420	2.228	0.021	0.045	1.466	2.266	0.060	0.036 *	1.523	2.321	0.035	0.041
Within 10 years	1.715	2.643	-0.007	0.052	1.738	2.649	0.080	0.041 *	1.778	2.674	0.032	0.047

*Notes:* In this table, we perform placebo tests using as sample individuals who were tried for a drunk-driving crime committed between April 4, 1995 and April 3, 1997 (Panel A), between April 4, 1996 and April 3, 1998 (Panel B), and between April 4, 1997 and April 3, 1999 (Panel C) instead of individuals who were tried for a drunk-driving crime committed between April 4, 1998 and April 3, 2000. We report Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2). The reported coefficients capture the differential effect of the trend variable for crimes committed in the last 12 months of the period under consideration, compared to those committed in the first 12 months. Standard errors are clustered at the district court and individual levels.

Number of observations: 7,817 for 1995-1997; 7,869 for 1996-1998; and 7,983 for 1997-1999.

Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Decomposition, extensive margin*

	1995-1997				1996-1998				1997-1999			
	Sample		RF		Sample		RF		Sample		RF	
	Mean	S.d.	Coeff.	s.e.	Mean	S.d.	Coeff.	s.e.	Mean	S.d.	Coeff.	s.e.
<i>Panel A: Probability of being convicted again for a violent crime</i>												
2 years before	0.038	0.192	0.003	0.005	0.044	0.206	-0.004	0.005	0.045	0.207	0.005	0.004
Within 2 years	0.031	0.173	0.001	0.004	0.037	0.189	-0.003	0.004	0.040	0.196	-0.003	0.004
Within 4 years	0.062	0.240	0.001	0.005	0.070	0.256	-0.002	0.006	0.074	0.261	-0.003	0.005
Within 6 years	0.082	0.275	0.001	0.006	0.094	0.292	0.000	0.006	0.101	0.301	-0.005	0.005
Within 8 years	0.100	0.300	0.000	0.006	0.114	0.317	0.007	0.006	0.121	0.326	-0.003	0.005
Within 10 years	0.118	0.323	-0.003	0.006	0.132	0.339	0.011	0.006 *	0.136	0.343	-0.007	0.006
<i>Panel B: Probability of being convicted again for a property crime</i>												
2 years before	0.117	0.322	0.002	0.005	0.117	0.321	0.000	0.006	0.117	0.321	0.008	0.005
Within 2 years	0.078	0.268	0.006	0.006	0.080	0.272	-0.002	0.006	0.086	0.281	0.003	0.006
Within 4 years	0.144	0.351	0.014	0.008 *	0.150	0.357	-0.005	0.007	0.149	0.356	-0.001	0.007
Within 6 years	0.184	0.388	0.019	0.009 **	0.190	0.392	-0.012	0.006 *	0.192	0.394	0.003	0.008
Within 8 years	0.213	0.410	0.016	0.009 *	0.218	0.413	-0.003	0.007	0.219	0.413	-0.003	0.007
Within 10 years	0.231	0.422	0.014	0.009	0.235	0.424	-0.002	0.007	0.236	0.425	-0.004	0.007
<i>Panel C: Probability of being convicted again for any other crime (excluding drunk driving)</i>												
2 years before	0.194	0.396	0.008	0.007	0.194	0.395	0.013	0.008	0.203	0.402	-0.001	0.007
Within 2 years	0.183	0.386	0.000	0.007	0.178	0.382	-0.006	0.009	0.183	0.386	0.012	0.009
Within 4 years	0.287	0.452	-0.001	0.009	0.288	0.453	0.016	0.015	0.299	0.458	0.003	0.010
Within 6 years	0.362	0.481	-0.006	0.010	0.365	0.481	0.024	0.019	0.379	0.485	0.013	0.011
Within 8 years	0.414	0.493	-0.010	0.010	0.418	0.493	0.029	0.023	0.434	0.496	0.010	0.012
Within 10 years	0.454	0.498	-0.010	0.011	0.456	0.498	0.034	0.027	0.468	0.499	0.010	0.011

*Notes:* In this table, we perform placebo tests using as sample individuals who were tried for a drunk-driving crime committed between April 4, 1995 and April 3, 1997 (Panel A), between April 4, 1996 and April 3, 1998 (Panel B), and between April 4, 1997 and April 3, 1999 (Panel C) instead of individuals who were tried for a drunk-driving crime committed between April 4, 1998 and April 3, 2000. We report Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2). The reported coefficients capture the differential effect of the trend variable for crimes committed in the last 12 months of the period under consideration, compared to those committed in the first 12 months. Standard errors are clustered at the district court and individual levels.

Number of observations: 7,817 for 1995-1997; 7,869 for 1996-1998; and 7,983 for 1997-1999.

Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Decomposition, intensive margin*

	1995-1997				1996-1998				1997-1999			
	Sample		RF		Sample		RF		Sample		RF	
	Mean	S.d.	Coeff.	s.e.	Mean	S.d.	Coeff.	s.e.	Mean	S.d.	Coeff.	s.e.
<i>Panel A: Number of violent crimes</i>												
2 years before	0.038	0.192	0.003	0.005	0.044	0.206	-0.004	0.005	0.045	0.207	0.005	0.004
Within 2 years	0.031	0.173	0.001	0.004	0.037	0.189	-0.003	0.004	0.040	0.196	-0.003	0.004
Within 4 years	0.074	0.306	-0.002	0.007	0.087	0.335	0.001	0.008	0.091	0.344	-0.006	0.006
Within 6 years	0.104	0.371	-0.002	0.008	0.122	0.401	0.004	0.009	0.143	0.479	-0.010	0.008
Within 8 years	0.143	0.483	-0.007	0.011	0.165	0.520	0.015	0.011	0.180	0.550	-0.005	0.008
Within 10 years	0.175	0.540	-0.010	0.012	0.195	0.567	0.019	0.011 *	0.215	0.642	-0.013	0.010
<i>Panel B: Number of property crimes</i>												
2 years before	0.144	0.422	0.002	0.006	0.145	0.424	-0.003	0.008	0.145	0.426	0.012	0.007
Within 2 years	0.095	0.348	0.008	0.008	0.098	0.350	-0.003	0.007	0.106	0.366	0.008	0.008
Within 4 years	0.228	0.663	0.020	0.015	0.221	0.597	-0.003	0.011	0.232	0.655	0.010	0.014
Within 6 years	0.331	0.863	0.032	0.019 *	0.338	0.867	-0.006	0.014	0.338	0.868	0.015	0.017
Within 8 years	0.423	1.051	0.033	0.022	0.426	1.049	0.002	0.017	0.421	1.040	0.013	0.019
Within 10 years	0.499	1.221	0.021	0.025	0.495	1.205	0.011	0.020	0.481	1.178	0.014	0.022
<i>Panel C: Number of other crimes (excluding drunk driving)</i>												
2 years before	0.251	0.571	0.010	0.010	0.256	0.585	0.013	0.008	0.268	0.597	-0.002	0.011
Within 2 years	0.222	0.501	-0.001	0.008	0.216	0.497	-0.006	0.009	0.222	0.501	0.007	0.012
Within 4 years	0.430	0.803	-0.003	0.014	0.432	0.801	0.016	0.015	0.460	0.840	-0.004	0.020
Within 6 years	0.624	1.043	-0.004	0.021	0.637	1.058	0.024	0.019	0.680	1.110	0.023	0.024
Within 8 years	0.815	1.289	-0.007	0.025	0.842	1.321	0.029	0.023	0.887	1.361	0.029	0.029
Within 10 years	0.991	1.508	-0.021	0.028	1.007	1.527	0.034	0.027	1.047	1.556	0.038	0.031

*Notes:* In this table, we perform placebo tests using as sample individuals who were tried for a drunk-driving crime committed between April 4, 1995 and April 3, 1997 (Panel A), between April 4, 1996 and April 3, 1998 (Panel B), and between April 4, 1997 and April 3, 1999 (Panel C) instead of individuals who were tried for a drunk-driving crime committed between April 4, 1998 and April 3, 2000. We report Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2). The reported coefficients capture the differential effect of the trend variable for crimes committed in the last 12 months of the period under consideration, compared to those committed in the first 12 months. Standard errors are clustered at the district court and individual levels.

Number of observations: 7,817 for 1995-1997; 7,869 for 1996-1998; and 7,983 for 1997-1999.

Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Labor market attachment

	1995-1997				1996-1998				1997-1999			
	Sample		RF		Sample		RF		Sample		RF	
	Mean	S.d.	Coeff.	s.e.	Mean	S.d.	Coeff.	s.e.	Mean	S.d.	Coeff.	s.e.
<i>Panel A: Has a job</i>												
2 years before	0.619	0.486	-0.004	0.009	0.628	0.483	-0.001	0.009	0.627	0.484	-0.005	0.008
At 2 years	0.564	0.496	0.008	0.009	0.557	0.497	-0.002	0.008	0.544	0.498	-0.011	0.008
At 4 years	0.538	0.499	-0.008	0.011	0.513	0.500	-0.019	0.011 *	0.480	0.500	0.010	0.008
At 6 years	0.486	0.500	0.013	0.008	0.462	0.499	0.003	0.010	0.448	0.497	-0.008	0.009
At 8 years	0.446	0.497	0.004	0.009	0.449	0.497	0.000	0.008	0.455	0.498	-0.010	0.008
At 10 years	0.452	0.498	-0.015	0.009 *	0.432	0.495	-0.004	0.008	0.384	0.486	-0.009	0.009
<i>Panel B: Cumulative number of weeks spent on unemployment-related benefits</i>												
2 years before	29.122	34.136	-1.516	0.465 ***	28.440	34.133	0.417	0.655	27.554	34.366	0.337	0.574
Within 2 years	30.295	35.824	-0.411	0.641	30.016	35.902	0.422	0.588	29.910	36.259	0.040	0.697
Within 4 years	57.092	65.634	-1.126	1.343	57.774	66.696	1.483	1.150	58.205	67.436	0.441	1.264
Within 6 years	82.262	92.970	-1.162	1.896	83.775	94.232	2.549	1.622	85.039	95.796	0.556	1.620
Within 8 years	105.943	118.217	-1.270	2.333	107.133	119.469	3.256	2.149	106.972	120.891	0.106	1.951
Within 10 years	125.056	140.884	-1.464	2.663	123.961	140.625	4.529	2.605 *	122.127	140.999	0.958	2.277
<i>Panel C: Cumulative earnings (in 1,000 DKK)</i>												
2 years before	319.17	301.27	-0.29	3.40	317.96	296.94	-2.29	4.34	314.28	296.21	2.09	4.14
Within 2 years	289.32	303.17	7.33	5.02	283.10	298.22	-7.32	5.30	277.43	298.09	1.68	4.59
Within 4 years	584.79	588.88	15.06	10.56	570.67	578.50	-17.99	10.46 *	549.81	574.36	-2.94	8.77
Within 6 years	867.23	870.30	18.41	16.14	836.02	849.02	-21.88	15.70	801.03	840.31	-2.52	12.49
Within 8 years	1126.72	1136.40	25.53	20.21	1094.73	1117.66	-25.03	20.42	1061.79	1114.33	-3.06	16.69
Within 10 years	1392.74	1415.38	29.77	24.49	1364.97	1398.32	-33.45	25.01	1320.73	1389.06	-13.29	21.64

*Notes:* In this table, we perform placebo tests using as sample individuals who were tried for a drunk-driving crime committed between April 4, 1995 and April 3, 1997 (Panel A), between April 4, 1996 and April 3, 1998 (Panel B), and between April 4, 1997 and April 3, 1999 (Panel C) instead of individuals who were tried for a drunk-driving crime committed between April 4, 1998 and April 3, 2000. We report Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2). The reported coefficients capture the differential effect of the trend variable for crimes committed in the last 12 months of the period under consideration, compared to those committed in the first 12 months. Standard errors are clustered at the district court and individual levels. Number of observations: 7,817 for 1995-1997; 7,869 for 1996-1998; and 7,983 for 1997-1999. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.



## Appendix A.7. Placebo tests, individuals convicted of a drunk-driving crime (outside of a court of law)

### A. Results

#### *Drunk driving*

	All		OLS		RF	
	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Probability of being convicted again for drunk driving</i>						
2 years before	0.028	0.165	-0.046	0.019 **	-0.003	0.002
Within 2 years	0.071	0.257	0.013	0.027	-0.004	0.004
Within 4 years	0.135	0.342	0.015	0.034	-0.002	0.006
Within 6 years	0.188	0.391	0.041	0.040	0.003	0.006
Within 8 years	0.221	0.415	0.082	0.043 *	0.005	0.007
Within 10 years	0.243	0.429	0.091	0.044 **	0.006	0.007
<i>Panel B: Number of drunk driving convictions</i>						
2 years before	0.028	0.165	-0.046	0.019 **	-0.003	0.002
Within 2 years	0.071	0.257	0.013	0.027	-0.004	0.004
Within 4 years	0.155	0.413	0.008	0.043	-0.002	0.007
Within 6 years	0.226	0.500	0.061	0.056	0.004	0.008
Within 8 years	0.290	0.612	0.093	0.065	0.009	0.010
Within 10 years	0.334	0.669	0.105	0.070	0.013	0.011
Trend			YES		YES	
Period FE			YES		YES	
Month-of-crime & district court FE			YES		YES	
Additional Cov.			YES		YES	

*Notes:* In this table, we focus on individuals who were arrested for drunk driving and whose case was not sent to court. We report Ordinary Least Squares (OLS) estimates derived from the regression of our outcome variable on a dummy variable indicating whether or not individual  $i$  received a custodial sentence (equation 3); and Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2). 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). Standard errors are clustered at the district court and individual levels.

Number of observations: 13,318.

Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Other crimes

	<b>All</b>		<b>OLS</b>		<b>RF</b>	
	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Probability of being convicted again for any crime other than drunk driving</i>						
2 years before	0.203	0.402	0.110	0.040 ***	0.004	0.006
Within 2 years	0.197	0.398	0.111	0.041 ***	-0.011	0.006 *
Within 4 years	0.298	0.457	0.128	0.045 ***	0.001	0.007
Within 6 years	0.366	0.482	0.160	0.043 ***	0.004	0.007
Within 8 years	0.413	0.492	0.154	0.043 ***	0.008	0.008
Within 10 years	0.444	0.497	0.130	0.043 ***	0.004	0.008
<i>Panel B: Number of convictions other than drunk-driving convictions</i>						
2 years before	0.313	0.736	0.150	0.074 **	0.011	0.010
Within 2 years	0.291	0.695	0.183	0.078 **	-0.023	0.011 **
Within 4 years	0.586	1.222	0.353	0.140 **	-0.002	0.020
Within 6 years	0.876	1.723	0.563	0.195 ***	-0.014	0.027
Within 8 years	1.118	2.100	0.595	0.230 ***	-0.009	0.033
Within 10 years	1.317	2.427	0.690	0.270 **	-0.009	0.037
Trend			YES		YES	
Period FE			YES		YES	
Month-of-crime & district court FE			YES		YES	
Additional Cov.			YES		YES	

*Notes:* In this table, we focus on individuals who were arrested for drunk driving and whose case was not sent to court. We report: Ordinary Least Squares (OLS) estimates derived from the regression of our outcome variable on a dummy variable indicating whether or not individual *i* received a custodial sentence (equation 3); and Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2). 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). Standard errors are clustered at the district court and individual levels.

Number of observations: 13,318.

Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Decomposition, extensive margin*

	<b>All</b>		<b>OLS</b>		<b>RF</b>	
	Mean	S.d.	<b>(1)</b>		<b>(2)</b>	
			Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Probability of being convicted again for a violent crime</i>						
2 years before	0.030	0.171	0.020	0.025	0.002	0.003
Within 2 years	0.025	0.158	0.029	0.023	-0.003	0.003
Within 4 years	0.048	0.214	0.078	0.033 **	-0.002	0.004
Within 6 years	0.067	0.250	0.086	0.035 **	-0.001	0.004
Within 8 years	0.079	0.269	0.072	0.035 **	0.000	0.005
Within 10 years	0.089	0.285	0.074	0.036 **	0.000	0.005
<i>Panel B: Probability of being convicted again for a property crime</i>						
2 years before	0.078	0.268	0.040	0.030	0.002	0.004
Within 2 years	0.056	0.229	0.083	0.034 **	-0.002	0.004
Within 4 years	0.092	0.289	0.128	0.039 ***	0.002	0.005
Within 6 years	0.116	0.320	0.117	0.039 ***	0.004	0.005
Within 8 years	0.132	0.338	0.117	0.040 ***	0.008	0.005
Within 10 years	0.142	0.349	0.111	0.040 ***	0.007	0.005
<i>Panel C: Probability of being convicted again for any other crime (excluding drunk driving)</i>						
2 years before	0.137	0.343	0.074	0.038 *	0.003	0.006
Within 2 years	0.150	0.357	0.048	0.033	-0.013	0.006 **
Within 4 years	0.240	0.427	0.060	0.041	-0.002	0.007
Within 6 years	0.308	0.462	0.117	0.043 ***	-0.001	0.007
Within 8 years	0.357	0.479	0.105	0.042 **	0.002	0.008
Within 10 years	0.390	0.488	0.128	0.043 ***	0.002	0.008
Trend			YES		YES	
Period FE			YES		YES	
Month-of-crime & district court FE			YES		YES	
Additional Cov.			YES		YES	

*Notes:* In this table, we focus on individuals who were arrested for drunk driving and whose case was not sent to court. We report: Ordinary Least Squares (OLS) estimates derived from the regression of our outcome variable on a dummy variable indicating whether or not individual *i* received a custodial sentence (equation 3); and Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2). 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). Standard errors are clustered at the district court and individual levels.

Number of observations: 13,318.

Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

*Decomposition, intensive margin*

	<b>All</b>		<b>OLS</b>		<b>RF</b>	
	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Number of violent crimes</i>						
2 years before	0.030	0.171	0.020	0.025	0.002	0.003
Within 2 years	0.025	0.158	0.029	0.023	-0.003	0.003
Within 4 years	0.060	0.281	0.096	0.043 **	-0.003	0.005
Within 6 years	0.085	0.338	0.102	0.047 **	-0.004	0.006
Within 8 years	0.102	0.373	0.095	0.050 *	-0.005	0.006
Within 10 years	0.130	0.466	0.080	0.055	-0.010	0.008
<i>Panel B: Number of property crimes</i>						
2 years before	0.098	0.357	0.032	0.038	0.001	0.005
Within 2 years	0.069	0.302	0.101	0.045 **	-0.003	0.005
Within 4 years	0.139	0.495	0.174	0.069 **	0.000	0.008
Within 6 years	0.212	0.727	0.244	0.104 **	0.000	0.012
Within 8 years	0.251	0.804	0.256	0.111 **	0.006	0.013
Within 10 years	0.293	0.933	0.284	0.130 **	0.005	0.015
<i>Panel C: Number of other crimes (excluding drunk driving)</i>						
2 years before	0.169	0.452	0.091	0.048 *	0.005	0.007
Within 2 years	0.183	0.463	0.048	0.041	-0.015	0.007 **
Within 4 years	0.368	0.779	0.081	0.073	-0.001	0.012
Within 6 years	0.550	1.039	0.216	0.103 **	-0.007	0.016
Within 8 years	0.719	1.267	0.215	0.121 *	-0.002	0.020
Within 10 years	0.863	1.507	0.288	0.141 **	0.001	0.023
Trend			YES		YES	
Period FE			YES		YES	
Month-of-crime & district court FE			YES		YES	
Additional Cov.			YES		YES	

*Notes:* In this table, we focus on individuals who were arrested for drunk driving and whose case was not sent to court. We report: Ordinary Least Squares (OLS) estimates derived from the regression of our outcome variable on a dummy variable indicating whether or not individual  $i$  received a custodial sentence (equation 3); and Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2). 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). Standard errors are clustered at the district court and individual levels.

Number of observations: 13,318.

Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Labor market attachment

	All		OLS		RF	
	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Has a job</i>						
2 years before	0.629	0.483	-0.071	0.042 *	-0.003	0.007
At 2 years	0.594	0.491	-0.204	0.042 ***	-0.008	0.007
At 4 years	0.541	0.498	-0.135	0.038 ***	-0.005	0.008
At 6 years	0.536	0.499	-0.085	0.041 **	-0.004	0.008
At 8 years	0.526	0.499	-0.066	0.041	-0.030	0.008 ***
At 10 years	0.441	0.497	-0.118	0.034 ***	0.007	0.008
<i>Panel B: Cumulative number of weeks spent on unemployment-related benefits</i>						
2 years before	15.425	27.964	3.267	3.268	-0.042	0.417
Within 2 years	17.551	30.332	10.409	3.687 ***	-0.041	0.467
Within 4 years	34.942	55.749	19.384	6.800 ***	0.042	0.877
Within 6 years	52.344	79.954	29.012	9.872 ***	-0.705	1.281
Within 8 years	65.023	99.864	34.477	12.138 ***	-0.442	1.617
Within 10 years	75.065	115.996	38.835	14.129 ***	0.368	1.917
<i>Panel C: Cumulative earnings (in 1,000 DKK)</i>						
2 years before	318.56	308.69	-45.40	21.27 **	-3.72	3.51
Within 2 years	328.91	319.07	-131.32	19.30 ***	-6.26	4.43
Within 4 years	647.97	615.28	-233.45	35.89 ***	-8.81	8.87
Within 6 years	966.01	913.42	-299.78	51.77 ***	-8.50	13.29
Within 8 years	1301.87	1219.86	-374.75	69.50 ***	-22.62	17.87
Within 10 years	1616.34	1520.88	-456.53	86.41 ***	-32.36	22.41
Trend			YES		YES	
Period FE			YES		YES	
Month-of-crime & district court FE			YES		YES	
Additional Cov.			YES		YES	

*Notes:* In this table, we focus on individuals who were arrested for drunk driving and whose case was not sent to court. We report: Ordinary Least Squares (OLS) estimates derived from the regression of our outcome variable on a dummy variable indicating whether or not individual *i* received a custodial sentence (equation 3); and Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2). 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). Standard errors are clustered at the district court and individual levels. Number of observations: 13,318. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.