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Camille Hémet, Bastien Michel

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Long-run evidence from an anticipated reform**

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JEL Codes: K14, K42, J24.

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

Bastien Michel<sup>†</sup>    Camille Hémet<sup>‡</sup>

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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 all custodial sentences passed in 1999. Our first key finding is that stakeholders anticipated the consequences of the reform and that wealthier offenders managed to postpone their trial until after the reform came into force to avoid prison. To measure the relative impact of incarceration, we therefore use a novel instrumental variable approach exploiting quasi-exogenous variation in the probability of being tried after the reform, and therefore incarcerated, based on offenders' crime date. We follow sampled individuals over a 15-year period and find that incarcerated offenders commit more crimes and have weaker ties to the labor market after release. Additionally, first-time offenders are more negatively affected than repeat offenders.

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

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

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<sup>†</sup> Corresponding author: Nantes University (LEMNA) and Aarhus University (Department of Economics and Business Economics) – [bastien.michel@univ-nantes.fr](mailto:bastien.michel@univ-nantes.fr)

<sup>‡</sup> Paris School of Economics, Université Paris 1 Panthéon Sorbonne – [camille.hemet@psemail.eu](mailto:camille.hemet@psemail.eu)

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## 1. Introduction

While 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 being questioned. In particular, although one of the main objectives of custodial sentences is to deter criminals from reoffending (the so-called *specific* deterrent effect), available evidence suggests that they may not be more effective in preventing recidivism than non-custodial sentences (see Villettaz et al. (2015), Chalfin and McCrary (2017), Roodman (2020), and Loeffler and Nagin (2022) for recent reviews).<sup>1</sup> However, recent studies from Finland and Norway (Bhuller et al., 2020; Huttunen et al., 2020), two countries with some of the most favorable prison conditions in the world, suggest that improving incarceration conditions can make custodial sentences more effective overall than non-custodial ones in combating recidivism.<sup>2</sup>

Our study sheds new light on this issue by providing contrasting evidence on the relative impact of custodial and non-custodial sentences up to 15 years post-sentencing 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.<sup>3</sup> Drunk driving is a very common offense and has a high cost for society worldwide.<sup>4</sup> In Denmark, it accounted for a quarter of all custodial sentences passed in 1999. Importantly, the reform affected offenders who did not exhibit an alcohol problem, which allows us to interpret our results as capturing the impact of incarceration on mild offenders. This

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<sup>1</sup> It should be noted that the distribution of estimates of the effect of incarceration is particularly dispersed. For example, focusing solely on studies using US data, some find positive effects (Hjalmarsson, 2009; Norris et al., 2021), others find insignificant or mixed ones (Loeffler, 2013; Nagin and Snodgrass, 2013; Green and Winik, 2010; Eren and Mocan, 2021), while still others find negative ones (Aizer and Doyle, 2015; Mueller-Smith, 2015). Note that we separate studies that attempt to measure the effect of incarceration from those that focus on the impact of incarceration length. We argue that these two groups of studies answer different research questions, with the latter focusing primarily on understanding whether the fixed costs of incarceration can be partially offset or augmented by the variable costs of incarceration.

<sup>2</sup> However, both papers show heterogeneity in the results, as further discussed below.

<sup>3</sup> The probation period was combined with a fine and community service or mandatory participation in a rehabilitation program.

<sup>4</sup> Drunk driving accounts for over 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 has 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). In countries where it is liable to imprisonment, it is often one of the most frequent charges resulting in custodial sentences.

reduces external validity concerns stemming from the fact that drunk-drivers may constitute a specific type of offenders, particularly prone to alcohol problems.<sup>5</sup>

An analysis of how the reform was implemented leads to our first key result, 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: wealthier defendants were more likely to have their trial delayed, thus avoiding prison. This selection in the nature of the offenders tried before and after the reform precludes us from using estimation methods that rely on the comparison of these two groups to measure the impact of the reform.<sup>6</sup>

To study the relative effects of custodial and non-custodial sentences in the context of this reform, we instead opt for 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, 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. Second, 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. 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 to 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>5</sup> In short, this is due to the fact that the reform extended to offenders without alcohol problems a system that had already existed for several years for people with alcohol problems.

<sup>6</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 probability of committing another crime, incarceration significantly and persistently increases the average number of crimes. Fifteen years after the trial, the average number of convictions is higher by 1.7 among incarcerated offenders (statistically significant at the 5% level), which represents a 53.7% increase at the sample mean. 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 crimes other than drunk driving, and, in particular, economically motivated crimes (burglary, fraud, handling of stolen goods, theft, robbery, shoplifting, etc.).

Consistent with these findings, we observe that custodial sentences weaken offenders' labor market attachment as compared to non-custodial ones: incarceration significantly reduces offenders' probability of having a job, increases their reliance on unemployment-related benefits, and, ultimately, reduces their earnings. After 10 years, incarceration represents a cumulative loss of 620,680 kroner (statistically significant at the 1% level), corresponding to a 42.7% reduction at the sample mean. After 15 years, incarceration still represents a cumulative loss of 602,140 kroner (statistically significant at the 10% level), corresponding to a 32.2% reduction at the sample mean.

Additionally, we find significant heterogeneity in the effect of incarceration depending on prior incarceration history. For individuals who had never previously been incarcerated, we show that custodial sentences increase the probability of being convicted and the number of convictions in the 15 years following conviction. For those with prior incarcerations, by contrast, we find that incarceration significantly reduces the likelihood of committing another crime, and has no effect on the number of subsequent convictions. Similarly, the negative effects on offenders' labor market participation are important for individuals who had not previously been incarcerated, but limited for those who had.

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

being 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. Using Danish administrative data to follow a group of well-identified mild offenders<sup>8</sup> over a period of 15 years, to our knowledge the longest period studied in this literature, we actually draw more mixed conclusions. This prompts caution about the link between prison conditions and the relative effectiveness of custodial and non-custodial sentences that cross-country comparisons may suggest.<sup>9</sup>

Our results also contribute to the growing body of research showing that there is considerable heterogeneity in the effect of incarceration – which in turn may contribute to explain differences in results across studies. In recent years, a few studies in the US (Jordan et al., 2023) and Norway (Bhuller et al., 2020) have emphasized that incarceration may have a particularly rehabilitative effect on first-time offenders. Here again, our study prompts caution, as our results indicate that incarceration has a particularly detrimental impact on first-time offenders sentenced for a relatively mild crime – consistent with the body of research on incarceration-related stigma (Pager, 2003; Raphael, 2014; Agan and Starr, 2017; Agan and Starr, 2018; Mueller-Smith and Schnepel, 2021; and Rose, 2021). Interestingly, our estimate of the impact of incarceration over a 15-year period shows some heterogeneity over time, suggesting instead that differences in the local context in which offenders are released (rather than offenders' criminal histories) may be an important source of explanation for differences in results between studies.

Finally, 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 that are detrimental to less privileged individuals in the functioning of the courts. 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

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<sup>7</sup> All three studies use random assignment of criminal cases to judges with different severity levels to measure the causal impact of incarceration. In this setting, the 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.

<sup>8</sup> Consequently, an important difference with the samples of other Scandinavian studies is that the trial outcome for the individuals included in our sample depends much less on the severity of the judge than on the result of a blood alcohol test and the law in force at the time of arrest and trial.

<sup>9</sup> To some extent, these results echo the literature on the impact of prison conditions, which has found mixed results to date. See Katz et al. (2003), Bedard and Helland (2004), Chen and Shapiro (2007) for the US, and Mastrobuoni and Terlizzese (2023) for Italy.

by their stakeholders. In fact, we show that standard Difference-in-Differences estimators tend to yield conclusions different from our own.

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.

## **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 became a major cause of convictions<sup>10</sup> and, in some countries, 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*, 72.2% of defendants tried for a drunk-driving crime during that year were sentenced to incarceration. Although the severity of the sentence varied depending on the characteristics of the offense (*e.g.* driver's level of impairment and existence of aggravating circumstances) and the number of prior drunk-driving convictions, the length of the prison sentence remained relatively short: in 95.3% of cases, it was below 60 days. Moreover, while fines and suspensions of the driving license were also frequently imposed on defendants – in 27.6% and 30.9% of trials respectively –, probation and community service were rarely used by judges. 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 as of 1994, offenders suffering from an alcohol abuse problem who received a prison sentence of no more than 60 days could ask the Prison and Probation

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<sup>10</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.



Service to benefit from a *pardon scheme*. As part of this scheme, their custodial sentence could be commuted to a non-custodial sentence, including a two-year probation period,<sup>11</sup> a fine, and mandatory participation in a yearlong rehabilitation program. Under this scheme, offenders had to take a drug causing acute sensitivity to ethanol and to participate in an alcohol treatment program<sup>12</sup> (more information is provided in *Appendix A.1*). This explains why the share of offenders who were actually incarcerated (37.9%) is lower than the share of those who received a custodial sentence (72.2%).<sup>13</sup>

Offenders who did serve a prison sentence benefitted from particularly advantageous conditions of incarceration by American and European standards (Lappi-Seppälä, 2007; Pratt, 2008; Pratt and Eriksson, 2011; Ward et al., 2013) – as is the case in other Nordic countries. These conditions stem from the authorities' wish to ensure that life in prison mirrors life on the outside as much as possible, in order to prepare prisoners for their release and facilitate their reintegration into society.<sup>14</sup> To avoid harmful interactions, note that drunk drivers (as well as other minor offenders) were also 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>15</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 (described above) being reserved for offenders exhibiting such a problem<sup>16</sup> and community

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<sup>11</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>12</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). Offenders were monitored throughout the duration of the treatment and the rest of the probation period. Probation officers were in charge of ensuring that the terms of 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.

<sup>13</sup> Our variable indicating whether or not an individual was incarcerated following a trial is a dummy variable capturing whether an individual has spent at least 10 days in prison in connection with the criminal case investigated as part of that trial – 10 days being the minimum duration of prison sentences requested for a drunk-driving crime.

<sup>14</sup> This so-called *normalization* principle was introduced in the early 1970s.

<sup>15</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.

<sup>16</sup> The only difference with the rehabilitation program implemented after the reform is that, until 2000, 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.

service for offenders who did not. It was substituted to the former sentences at the following rate: 30 hours for 10 to 14 days of imprisonment, 40 hours for 20-30 days, and 60 hours for 40 to 50 days.<sup>17</sup>

As displayed in *Table 1*, the share of offenders who received a custodial sentence dropped significantly after the reform, from 72.2% in 1999 to 14.2% in 2001. Similarly, the share of offenders who were actually incarcerated decreased from 37.8% to 14.3%. In contrast, the share of offenders who were placed on probation by a judge rose from 0.7% to 59.1%. 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 particularly good, 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 a 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 check applicants' criminal record during the hiring process.

It should be noted that, 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).

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<sup>17</sup> 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).

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

#### 3.1. Anticipation

In order to determine how to measure the impact of the reform, we start by analyzing how the reform was implemented using administrative data on the universe of drunk-driving crimes committed or tried around the time of the reform. *Figure 1* shows the evolution of the following four indicators between 1998 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). For the years 1998, 1999, and 2001, vertical lines are only drawn for comparison purposes.<sup>18</sup>

The evolution of these indicators reveals striking breaks and discontinuities in the three-month transition period that followed the signature of the reform and preceded its entering into force. First, 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>19</sup> This is the case despite the fact that there was no similar variation in the number of alleged crimes resulting in a trial committed in the preceding months (*Figure 1.a*), or in the number of cases tried during the same period in adjacent years, 1998, 1999, and 2001 (*Figure 1.b*).<sup>20</sup> This suggests that stakeholders (courts of justice and/or defendants) anticipated the change in legislation and that, as a consequence, a large share of trials were postponed until after the reform.<sup>21</sup>

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<sup>18</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 evolutions observed in the data.

<sup>19</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.

<sup>20</sup> In *Appendix A.2*, 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.

<sup>21</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.

Second, the shares of drunk drivers who received a custodial sentence (*Figure 1.c*) and 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>22</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 35.4% on average in the three weeks preceding the date of the reform – representing a 48.1% 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>23</sup>

These patterns show that stakeholders (defendants, courts, or both) changed their behavior in anticipation of the reform. 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 impending reform faced the risk of being incarcerated if tried before the reform, while they merely faced the risk of being placed on probation if tried after. The same feature also guarantees that defendants tried prior to the passing of a law lowering the sanction for the crime they were convicted of may request that their case be retried if they are still in prison when the reform enters into force.<sup>24</sup>

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<sup>22</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>23</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 February 2024). 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.

<sup>24</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.

### 3.2. Selection

Going further, we investigate the characteristics of the defendants whose trial did take place during the transition period. 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.<sup>25</sup> 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 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.

We find evidence suggesting that the identity of the individuals who had their case postponed was not random. Indeed, while we find no indication of any change in the nature of the cases tried in the first, third,<sup>26</sup> and fourth quarters of 1999 and 2000 (columns 1, 3 and 4), we do find 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

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<sup>25</sup> There were 82 district courts at the time.

<sup>26</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.

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 17.5% 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. We find no such evidence. In particular, we do not observe any change in the number of crimes and, in particular, drunk-driving crimes committed by offenders tried during the transition period (the reform systematically applying 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).<sup>27</sup>

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, these observations indicate that the way drunk-driving cases were handled in district courts during the transition period is likely to have 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 some traditional quasi-experimental estimators (*e.g.*, Difference-in-Differences)

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<sup>27</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.

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>28</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 opt for an instrumental variable approach that exploits exogenous variation in the probability of receiving a custodial sentence based on how close to the reform offenders committed their crime. This variation comes from the combination of two features of the justice system. First, 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. Second, the processing of criminal cases in district courts generates a time gap of several months between the moment a crime is committed and the moment the corresponding decision of justice is rendered – as further documented below. Together, these features ensure that the closer to the reform a crime was committed, the more likely the offender was to be tried after the reform under the new law, and therefore to avoid prison.

*Figure 2* provides visual support for this new instrument. 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-

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<sup>28</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 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 such 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.

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>29</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 one indicates 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 (*Figure 2.a*).<sup>30</sup> The second indicator shows that this 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, 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 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*).

## 4.2. Sampling strategy<sup>31</sup>

Building on the above observations, our identification strategy relies on the comparison of individuals who committed their drunk-driving crime at different dates *prior* to the signature of the reform, and therefore faced varying probabilities of incarceration.

A natural sampling approach would consist in focusing on offenders tried for a drunk-driving crime committed in the 12-month period preceding the signing of the law (between April 4, 1999 and

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<sup>29</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). These manipulations are not driving the evolutions observed in the data.

<sup>30</sup> Note that this time gap was almost entirely driven by the case processing time in district courts. It can also be noted that this gap tends to increase slightly as crimes are committed closer to the reform. As discussed in *Section 4.4*, however, the implications for the study design and interpretation of our results are limited.

<sup>31</sup> *Appendix A.3* provides detailed information on the administrative datasets used in this paper.



April 3, 2000), since our instrument exhibits no variation among individuals tried for a drunk-driving crime committed earlier on (*Figure 2*). However, results obtained on this sample risk being biased if our outcomes of interest exhibit a trend, *i.e.* if they increase (or decrease) with offenders' date of crime. We actually document the existence of such trends among individuals who were arrested for drunk driving 13 to 36 months before the law was signed, and were therefore unaffected by the reform (*Appendix A.4*).<sup>32</sup> For instance, individuals who were arrested for a drunk-driving crime earlier on tend to commit fewer crimes after 5, 10 and 15 years than those arrested later (*Figures A.4.1.a, b, c*). We also observe such trends for labor market outcomes (*Figures A.4.2.a, b, c*).

In order to control for these trends in our outcome variables, we also include in our sample individuals tried for a drunk-driving crime committed 13 to 36 months prior to the reform. Hence, our final sample includes all offenders tried for a drunk-driving crime in the 36-month period prior to the signing of the law (between April 4, 1997 and April 3, 2000). It includes 12,295 cases, corresponding to 11,453 distinct defendants.<sup>33</sup>

*Table 3* describes the characteristics of the defendants included in our sample. They are predominantly males in their late thirties. Close to 63.6% of them were holding some type of job at the end of the year preceding the date of their crime, 72.4% received social benefits in the 12-month period preceding their crime. On average, defendants received transfers for 22.7 weeks, with unemployment-related benefits alone accounting for 13.4 weeks. In total, 21.4% of the defendants had already had at least one conviction for a drunk-driving crime in the previous five years (11.2% in the previous two years). Few of them were in a relationship (29.2%), and defendants born abroad and descendants of immigrants represented 5.0% and 0.3% 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>32</sup> It can be noted that a 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. However, restricting the sample to individuals tried for a drunk-driving crime committed 0 to 24 months before the signing of the 2000 reform yields similar conclusions. Our preferred approach is to include individuals arrested 24 to 36 months prior to the signing of the reform in our sample, as this allows us to more accurately estimate the effect of the trend on the outcome variables. *Appendix A.4* provides visual evidence of this.

<sup>33</sup> We only exclude those under the age of criminal responsibility (18 at the time), as special sanctions apply to them. They represented less than 1% of all drunk-driving offenders tried in 1999.

### 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, taking the value 0 for individuals who committed their crime on April 4, 1997 and increasing with time (the unit for this variable is 100 days);<sup>34</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 36 months prior to the reform); and  $X_i$  is a vector including all variables in the conditioning set detailed in *Appendix A.3*. More specifically, we control for various trial characteristics, such as 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>35</sup> past criminal activity (the number of convictions for other drunk-driving crimes, non-road traffic crimes and incarceration spells in the 5-year period preceding their crime), marital status, highest educational achievement, employment status, annual earnings, etc. 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 82 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 36 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 trend in the 12-month period preceding it. It can already be noted that a graphical analysis of the

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<sup>34</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 further 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>35</sup> We use Statistics Denmark's typology: "immigrants", "descendants of immigrants", or "rest of the population."

evolution of our outcome variables suggests that  $\delta^{RF}$  is different from 0 for drunk-driving offenders tried in district courts (but, reassuringly, not for those whose drunk-driving crime did not warrant appearing before a judge) (*Appendix A.4*).

Standard errors are clustered at the district court and individual levels.

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*

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 being incarcerated.<sup>36</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

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<sup>36</sup> Our variable indicating whether or not an individual was incarcerated is a dummy variable capturing whether the individual has spent at least 10 days in prison – 10 days being the minimum duration of prison sentences requested for a drunk-driving crime.

fixed effects (column 1), dummy variables indicative of the nature of the drunk-driving charge (column 2), information about the criminal case (column 3), and defendant characteristics (column 4).

As expected, we find that, 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 7.3 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 the instrument is independent of both defendant and case characteristics (given equation (2), once a time trend, period, month-of-crime and district court fixed effects are controlled for).

#### *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 (given equation (2), 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 never statistically significant, indicating that the independence assumption is likely to be met. 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, bringing additional evidence that the independence assumption holds.

The exclusion restriction implies that the proposed instrument,  $P_i * T_i$ , does not have any direct impact on our outcome variables (defendants' crime and labor outcomes up to ten years after the trial), but only through its effect on the likelihood of offenders being incarcerated. The first obvious concern is that the risk of recidivism and/or prospects of employment may vary with defendants' date of crime (*Appendix A.4*). However, as discussed in *Section 4.2*, we are able to mitigate this issue by

estimating the effect of these natural trends using individuals tried for a drunk-driving offense committed 13 to 36 months before the reform was signed. A second 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 criminal behavior. However, as displayed in *Figure 2*, the waiting time is actually positively correlated with the probability of receiving a *non*-custodial sentence. Hence, under the assumption that increased waiting time does foster recidivism, our results would therefore *underestimate* (not overestimate) the impact of incarceration relative to that of probation.<sup>37</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 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*. All first-stage coefficients are positive, except one, for female offenders. However, as the size of this subgroup is relatively limited (1,086 observations) and the coefficient is small (0.009) and not statistically significant at the 10% level (the associated standard error is 0.032), the overall picture suggests that problems of non-monotonicity are probably limited.

## 5. Results

### 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>38</sup> In *Figures 3 to 5*, we report on the differential

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<sup>37</sup> However, two factors suggest that the importance of this concern is probably limited. 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). 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. Roodman (2020) reaches similar conclusions in his review of the evidence on the subject.

<sup>38</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.

effect of the two types of sentences on different crime outcomes (all crimes, drunk-driving crimes, non-drunk-driving crimes, property crimes, violent crimes, and other non-drunk-driving crimes), as measured by our IV estimates. To do so, the following two *cumulative* indicators are computed every three months for each type 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 top panels; and the number of convictions by time  $t$  (the *intensive* margin), displayed in bottom panels. A subset of the coefficients is displayed in *Tables 5.A to 6.B*.

Looking at their differential impact on crime in general, we see that custodial sentences significantly increase offenders' involvement in subsequent criminal activity compared to non-custodial sentences (*Figure 3*). While we do not find any impact on the probability of committing another crime (*Figure 3.a*), incarceration significantly and persistently increases the average number of crimes committed (*Figure 3.b*). Fifteen years after the trial, incarcerated offenders record an average of 1.68 additional convictions (statistically significant at the 5% level), representing a 53.7% increase at the sample mean. Because custodial and non-custodial sentences appear to be equally effective in preventing individuals from being convicted again, the effect on the intensive margin is in fact mechanically larger on offenders who were reconvicted at least once (about 40% larger). Hence, our results suggest that, overall, although incarceration does not increase the number of reoffenders relative to probation, it intensifies their subsequent criminal activities. This is consistent with the idea that custodial sentences can have a criminogenic effect (Aizer and Doyle, 2015, Mueller-Smith, 2015), even for offenders exhibiting relatively low proclivity for criminal behavior, such as drunk drivers.<sup>39</sup>

Breaking down the effects on overall crime into effects on drunk-driving crimes and non-drunk-driving crimes (*Figure 4*), we find no differential impact on subsequent drunk-driving convictions: the two sanctions appear to be equally effective in preventing offenders from being reconvicted for a drunk-driving crime (*Figure 4.a*), and their impact on the average number of subsequent convictions for drunk driving is also similar (*Figure 4.c*). In both cases, point estimates are relatively small in

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<sup>39</sup> The timing of the effects is 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. Then, once the incapacitation effect is no longer at play, the negative impact of incarceration after release accumulates over time, up to 15 years after the initial decision of justice. 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.

magnitude and systematically fail to be statistically significant at the 5% level. This lack of effect may come at least partly from the fact that, among compliers, the number of defendants with a serious alcohol problem who were required to participate in an alcohol rehabilitation program is limited because of the pardon scheme discussed in *Section 2.1*. 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*). The magnitude of this effect is large: incarcerated offenders show an average of 1.66 additional convictions after 15 years – representing a 69.7% increase at the sample mean.

A closer look at the nature of the non-drunk-driving crimes committed highlights the importance of economically motivated crimes (burglary, fraud, handling of stolen goods, theft, robbery, shoplifting, etc.). In *Figure 5*, we report on the relative impact of custodial and non-custodial sentences on subsequent convictions for violent (left panel), property (central panel), and other non-drunk-driving crimes (right panel) taken separately, leaving drunk-driving crimes out of the analysis. We observe a strong and particularly significant rise in the number of convictions for property crimes, which increases by 0.88 (statistically significant at the 1% level – representing a 144.3% increase at the sample mean). 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 non-drunk-driving 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.

Next, we study the relative effect of incarceration on offenders who had never been incarcerated before (referred to as “*first-time*” offenders) and on offenders who had already been to prison (referred to as “*repeat*” offenders) – each group representing approximately half of the total sample. As *Figure 6* shows, custodial sentences have a particularly adverse effect on first-time offenders but a limited one on repeat offenders, compared to non-custodial ones. For first-time offenders, custodial sentences increase the probability of being convicted again and the number of reconvictions in the 15 years following the first verdict. For repeat offenders, incarceration has no impact on the average number of crimes but seems to reduce the recidivism rate. These findings are broadly consistent with the idea that incarceration has a fixed cost for offenders, which materializes for instance through the stigma associated with it. In *Appendix A.6*, we report heterogeneous effects on property crimes and reach

identical conclusions. It is interesting to note that these results are radically different from those found in Jordan et al. (2023). This suggests that individuals' criminal history may not be the main missing factor in explaining the differences in results observed between studies.

We perform several placebo tests to check the robustness of our main results. First, we examine whether the instrument is correlated with the pretrial values of our outcome variables. The results of these tests, reported in all figures and tables, show that our results cannot be explained by differential pre-trends. Secondly, we seek to understand whether the results can be partly explained by the way in which we have estimated the natural trends of our outcomes (as highlighted in *Appendix A.4*). To do this, we re-estimate our results by restricting our sample to individuals tried for drunk-driving crimes committed in the 24-month (rather than 36-month) period prior to the signing of the reform (*Appendix A.7*), and find similar results.

It is interesting to note that the naive OLS approach yields different results than those just discussed. As displayed in *Tables 5.A* and *5.B*, the standard OLS estimates suggest that custodial sentences tend to decrease the total number of convictions at 15 years (-0.15, statistically significant at the 10% level), as well as the number of convictions for non-drunk-driving crimes (-0.21, statistically significant at the 1% level). OLS estimates also suggest that custodial sentences tend to increase the number of convictions for drunk driving at 15 years (+0.05, statistically significant at the 5% level). It is also worth noting that simple Difference-in-Differences estimates<sup>40</sup> yield different results (*Appendix A.8*): they show no impact on the intensive margin and a negative impact on the extensive margin (but only in the long term).<sup>41</sup>

## 5.2. Impact on labor market outcomes

We now turn to the relative impact of custodial and non-custodial sentences on offenders' labor market outcomes, an important dimension of offenders' live after release. To do so, we compute the following outcomes every quarter or year (depending on data availability) from the date when the drunk-driving case was settled in court: a) the probability of having a job in year  $t$ ;<sup>42</sup> b) the cumulative

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<sup>40</sup> RF estimates are obtained by regressing the outcome variables on a dummy variable indicative of whether or not a defendant was tried before the reform (the DiD instrument), 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 36 months prior to the reform), and  $X_i$ , a vector including all variables in the conditioning set detailed in *Appendix A.3*. IV estimates are obtained by instrumenting whether or not a defendant was incarcerated by the DiD instrument.

<sup>41</sup> It can be noted that Andersen's results suggest that the combination of DiD and matching may at least partially mitigate the consequences of the selection problem described in Section 3.2 (Andersen, 2015).

<sup>42</sup> Measured by Statistics Denmark at the end of the month of November.



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 7*. 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 incarceration are less likely to be employed after serving their sentence than those who only received a non-custodial sentence (top panel of *Figure 7*). As a consequence, they are more likely to rely on unemployment-related benefits (middle panel of *Figure 7*) and have lower earnings (bottom panel of *Figure 7*). This result holds for almost every year following the decision of justice – except for a few years in the second part of the study 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 32 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 86.5% decrease at the sample mean.<sup>43</sup> As a consequence, incarceration represents a cumulative earnings loss of 620,680 kroner after ten years (statistically significant at the 5% level), and 602,140 kroner after fifteen (statistically significant at the 1% level) – corresponding respectively to a 42.7% and 32.2% reduction in the sample average. Our findings therefore suggest that incarceration has an important adverse impact on individuals' attachment to the labor market.<sup>44</sup>

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 25 days) and that drunk drivers are generally incarcerated in open prisons, where offenders are allowed to leave the facility during the day to go to work (although the suspension of their driver's license may limit their ability to do so). However, it should be noted that the negative differential effect of incarceration on labor market outcomes materializes only in the medium term, and 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.

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<sup>43</sup> Data availability constraints prevent us from calculating this result beyond 12 years.

<sup>44</sup> Although these results are in contradiction with those obtained in Bhuller et al. (2020), they are of the same order of magnitude.

In fact, the impact of the two sanctions on the probability of having a job is remarkably similar during the first 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).

From the seventh year, the difference between the two types of sanctions decreases. Interestingly, this coincides with the time when the conviction is removed from the criminal record of offenders sentenced to incarceration.<sup>45</sup> However, towards the end of the period, the difference in the probability of having a job becomes more pronounced again. This coincides with the general rise in unemployment from 2009 onwards in the wake of the 2008 economic crisis, suggesting that incarceration 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.* the 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).

Generally speaking, the plot of the curve representing the relative impact of custodial and non-custodial sentences on labor market attachment (*Figure 7.a*) shows similarities with that of the curve representing the effect on property crimes (*Figure 5.e*), suggesting that offenders' post-release difficulties on the job market may be an important mechanism behind the criminogenic effect of incarceration. In particular, the two plots 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'

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<sup>45</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.

labor market attachment slightly precedes the increase in the number of crimes committed.<sup>46</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 ceases from the eighth year.<sup>47</sup>

Finally, as *Figure 8* shows, the negative effect of custodial sentences on labor market attachment is entirely driven by first-time offenders.

We perform the same robustness checks as those described in the previous section and reach similar conclusions (*Appendices A.7* and *A.8*).

## 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 so, 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 in line with their best interest. In the case of the change in the drunk-driving legislation studied here, we show that a large share of the cases which should have been tried in the months preceding the entering into force of the reform (from the moment the law was signed) was actually tried after and that, as a consequence, a group of offenders who should have been tried before the reform was tried

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<sup>46</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>47</sup> 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, therefore, 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), 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. Results are available upon request.

after and 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 nature and 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 has so far tended to suggest that custodial sentences may be more effective overall than non-custodial ones to reduce recidivism in countries with good detention conditions. It also contributes to our understanding of the heterogeneous effects that incarceration can have on offenders, suggesting that the main explanation for the differences in outcomes found in different contexts may not lie in offenders' criminal histories, as recently suggested (Jordan et al., 2023). Being able to measure impact over a 15-year period highlights the existence of some heterogeneity over time and suggests instead that variations in the local context in which offenders are released may be an important source of explanation for the differences between study findings.

In any case, our results provide additional evidence that custodial sentences can have a negative impact on offenders spanning far beyond their period of incarceration, and suggest that accompanying offenders following their release could help mitigate the costs of incarceration for offenders and for society.

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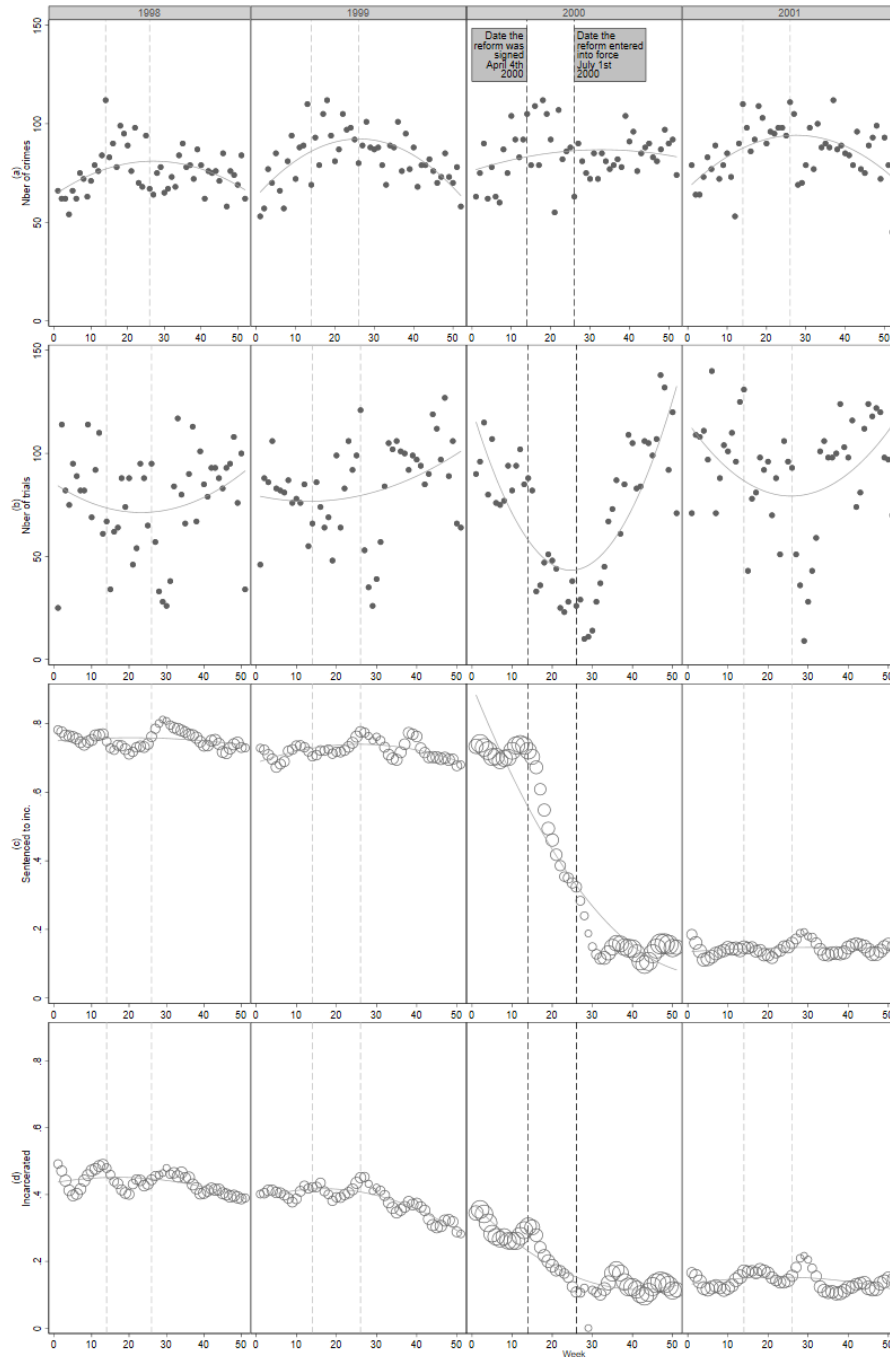


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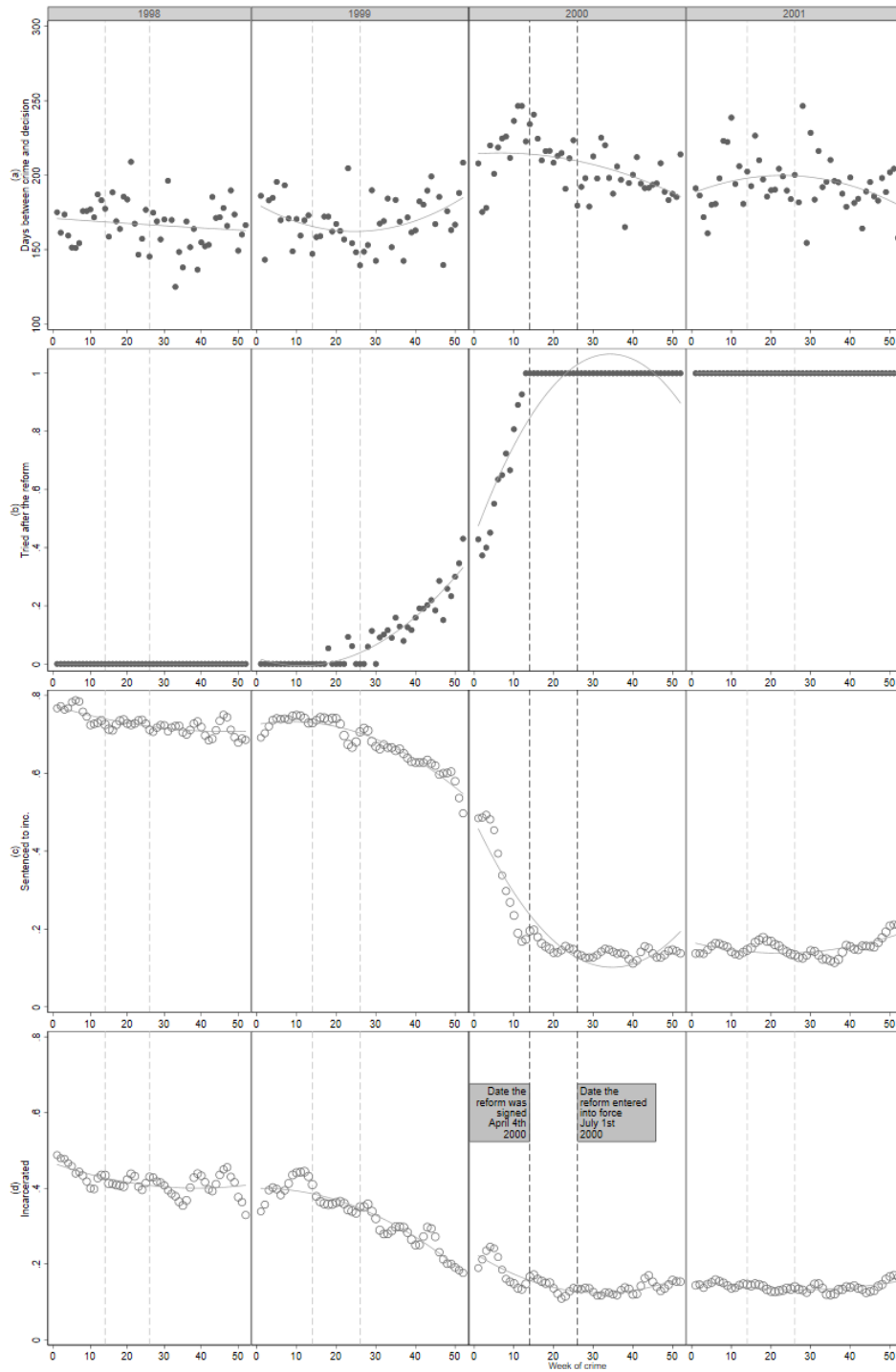
# Figures

**Figure 1:** Implementation of the drunk-driving legislation reform



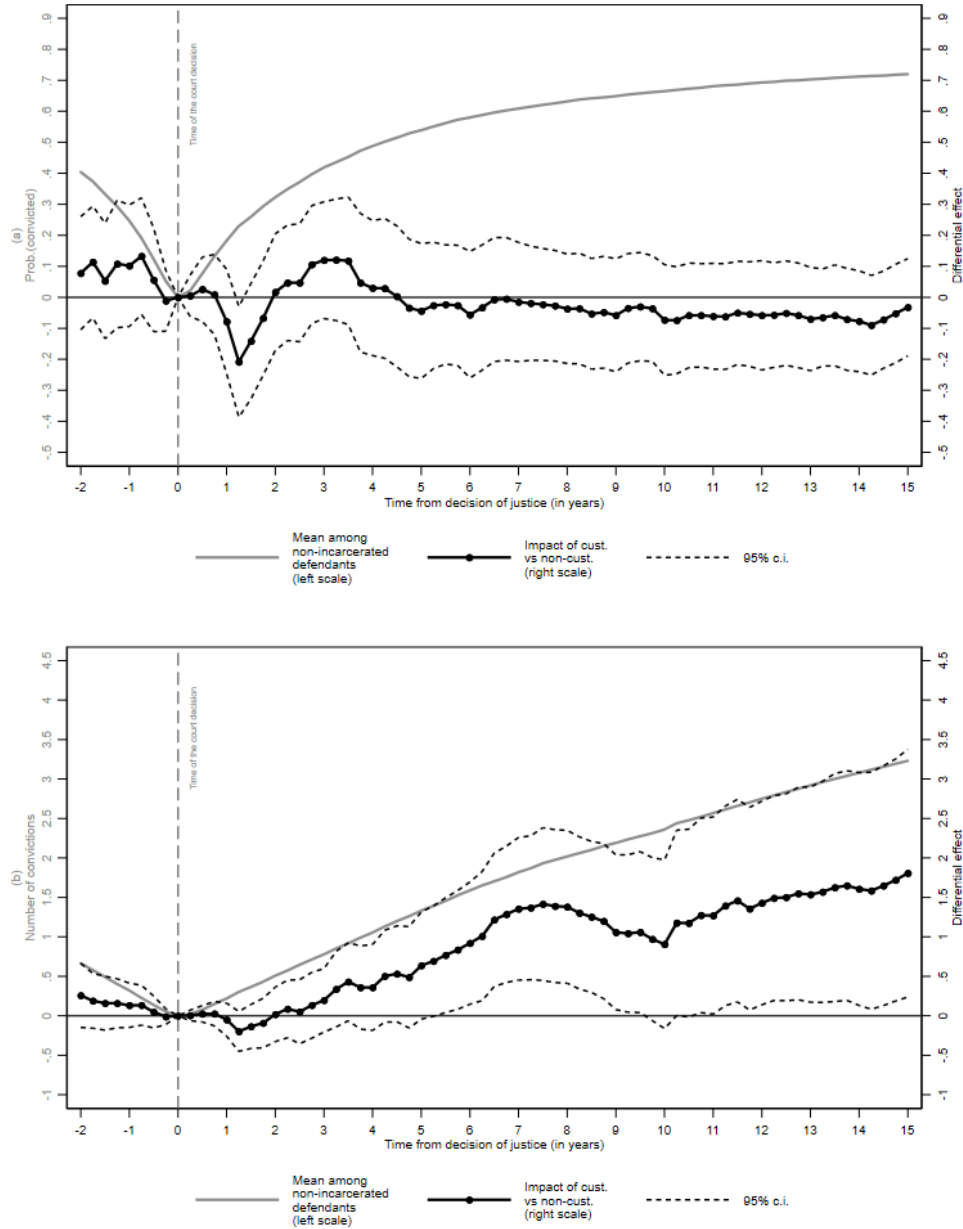
*Notes:* The consequences of the reform are depicted here through the evolution of the following four indicators around the time of the change in legislation: a) the number of drunk-driving crimes resulting in a trial committed every week (top-coded); b) the number of drunk-driving cases tried every 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).

**Figure 2: Motivation for the instrumental variable approach**



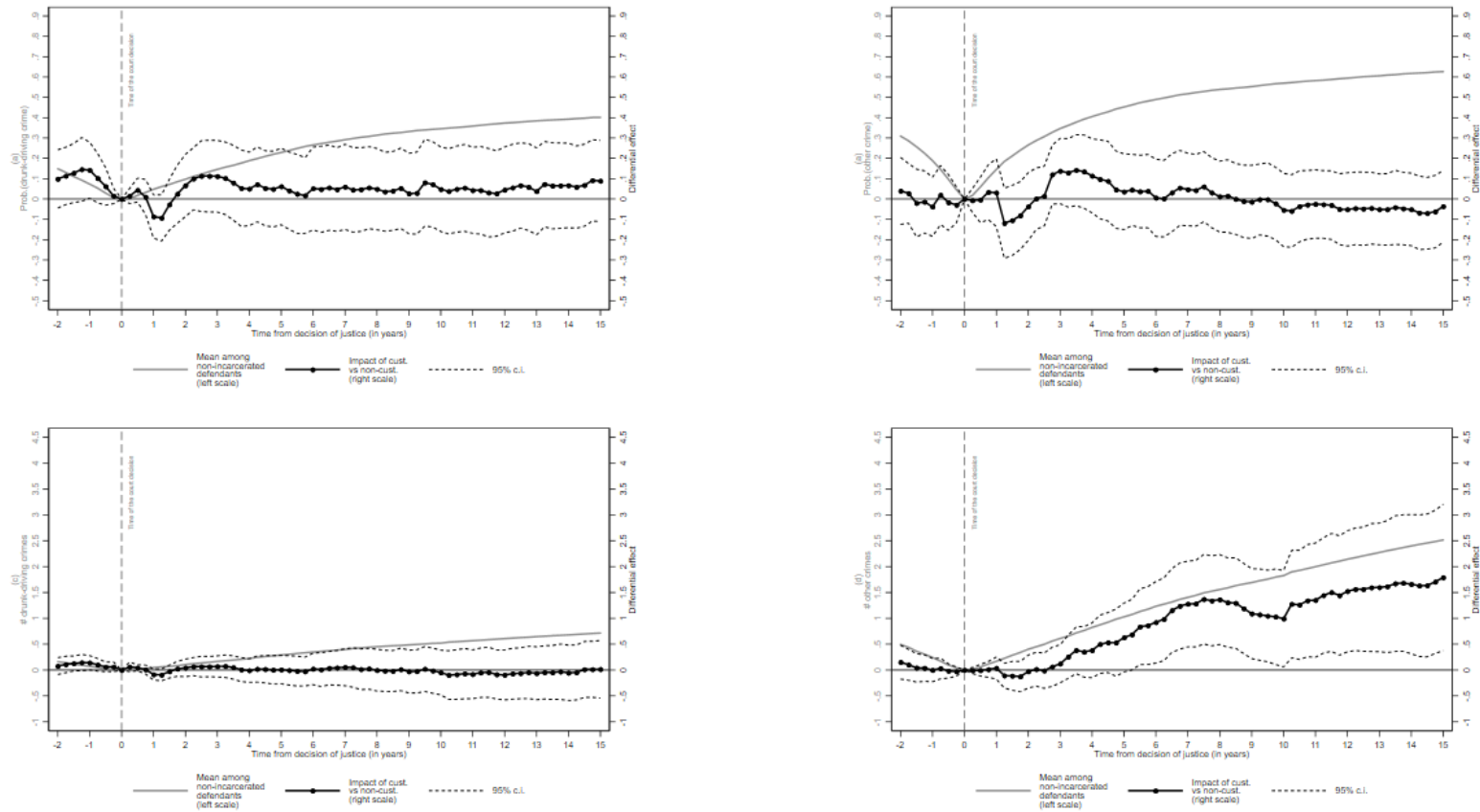
*Notes:* This figure depicts the evolution of the following four indicators around the time of the reform: a) the average time gap between the moment a crime was committed and the moment the decision of justice was rendered by a district court by *week of crime* (top-coded); b) the share of cases tried 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).

**Figure 3:** Impact of custodial vs. non-custodial sentences on crimes



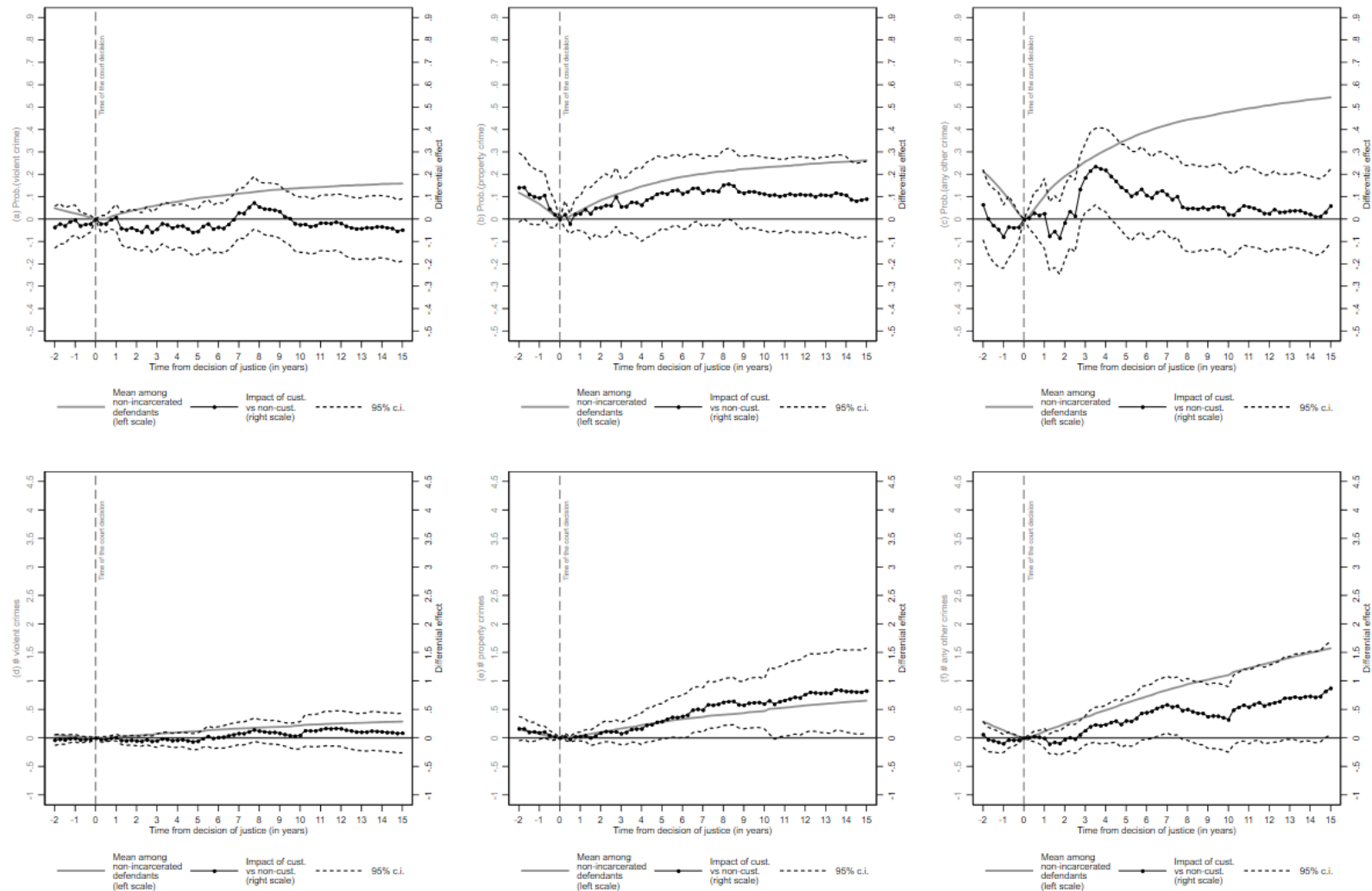
*Notes:* This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) on the following outcomes: a) the probability of being convicted of a post-sentencing crime (any type of crime); b) the number of such convictions. Crime outcomes are measured every 3 months from the date when the drunk-driving case was settled in court. Dashed lines represent the 95% confidence interval.

**Figure 4:** Impact of custodial vs. non-custodial sentences on drunk-driving and other crimes



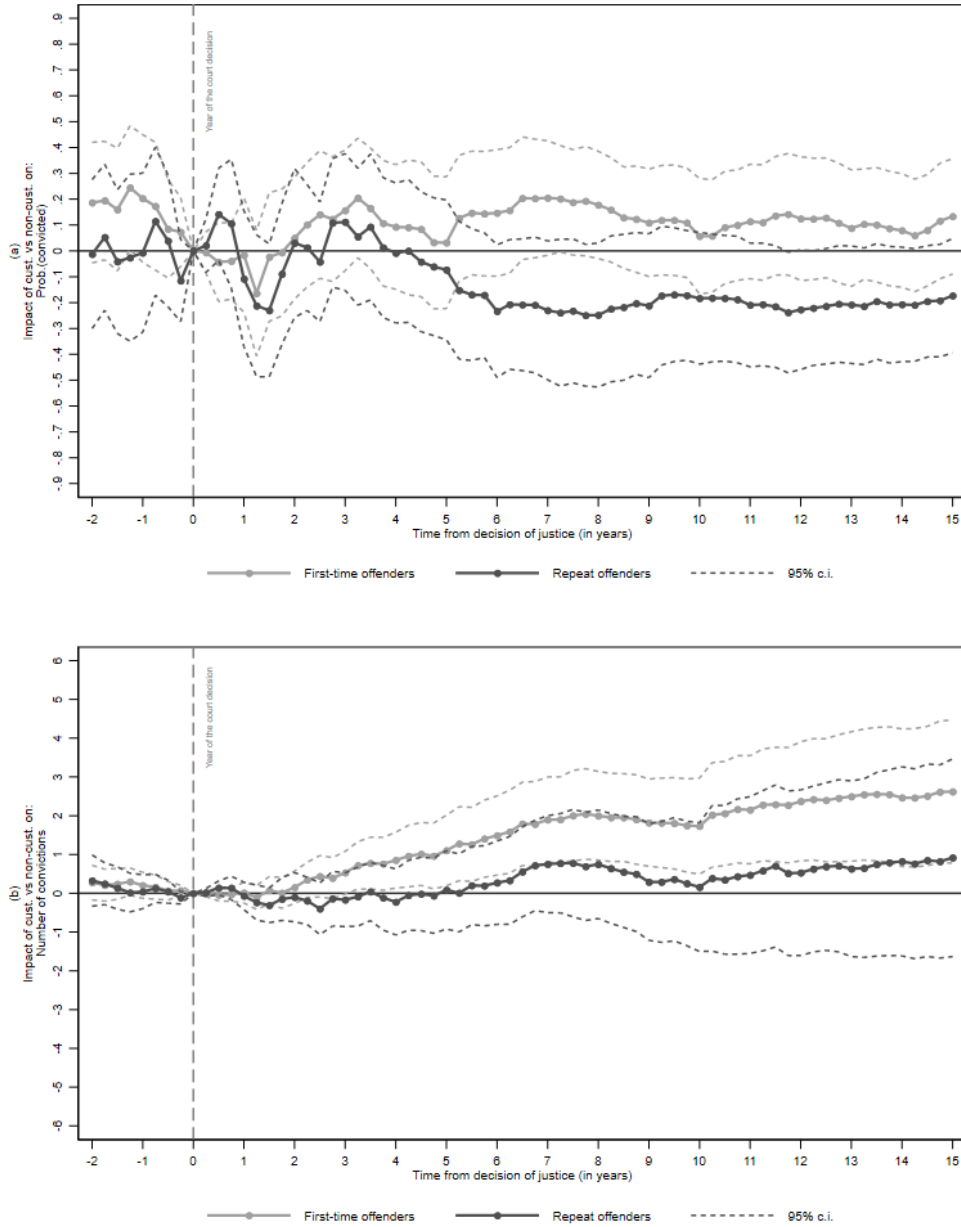
*Notes:* This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) on the following outcomes: a) the probability of being convicted of a drunk-driving crime; b) the probability of being convicted of any non-drunk-driving crime; c) the number of convictions for a drunk-driving crime; and d) the number of convictions for any crime other than drunk driving. 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.

**Figure 5:** Impact of custodial vs. non-custodial sentences on other crimes: violent crimes, property crimes, other non-drunk-driving crimes



*Notes:* This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) at the extensive and intensive margins 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. These two outcomes are measured every 3 months from the date when the drunk-driving case was settled in court.

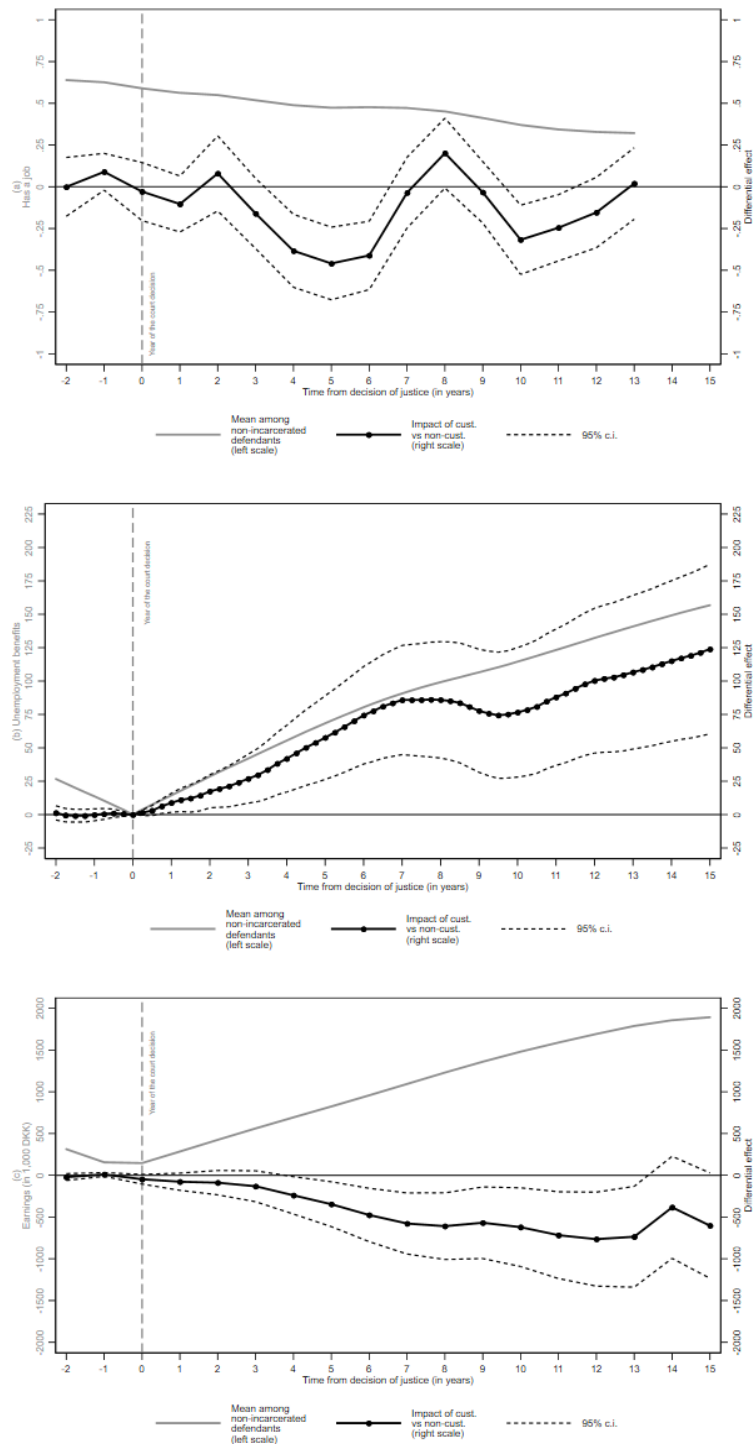
**Figure 6:** Heterogeneous impact of custodial vs. non-custodial sentences on crimes



*Notes:* This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) on the following outcomes for individuals who have never been incarcerated (first-time offenders) and for those who have (repeat offenders): a) the probability of being convicted of a post-sentencing crime (any type of crime); b) the number of such convictions. 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.

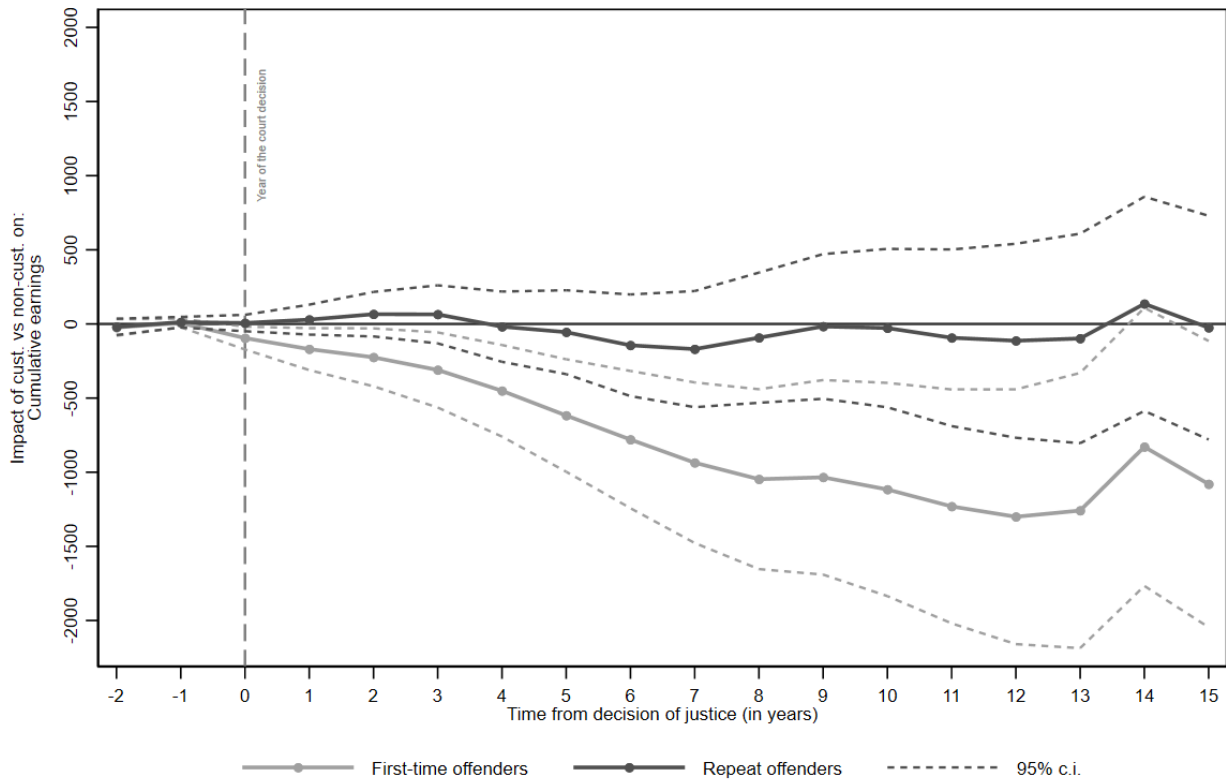


**Figure 7: Impact of custodial vs. non-custodial sentences on labor market attachment**



*Notes:* 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. Data availability constraints prevent us from calculating the impact on outcome a) beyond 12 years.

**Figure 8:** Heterogeneous impact of custodial vs. non-custodial sentences on labor market attachment



*Notes:* This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) on cumulative income (in 1,000 DKK) for individuals who have never been incarcerated (first-time offenders) and for those who have (repeat offenders). We measure the impact on cumulative income every year from the date when the drunk-driving case was settled in court. Dashed lines represent the 95% confidence interval.

## 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	4,232	0.012	0.107	3,631	0.012	0.111	4,631	0.008	0.089
Prison sentences (any)	4,232	0.729	0.445	3,631	0.691	0.462	4,631	0.743	0.437
-Prison, cond.	4,232	0.007	0.084	3,631	0.290	0.454	4,631	0.591	0.492
<i>Length in days (incl. 0s)</i>	4,232	0.196	3.168	3,631	6.371	12.547	4,631	12.591	14.834
-Prison, uncond.	4,232	0.722	0.448	3,631	0.391	0.488	4,631	0.143	0.350
<i>Length in days (incl. 0s)</i>	4,232	18.197	25.429	3,631	11.340	25.216	4,631	6.671	25.963
<i>Length between 1 and 60 days</i>	4,232	0.688	0.464	3,631	0.363	0.481	4,631	0.113	0.317
<i>Length over 60 days</i>	4,232	0.034	0.183	3,631	0.028	0.164	4,631	0.029	0.169
-Incarcerated	4,232	0.379	0.485	3,631	0.196	0.397	4,631	0.139	0.346
Other sentences (any)	4,232	0.258	0.437	3,631	0.304	0.460	4,631	0.256	0.437
-Community work	4,232	0.000	0.022	3,631	0.158	0.365	4,631	0.310	0.463
-Fine	4,232	0.276	0.447	3,631	0.598	0.490	4,631	0.860	0.347
<i>Amount in DKK (incl. 0s)</i>	4,232	1621.60	3131.417	3,631	3433.63	3793.519	4,631	5107.07	3752.804
-Driv. lic. suspended	4,232	0.309	0.462	3,631	0.301	0.459	4,631	0.315	0.465
<i>Length in months (incl. 0s)</i>	4,232	1.857	2.777	3,631	1.806	2.753	4,631	1.888	2.785
Appeal	4,232	0.017	0.128	3,631	0.023	0.149	4,631	0.021	0.144

*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	Whole sample			Year 2000		Weeks 14-26		Weeks 27-39		Weeks 40-52	
	#Obs.	Mean	S.d.	ΔT1		ΔT2-ΔT1		ΔT3-ΔT1		ΔT4-ΔT1	
				Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: All offenders</i>											
Has a job	7,894	0.641	0.480	0.006	0.078	-0.028	0.031	-0.013	0.035	-0.032	0.028
Income	7,894	257.239	140.814	9.420	20.848	-14.811	8.161 *	-18.552	9.936 *	-4.214	9.128
Earnings	7,894	160.268	158.835	9.995	24.429	-26.112	10.176 **	-17.331	12.983	-10.118	11.036
Weeks of social transfers	7,894	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	7,894	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	7,894	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	7,894	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	7,894	5.237	15.503	-5.938	2.899 **	0.389	1.073	-0.716	1.129	-0.178	0.979
Nber of crime in past 5 years	7,894	1.439	2.418	-0.575	0.407	0.054	0.142	0.241	0.165	0.069	0.136
Nber of DD crime in past 5 years	7,894	0.357	0.803	-0.091	0.127	-0.038	0.046	-0.028	0.051	0.046	0.044
Nber of other crime in past 5 years	7,894	1.076	1.973	-0.452	0.328	0.095	0.118	0.258	0.134 *	0.030	0.111
Nber of inc. spell in past 5 years	7,894	0.269	0.748	-0.077	0.123	0.034	0.037	-0.029	0.036	0.023	0.040
<i>Panel B: O+A18ffenders who received a prison sentence (whether it be a conditional or an unconditional one)</i>											
Has a job	5,597	0.638	0.481	0.094	0.099	-0.056	0.042	-0.011	0.040	0.013	0.032
Income	5,597	263.480	136.824	40.377	27.109	-25.229	11.526 **	-20.646	12.248 *	1.570	9.689
Earnings	5,597	164.176	159.080	47.246	32.821	-28.753	13.380 **	-17.158	13.253	-3.413	11.161
Weeks of social transfers	5,597	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	5,597	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	5,597	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	5,597	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	5,597	5.483	15.824	-5.155	3.017 *	0.848	1.439	0.638	1.375	-0.114	1.211
Nber of crime in past 5 years	5,597	1.436	2.427	-0.439	0.528	-0.060	0.176	0.173	0.215	0.117	0.158
Nber of DD crime in past 5 years	5,597	0.424	0.872	-0.087	0.183	0.015	0.062	0.024	0.065	0.046	0.058
Nber of other crime in past 5 years	5,597	1.009	1.883	-0.327	0.398	-0.079	0.139	0.136	0.169	0.075	0.125
Nber of inc. spell in past 5 years	5,597	0.310	0.805	-0.083	0.178	0.086	0.054	-0.004	0.052	0.035	0.054

*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	Whole sample			Instrument	
	#Obs.	Mean	S.d.	Coeff.	s.e.
Female	12,295	0.088	0.284	-0.001	0.005
Juvenile, when the crime is committed	12,295	0.000	0.000	0.000	0.000
Age, when the decision is rendered	12,295	38.227	11.000	-0.148	0.197
<i>Immigration status</i>					
Immigrant	12,295	0.050	0.218	-0.001	0.004
Descendant	12,295	0.003	0.057	-0.001	0.001
Rest of the population	12,295	0.947	0.225	0.002	0.004
<i>Family status</i>					
Single	12,295	0.488	0.500	0.008	0.009
In partnership	12,295	0.292	0.455	-0.007	0.006
Separated	12,295	0.194	0.396	-0.002	0.006
Widow	12,295	0.014	0.117	-0.001	0.002
Unknown	12,295	0.012	0.109	0.002	0.002
<i>Education status</i>					
Primary education	12,295	0.522	0.500	0.009	0.009
Secondary education	12,295	0.377	0.485	-0.003	0.008
Higher education	12,295	0.063	0.243	-0.002	0.004
Unknown	12,295	0.038	0.191	-0.005	0.003
<i>Attachment to the labor market</i>					
Has a job	12,295	0.636	0.481	-0.002	0.008
Earnings	12,295	158.722	156.001	-1.795	2.532
Any social transfers	12,295	0.724	0.447	-0.009	0.008
Weeks of social transfers	12,295	22.732	21.503	0.028	0.392
Weeks of labour market-related transfers	12,295	13.352	18.454	0.283	0.336
Weeks of health-related transfers	12,295	3.341	8.855	-0.184	0.138
Weeks of self-supporting transfers	12,295	0.798	5.266	0.017	0.087
Weeks of pension-related transfers	12,295	5.241	15.492	-0.088	0.267
<i>Criminal priors</i>					
Any crime in past 2 years	12,295	0.215	0.411	-0.005	0.006
<i>Any DD crime in past 2 years</i>	12,295	0.112	0.315	0.001	0.004
Nber of crimes in past 2 years	12,295	0.298	0.642	-0.003	0.010
<i>Nber of DD crime in past 2 years</i>	12,295	0.122	0.358	0.003	0.005
Any crime in past 5 years	12,295	0.313	0.464	-0.004	0.008
<i>Any DD crime in past 5 years</i>	12,295	0.214	0.410	-0.002	0.007
Nber of crimes in past 5 years	12,295	1.419	2.397	-0.023	0.040
<i>Nber of DD crimes in past 5 years</i>	12,295	0.350	0.753	-0.002	0.014

*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: 12,295.

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

**Table 4: First-stage**

	Probability to be incarcerated			
	(1)	(2)	(3)	(4)
Instrument	-0.073*** (0.008)	-0.072*** (0.008)	-0.072*** (0.008)	-0.073*** (0.008)
R-squared	0.050	0.059	0.062	0.072
Mean	0.371			
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 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 the probability for them to be 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: 12,295.

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

**Table 5.A: Impact on crime (extensive margin)**

	All			OLS		RF		IV			
	N	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</i>											
2 years before	12295	0.40	0.49	-0.01	0.01	-0.01	0.01	0.08	0.10	0.08	0.09
Within 5 years	12295	0.55	0.50	0.02	0.01 **	0.00	0.01	-0.07	0.11	-0.04	0.11
Within 10 years	12295	0.67	0.47	0.01	0.01	0.01	0.01	-0.10	0.10	-0.07	0.09
Within 15 years	12295	0.72	0.45	0.00	0.01	0.00	0.01	-0.07	0.09	-0.04	0.08
<i>Panel B: Probability of being convicted again for drunk-driving</i>											
2 years before	12295	0.16	0.36	0.01	0.01	-0.01	0.01	0.10	0.07	0.10	0.07
Within 5 years	12295	0.24	0.43	0.02	0.01 **	0.00	0.01	0.05	0.10	0.06	0.10
Within 10 years	12295	0.36	0.48	0.01	0.01	0.00	0.01	0.02	0.11	0.05	0.11
Within 15 years	12295	0.41	0.49	0.02	0.01 *	-0.01	0.01	0.06	0.10	0.08	0.10
<i>Panel C: Probability of being convicted again for any crime other than drunk-driving</i>											
2 years before	12295	0.30	0.46	-0.02	0.01 **	0.00	0.01	0.04	0.09	0.04	0.08
Within 5 years	12295	0.45	0.50	0.00	0.01	0.00	0.01	0.01	0.10	0.04	0.10
Within 10 years	12295	0.57	0.50	0.00	0.01	0.00	0.01	-0.09	0.10	-0.05	0.09
Within 15 years	12295	0.63	0.48	0.00	0.01	0.00	0.01	-0.07	0.10	-0.04	0.09
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											90

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

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

**Table 5.B:** Impact on crime (intensive margin)

	All			OLS		RF		IV			
	N	Mean	S.d.	(1)		(2)		(3)		(4)	
				Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Total number of convictions</i>											
2 years before	12295	0.65	1.02	-0.04	0.02 *	-0.02	0.01	0.26	0.22	0.26	0.21
Within 5 years	12295	1.33	1.83	0.00	0.04	-0.05	0.03 *	0.54	0.40	0.64	0.35 *
Within 10 years	12295	2.35	3.00	-0.05	0.07	-0.07	0.04 *	0.74	0.62	0.91	0.55 *
Within 15 years	12260	3.13	3.99	-0.15	0.08 *	-0.12	0.05 **	1.40	0.86	1.68	0.75 **
<i>Panel B: Number of drunk-driving convictions</i>											
2 years before	12295	0.17	0.41	0.01	0.01	-0.01	0.01	0.08	0.08	0.08	0.08
Within 5 years	12295	0.31	0.62	0.04	0.01 ***	0.00	0.01	-0.02	0.15	0.01	0.14
Within 10 years	12295	0.55	0.90	0.05	0.02 **	0.00	0.02	-0.10	0.23	-0.05	0.22
Within 15 years	12260	0.73	1.13	0.05	0.02 **	0.00	0.02	-0.07	0.27	0.01	0.27
<i>Panel C: Number of convictions other than drunk-driving convictions</i>											
2 years before	12295	0.47	0.87	-0.04	0.02 ***	-0.01	0.01	0.16	0.18	0.15	0.17
Within 5 years	12295	1.01	1.60	-0.04	0.04	-0.05	0.03 *	0.57	0.37	0.63	0.34 *
Within 10 years	12295	1.78	2.65	-0.11	0.05 *	-0.07	0.04 **	0.88	0.54	1.00	0.48 **
Within 15 years	12260	2.38	3.53	-0.21	0.07 ***	-0.12	0.05 **	1.46	0.77 *	1.66	0.67 **
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										90	

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



**Table 6.A: Impact on non-drunk-driving crimes, decomposition (extensive margin)**

	All			OLS		RF		IV			
	N	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 crime violent</i>											
2 years before	12295	0.05	0.21	-0.01	0.00 *	0.00	0.00	-0.03	0.05	-0.04	0.05
Within 5 years	12295	0.09	0.29	-0.01	0.01	0.00	0.00	-0.06	0.06	-0.06	0.05
Within 10 years	12295	0.14	0.35	0.00	0.01	0.00	0.00	-0.03	0.07	-0.02	0.06
Within 15 years	12295	0.16	0.37	0.00	0.01	0.00	0.01	-0.07	0.08	-0.06	0.07
<i>Panel B: Probability of being convicted again for a property crime</i>											
2 years before	12295	0.12	0.32	0.00	0.01	-0.01	0.01 *	0.14	0.08 *	0.14	0.08 *
Within 5 years	12295	0.17	0.38	0.00	0.01	-0.01	0.01	0.10	0.08	0.12	0.08
Within 10 years	12295	0.23	0.42	0.01	0.01	-0.01	0.01	0.09	0.09	0.11	0.08
Within 15 years	12295	0.26	0.44	0.00	0.01	-0.01	0.01	0.07	0.09	0.09	0.09
<i>Panel C: Probability of being convicted again for any other crime (excluding drunk driving)</i>											
2 years before	12295	0.20	0.40	-0.02	0.01 **	0.00	0.01	0.07	0.08	0.07	0.08
Within 5 years	12295	0.35	0.48	-0.01	0.01	-0.01	0.01	0.11	0.11	0.13	0.10
Within 10 years	12295	0.47	0.50	-0.01	0.01	0.00	0.01	0.00	0.10	0.02	0.10
Within 15 years	12295	0.54	0.50	-0.01	0.01	0.00	0.01	0.03	0.09	0.05	0.08
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.

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

**Table 6.B:** Impact on non-drunk-driving crimes, decomposition (intensive margin)

	All			OLS		RF		IV			
	N	Mean	S.d.	(1)		(2)		(3)		(4)	
				Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Number of violent crimes</i>											
2 years before	12295	0.05	0.21	-0.01	0.00 *	0.00	0.00	-0.03	0.05	-0.04	0.05
Within 5 years	12295	0.12	0.39	-0.01	0.01	0.00	0.01	-0.06	0.08	-0.06	0.07
Within 10 years	12295	0.22	0.64	0.00	0.01	0.00	0.01	0.04	0.14	0.05	0.12
Within 15 years	12260	0.27	0.73	0.00	0.01	0.00	0.01	-0.02	0.17	0.00	0.15
<i>Panel B: Number of property crimes</i>											
2 years before	12295	0.15	0.43	0.00	0.01	-0.01	0.01	0.16	0.11	0.16	0.11
Within 5 years	12295	0.28	0.73	0.01	0.02	-0.02	0.01 *	0.27	0.16 *	0.29	0.16 *
Within 10 years	12295	0.47	1.16	0.01	0.03	-0.04	0.02 ***	0.56	0.23 **	0.60	0.22 ***
Within 15 years	12260	0.61	1.46	-0.01	0.03	-0.06	0.02 ***	0.83	0.30 ***	0.88	0.28 ***
<i>Panel C: Number of other crimes (excluding drunk driving)</i>											
2 years before	12295	0.27	0.60	-0.03	0.01 ***	0.00	0.01	0.07	0.11	0.06	0.11
Within 5 years	12295	0.59	1.01	-0.04	0.02 **	-0.02	0.02	0.28	0.24	0.30	0.22
Within 10 years	12295	1.06	1.55	-0.10	0.03 ***	-0.02	0.02	0.26	0.33	0.32	0.29
Within 15 years	12260	1.46	2.08	-0.19	0.04 ***	-0.05	0.03 *	0.56	0.47	0.69	0.41 *
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.  
Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

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

	All			OLS		RF			IV						
	N	Mean	S.d.	(1)		(2)			(3)		(4)				
				Coeff.	s.e.		Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.			
<i>Panel A: Has a job</i>															
2 years before	12295	0.63	0.48	0.00	0.01		0.00	0.01	0.03	0.10	0.00	0.09			
Within 5 years	12295	0.46	0.50	-0.04	0.01	***	0.03	0.01	***	-0.46	0.11	***	-0.46	0.11	***
Within 10 years	12293	0.37	0.48	-0.02	0.01	**	0.02	0.01	***	-0.35	0.10	***	-0.32	0.11	***
<i>Panel B: Cumulative number of weeks spent on unemployment-related benefits</i>															
2 years before	12295	27.69	34.61	0.44	0.21	**	-0.14	0.20		-4.49	8.08		1.89	2.69	
Within 5 years	12295	73.27	82.30	11.63	1.29	***	-4.13	1.14	***	47.68	19.23	**	56.41	16.06	***
Within 10 years	12295	123.03	140.99	19.79	2.25	***	-5.59	1.81	***	62.41	29.78	**	76.38	24.99	***
Within 15 years	12295	166.41	188.88	22.68	2.99	***	-9.13	2.40	***	106.85	38.80	***	124.68	32.50	***
<i>Panel C: Cumulative earnings (in 1,000 DKK)</i>															
2 years before	12295	314.06	297.08	0.68	1.93		1.49	1.55		21.28	64.27		-20.32	20.87	
Within 5 years	12295	811.39	822.45	-40.60	12.75	***	25.31	9.55	***	-316.61	182.36	*	-345.61	137.25	**
Within 10 years	12295	1454.18	1491.56	-75.92	25.82	***	45.45	16.97	***	-609.21	297.26	**	-620.68	240.54	***
Within 15 years	12289	1872.70	2005.23	-76.52	35.96	**	44.12	23.52	*	-626.70	377.16	*	-602.14	321.87	*
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. Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## Appendix

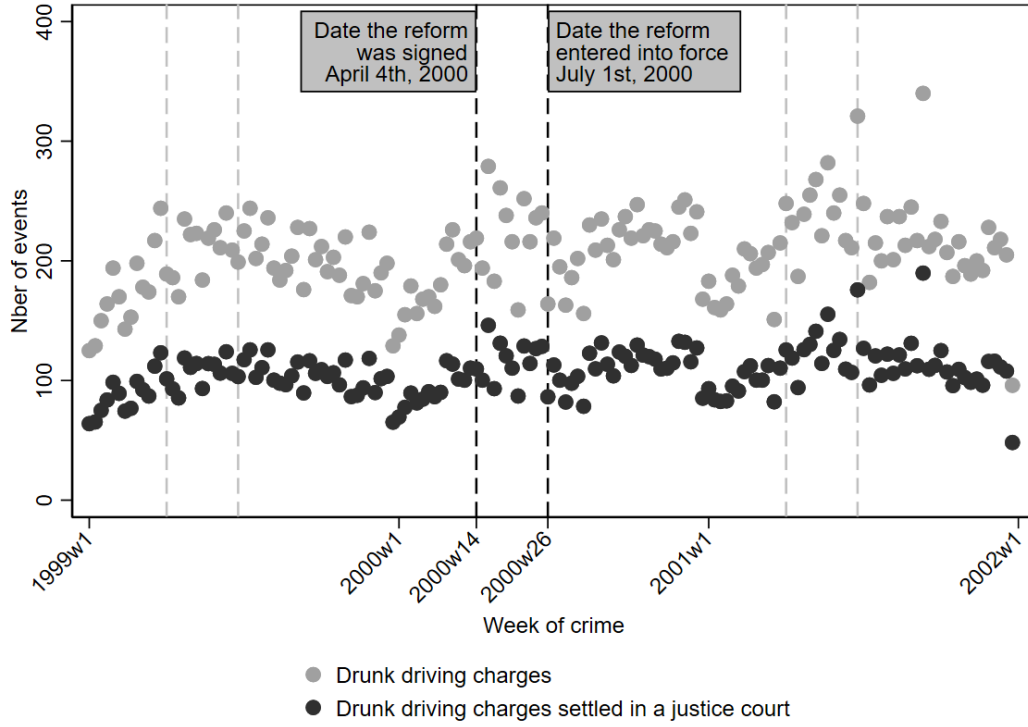
**Appendix A.1.** Pardon scheme for drunk drivers with “a strong need for rehabilitation” offered prior to the 2000 reform

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.** Evolution of the number of charges and trials



### Appendix A.3. Data sources<sup>49</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>50</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>49</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.

<https://www.dst.dk/extranet/forskningvariabellister/Oversigt%20over%20registre.html> (accessed in February 2024).

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

<sup>50</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 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 crimes. In order to do so, we calculate whether or not individuals were convicted again of another 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 drunk-driving crimes and of any non-drunk-driving crimes. In both cases, we compute the number of such crimes they committed. We measure the impact of the reform on these outcomes from three months to 15 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 15 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 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 incarceration spells in the 5-year period preceding their crime, as well as the number of convictions for other drunk-driving crimes and other crimes), marital status,

highest educational achievement, employment status, annual earnings, and reliance on social transfers.

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



## VARIABLES DESCRIPTION

### A. Conditioning set

Variables	<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 February 2024): <a href="https://www.dst.dk/da/Statistik/dokumentation/nomenklaturer/overtraedelsestyper-i-kriminalstatistikken">https://www.dst.dk/da/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.
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: <ul style="list-style-type: none"> <li>- Immigrants</li> <li>- Descendants of immigrants</li> <li>- Unknown status</li> <li>- Rest of the population</li> </ul> Variable source: information on the immigration status of the offender was retrieved from the <i>IE_TYPE</i> variable.

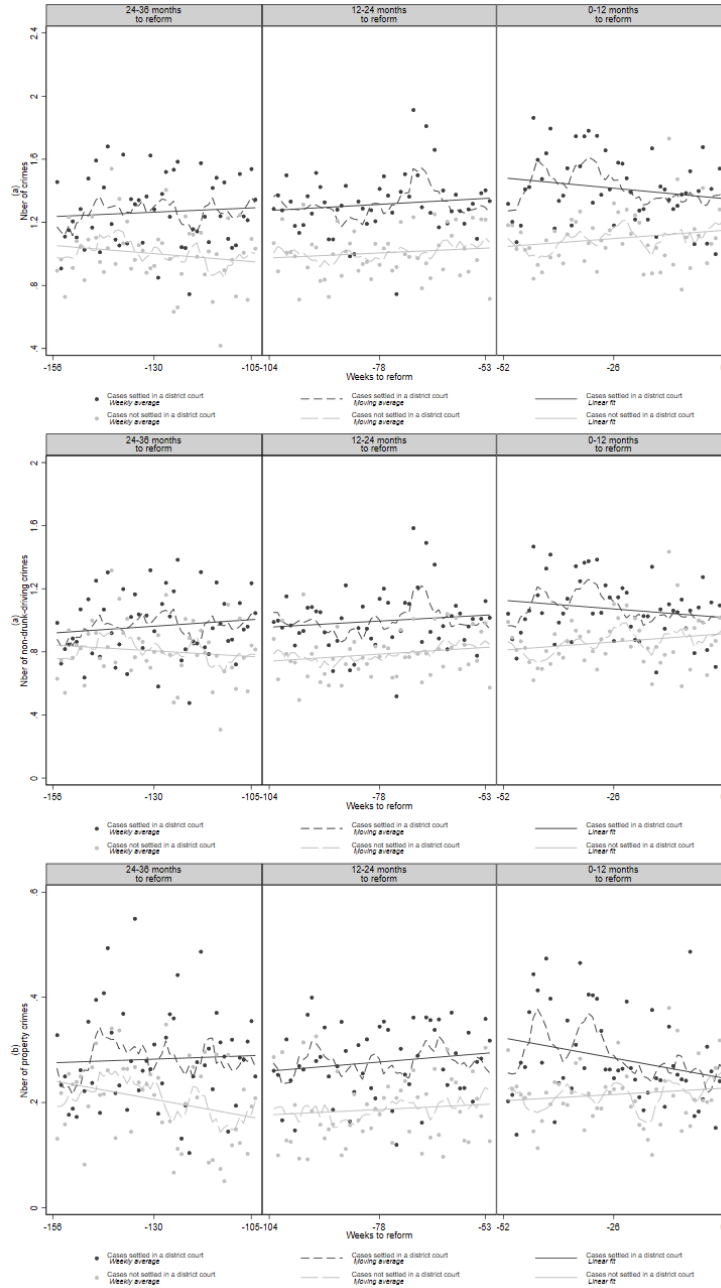
Offender's marital status	<p>Dummy variables indicative of the following five groups of individuals:</p> <ul style="list-style-type: none"> <li>- Single</li> <li>- In a partnership</li> <li>- Separated</li> <li>- Widow</li> <li>- Unknown status</li> </ul> <p>Measured at the end of the year preceding 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>

## 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>

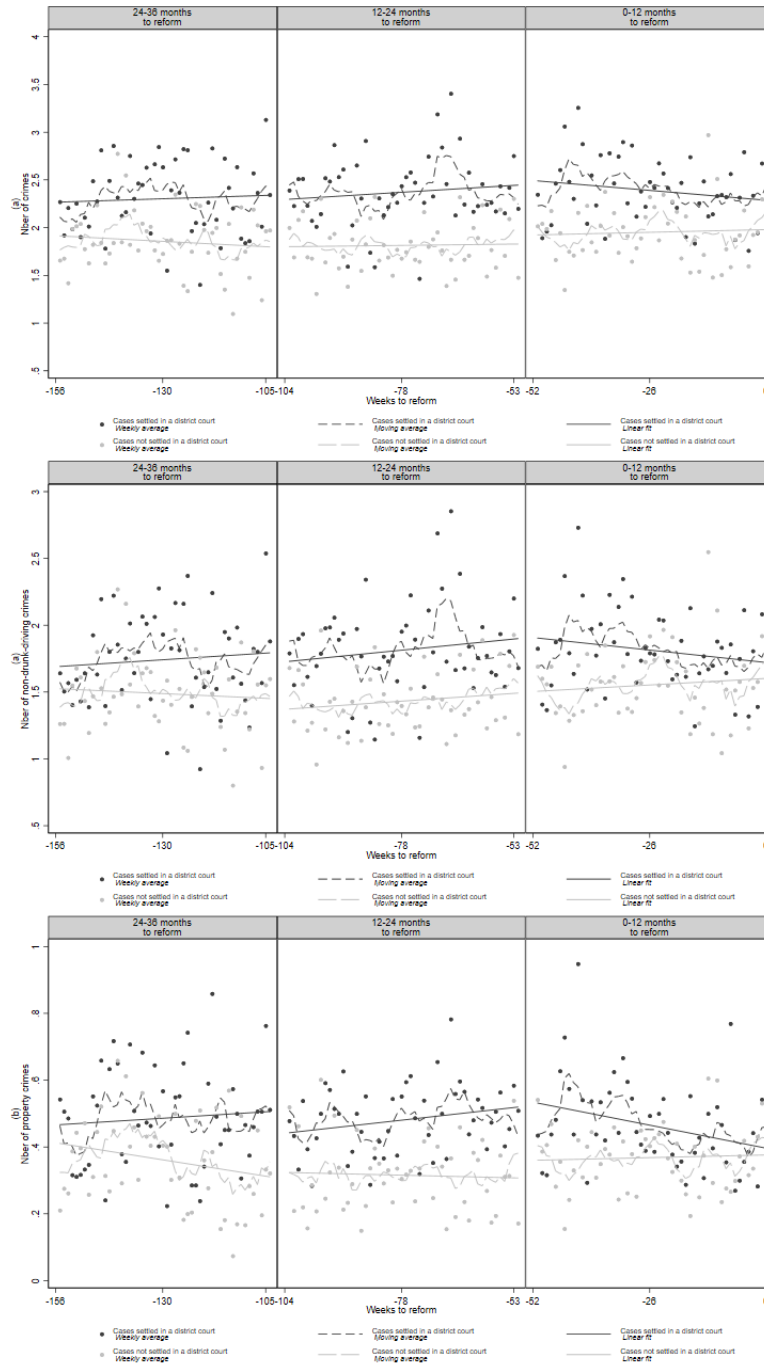
## Appendix A.4. Trends in outcome variables

Figure A.4.1.a Trends in crime outcomes at 5 years



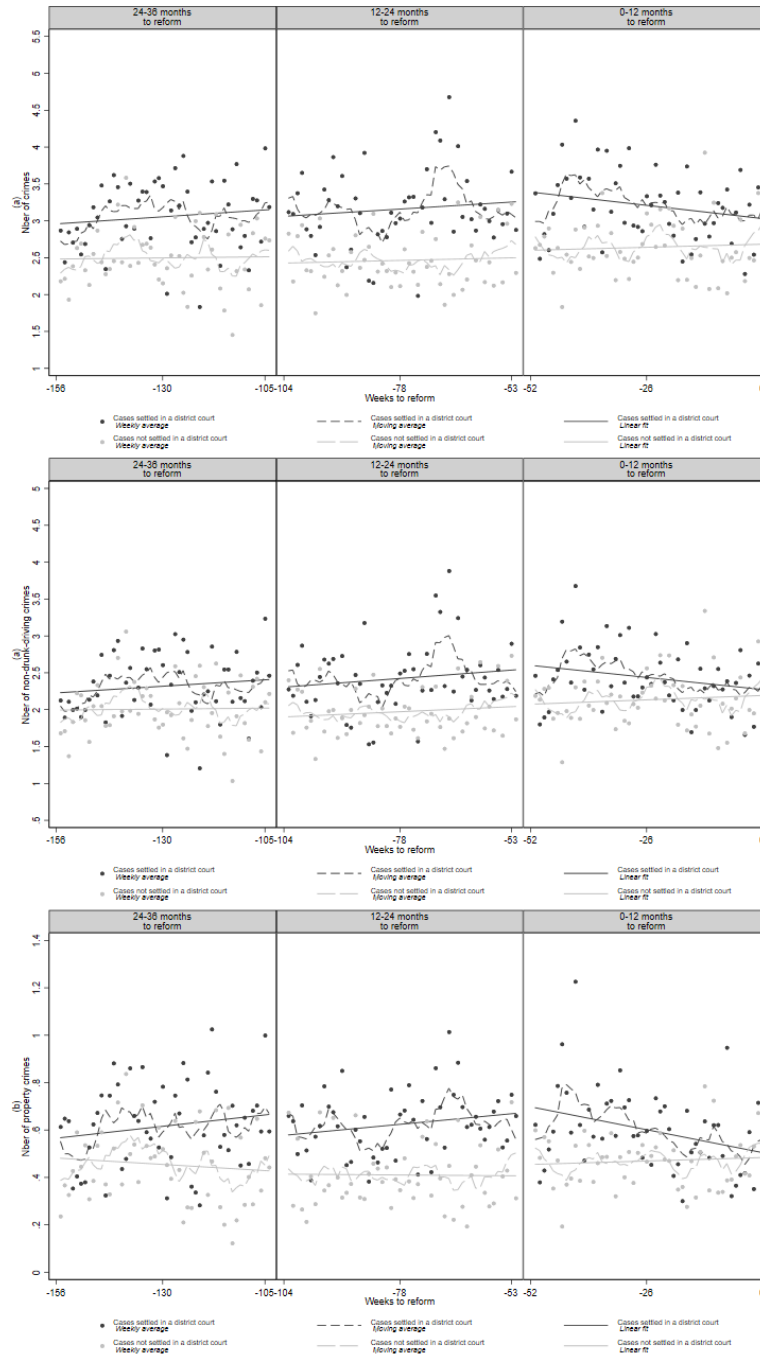
*Notes:* This figure represents the average number of crimes committed in the *five years* following sentencing for drunk drivers arrested during the 36-month period prior to the signing of the reform. Each point represents the average number of crimes committed by individuals arrested during a given week. Individuals tried for their drunk-driving crime are shown in dark grey, and those whose drunk-driving crime did not warrant an appearance before a judge are shown in light grey. For each year and type of individual, we estimate and reproduce a linear trend. Panel a) shows the total number of crimes, panel b) the number of non-drunk driving crimes, and panel c) the number of property crimes.

**Figure A.4.1.b Trends in crime outcomes at 10 years**



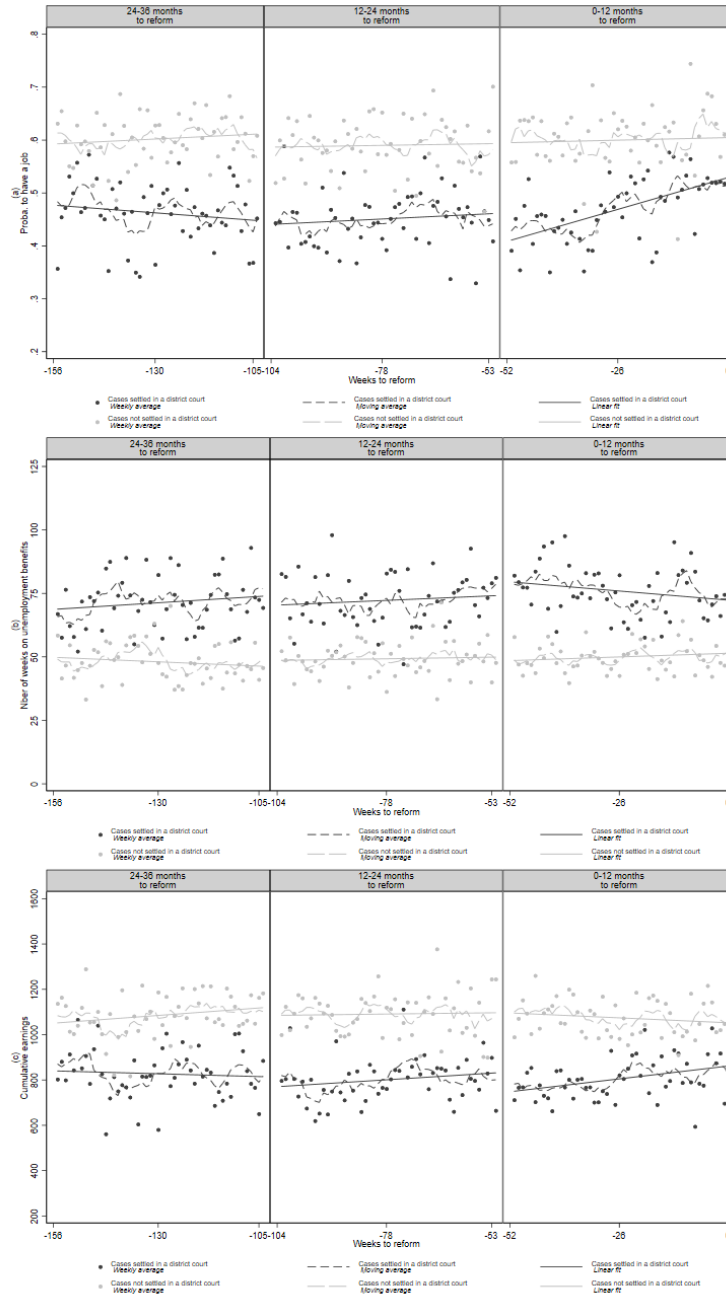
**Notes:** This figure represents the average number of crimes committed in the *ten years* following sentencing for drunk drivers arrested during the 36-month period prior to the signing of the reform. Each point represents the average number of crimes committed by individuals arrested during a given week. Individuals tried for their drunk-driving crime are shown in dark grey, and those whose drunk-driving crime did not warrant an appearance before a judge are shown in light grey. For each year and type of individual, we estimate and reproduce a linear trend. Panel a) shows the total number of crimes, panel b) the number of non-drunk driving crimes, and panel c) the number of property crimes.

**Figure A.4.1.c Trends in crime outcomes at 15 years**



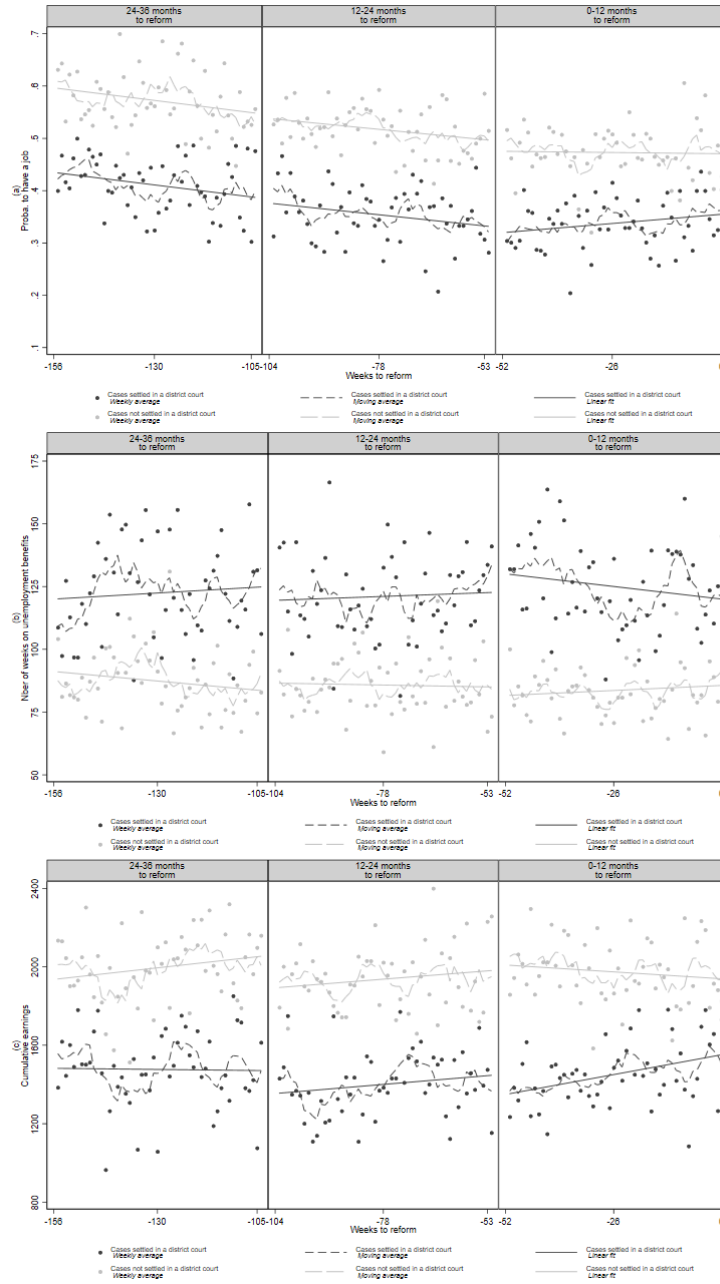
**Notes:** This figure represents the average number of crimes committed in the 15 years following sentencing for drunk drivers arrested during the 36-month period prior to the signing of the reform. Each point represents the average number of crimes committed by individuals arrested during a given week. Individuals tried for their drunk-driving crime are shown in dark grey, and those whose drunk-driving crime did not warrant an appearance before a judge are shown in light grey. For each year and type of individual, we estimate and reproduce a linear trend. Panel a) shows the total number of crimes, panel b) the number of non-drunk driving crimes, and panel c) the number of property crimes.

**Figure A.4.2.a Trends in labor outcomes at 5 years**



*Notes:* This figure represents offenders' labor market attachment 5 years following sentencing for drunk drivers arrested during the 36-month period prior to the signing of the reform. Each point represents the average number of crimes committed by individuals arrested during a given week. Individuals tried for their drunk-driving crime are shown in dark grey, and those whose drunk-driving crime did not warrant an appearance before a judge are shown in light grey. For each year and type of individual, we estimate and reproduce a linear trend. Panel a) shows the probability of having a job at the end of each year following the decision of justice; Panel b) shows the cumulative number of weeks during which offenders received unemployment-related benefits; and Panel c) shows the cumulative income (in 1,000 DKK).

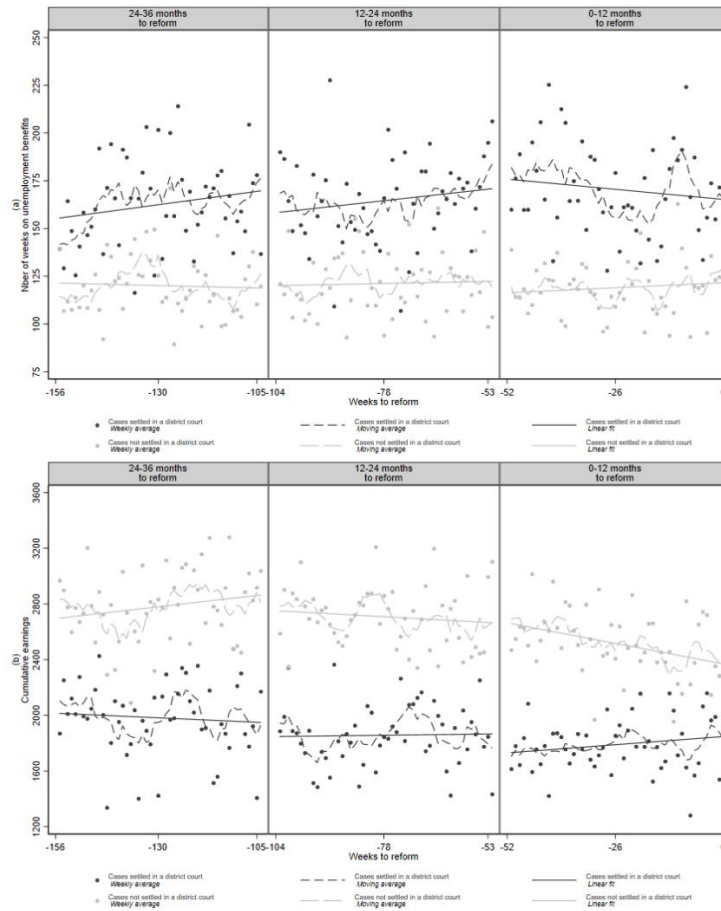
**Figure A.4.2.b Trends in labor outcomes at 10 years**



*Notes:* This figure represents offenders’ labor market attachment 10 years following sentencing for drunk drivers arrested during the 36-month period prior to the signing of the reform. Each point represents the average number of crimes committed by individuals arrested during a given week. Individuals tried for their drunk-driving crime are shown in dark grey, and those whose drunk-driving crime did not warrant an appearance before a judge are shown in light grey. For each year and type of individual, we estimate and reproduce a linear trend. Panel a) shows the probability of having a job at the end of each year following the decision of justice; Panel b) shows the cumulative number of weeks during which offenders received unemployment-related benefits; and Panel c) shows the cumulative income (in 1,000 DKK).



**Figure A.4.2.c Trends in labor outcomes at 15 years**



*Notes:* This figure represents offenders' labor market attachment 15 years following sentencing for drunk drivers arrested during the 36-month period prior to the signing of the reform. Each point represents the average number of crimes committed by individuals arrested during a given week. Individuals tried for their drunk-driving crime are shown in dark grey, and those whose drunk-driving crime did not warrant an appearance before a judge are shown in light grey. For each year and type of individual, we estimate and reproduce a linear trend. Panel a) shows the cumulative number of weeks during which offenders received unemployment-related benefits; and Panel b) shows the cumulative income (in 1,000 DKK).

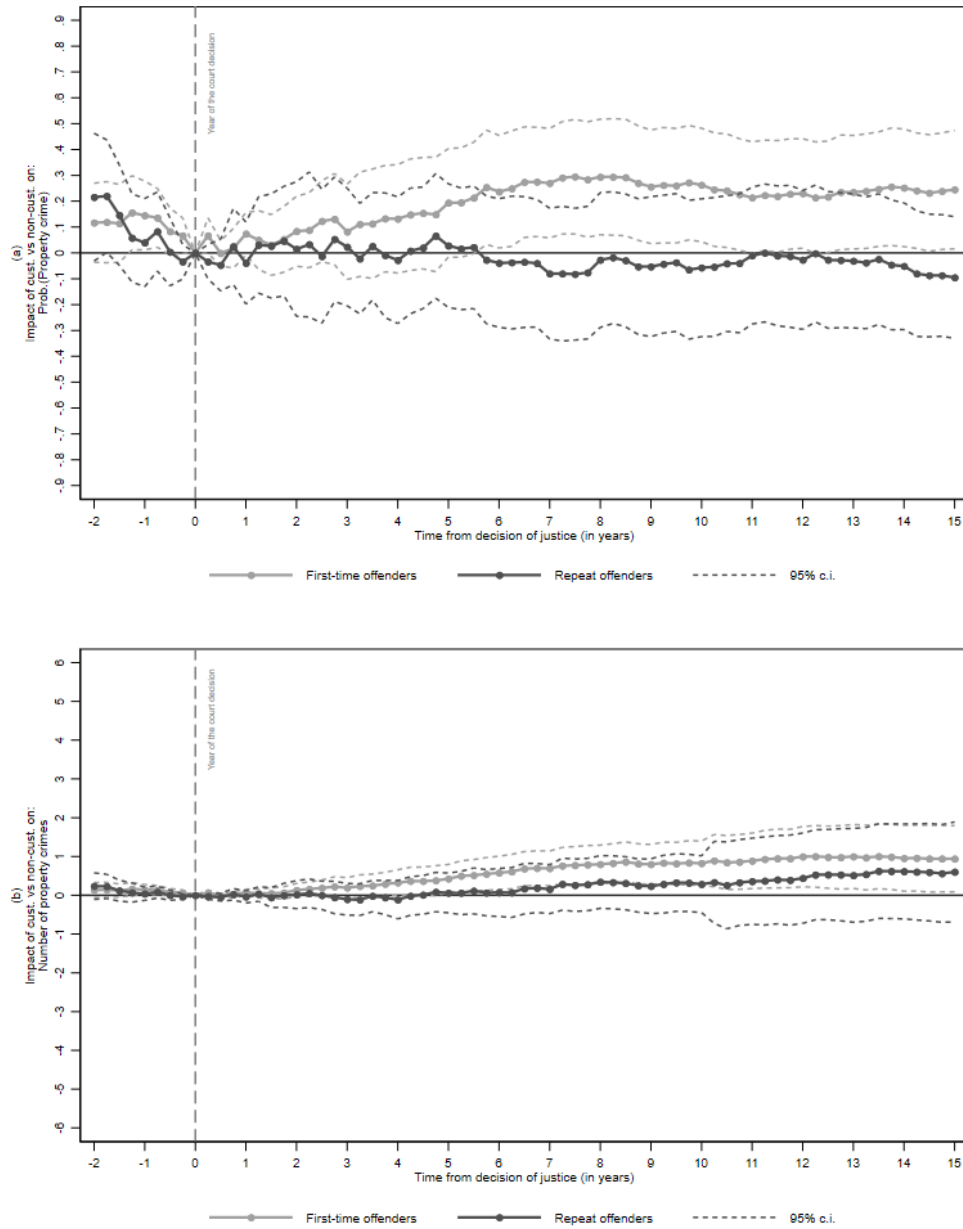
## Appendix A.5. First-stage by subgroup

Subgroups	# Obs.	Probability of being incarcerated		
		Mean	Instrument	
Gender	<i>Female</i>	1,086	0.299 (0.032)	0.009 (0.032)
	<i>Male</i>	11,209	0.378 (0.008)	-0.081*** (0.008)
Age	<i>Below 30</i>	3,300	0.357 (0.016)	-0.071*** (0.016)
	<i>Between 30 and 40</i>	4,065	0.393 (0.013)	-0.075*** (0.013)
	<i>Above 40</i>	4,930	0.362 (0.013)	-0.072*** (0.013)
Origin	<i>Immigrant</i>	654	0.306 (0.041)	-0.088** (0.041)
	<i>Other</i>	11,639	0.375 (0.008)	-0.073*** (0.008)
Education	<i>Lower education</i>	6,418	0.377 (0.010)	-0.080*** (0.010)
	<i>Higher education</i>	5,410	0.365 (0.013)	-0.065*** (0.013)
Employment status	<i>Had a job</i>	7,824	0.363 (0.009)	-0.086*** (0.009)
	<i>Did not have a job</i>	4,471	0.384 (0.013)	-0.049*** (0.013)
Prior drunk driving	<i>No prior drunk driving</i>	5,107	0.344 (0.012)	-0.062*** (0.012)
	<i>Prior drunk driving(s)</i>	7,188	0.390 (0.011)	-0.083*** (0.011)
Prior incarceration	<i>No prior incarceration spell</i>	6,877	0.335 (0.018)	-0.078*** (0.018)
	<i>Prior incarceration spell(s)</i>	5,418	0.416 (0.012)	-0.075*** (0.012)

*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 was incarcerated is regressed on my 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.

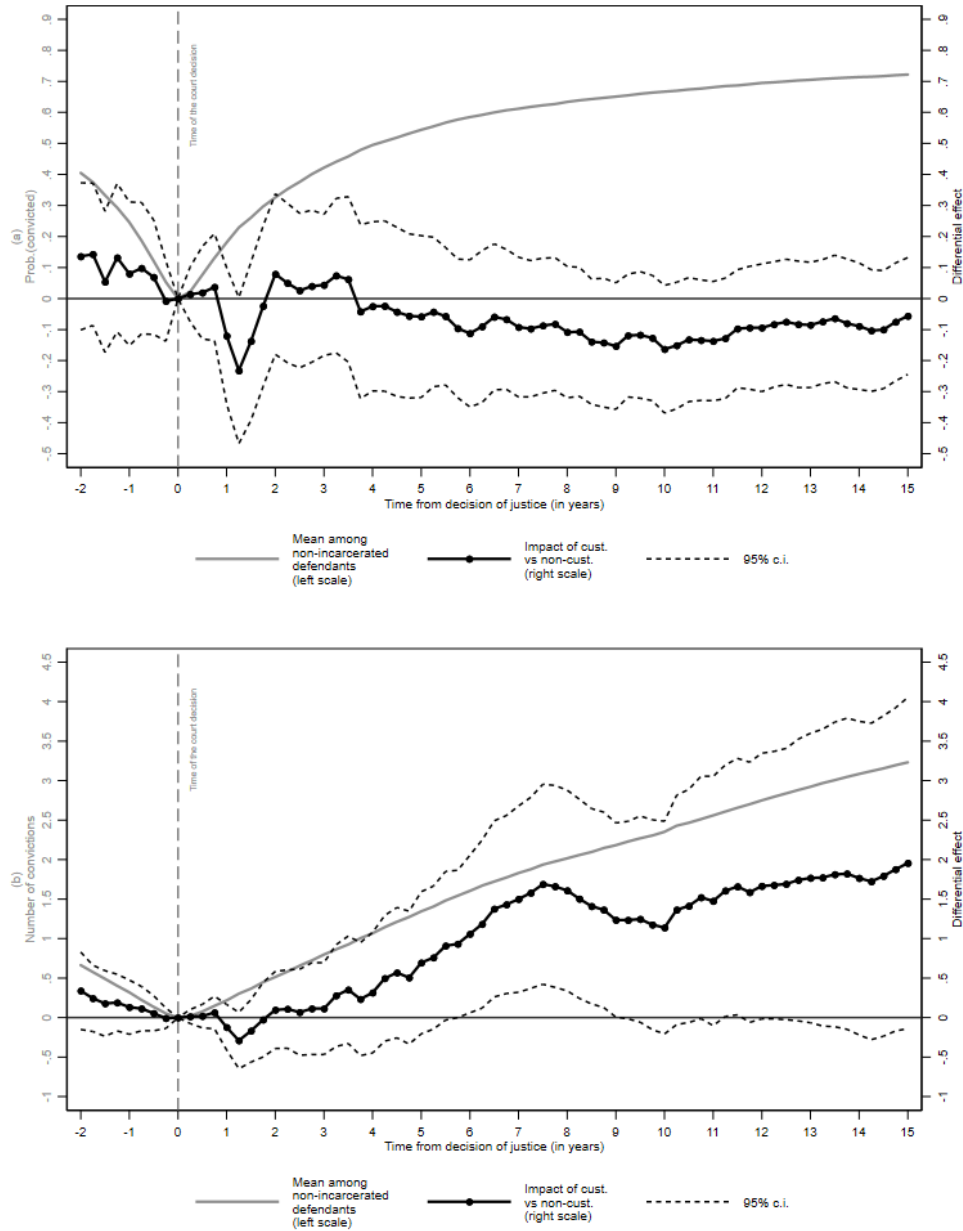
## Appendix A.6. Heterogeneous impact of custodial vs. non-custodial sentences on *property* crimes



*Notes:* This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) on the following outcomes for individuals who have never been incarcerated (first-time offenders) and for those who have (repeat offenders): a) the probability of being convicted of a post-sentencing *property* crime; b) the number of such convictions. 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.

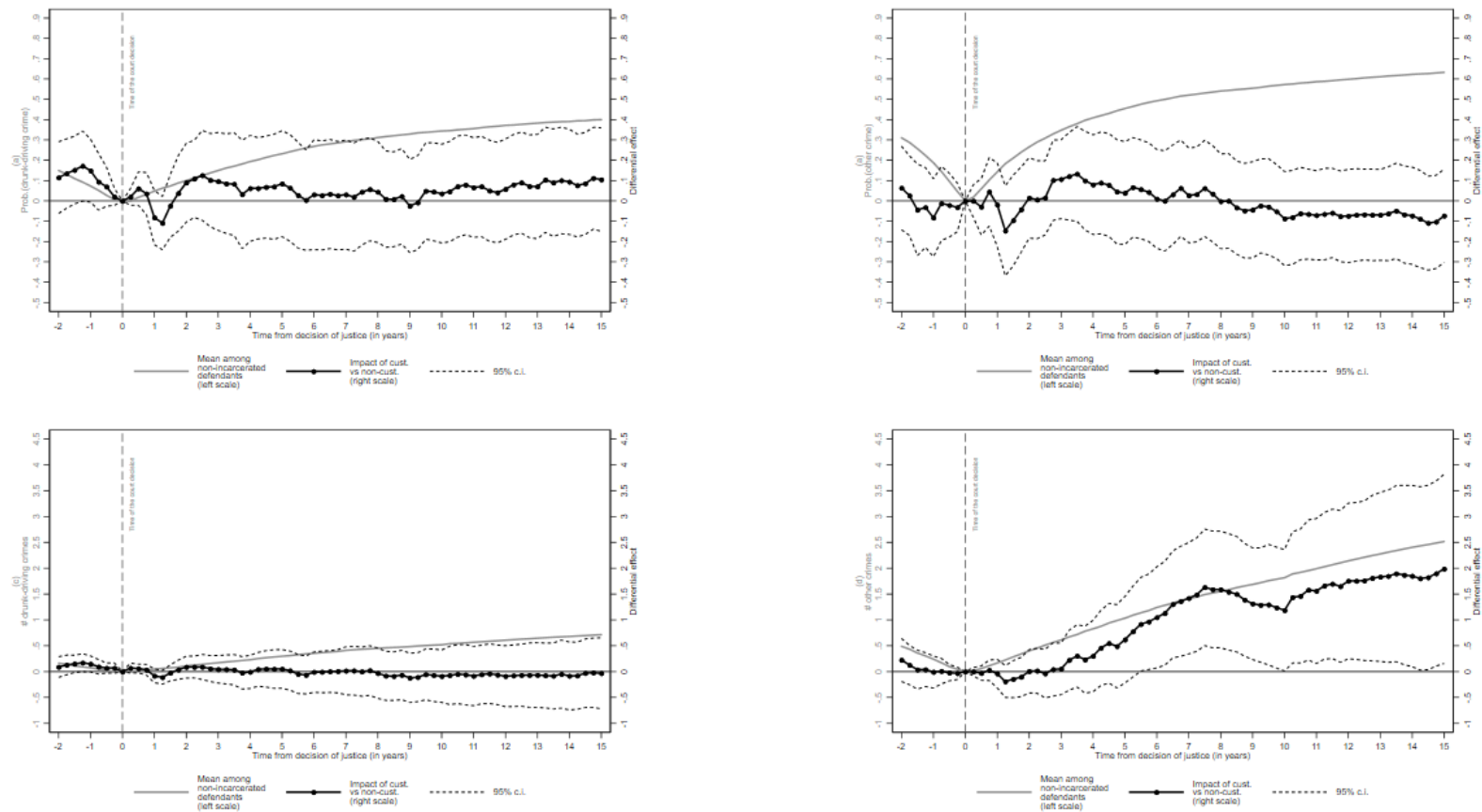
**Appendix A.7.** Alternative specification (24-month period sample)

**Figure A.7.1:** Impact of custodial vs. non-custodial sentences on crimes



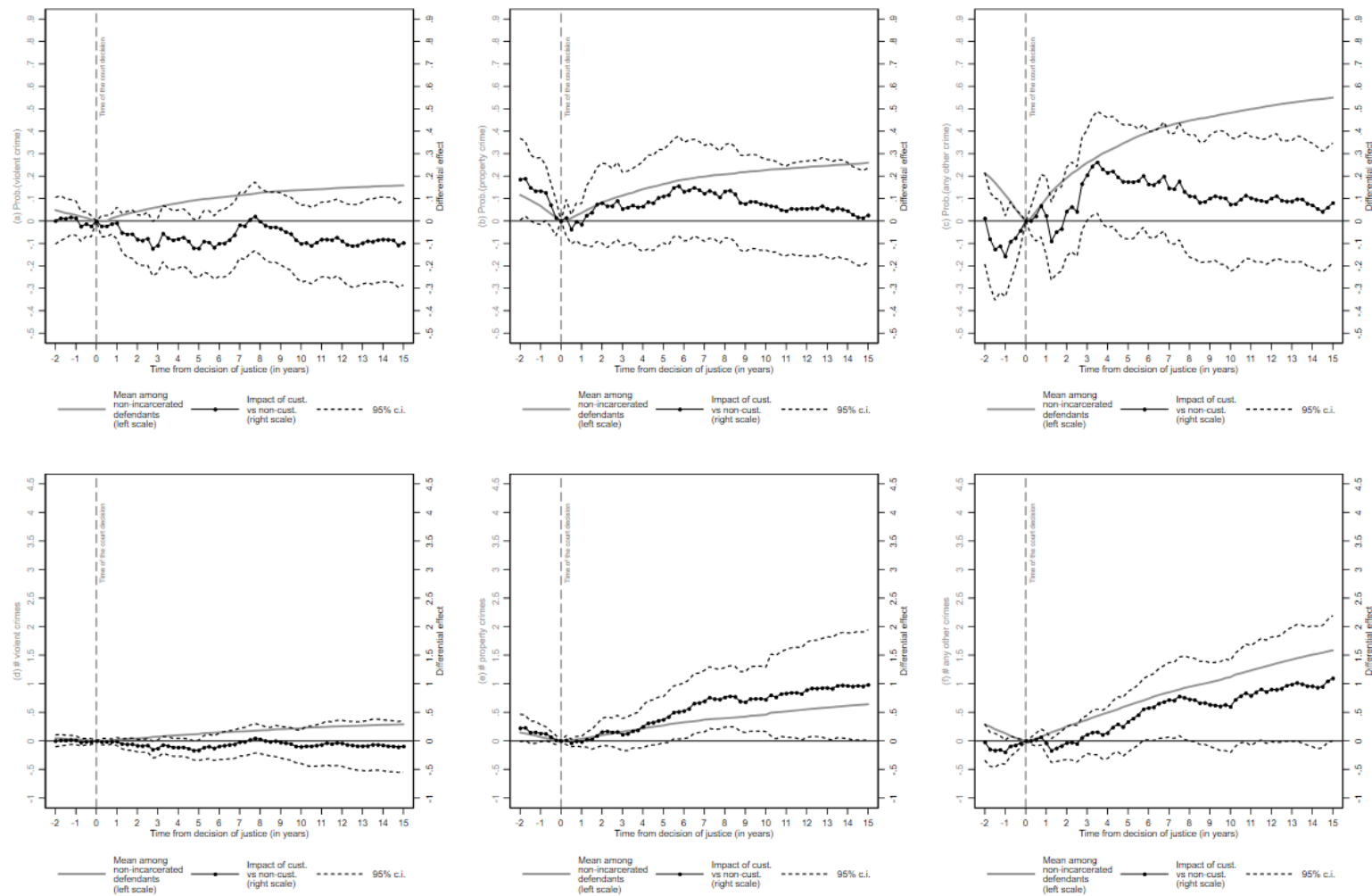
*Notes:* This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) on the following outcomes: a) the probability of being convicted of a post-sentencing crime (any type of crime); b) the number of such convictions. Crime outcomes are measured every 3 months from the date when the drunk-driving case was settled in court. Dashed lines represent the 95% confidence interval. Here, we restrict the sample to individuals tried for a drunk-driving crime committed in the 24-month period before the signing of the reform to test the sensitivity of our results to a change in the method used to estimate the trend in the outcome variables.

**Figure A.7.2:** Impact of custodial vs. non-custodial sentences on drunk-driving and other crimes



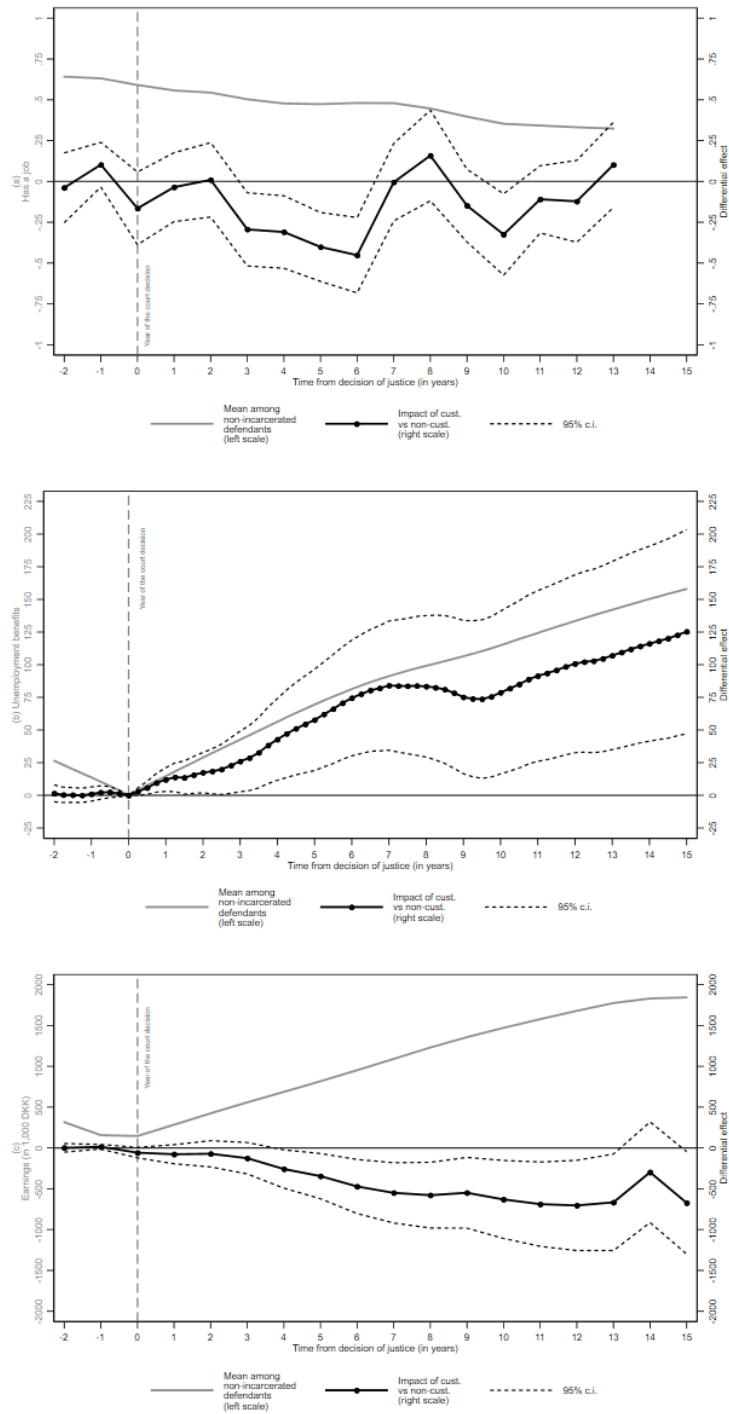
**Notes:** This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) on the following outcomes: a) the probability of being convicted of a drunk-driving crime; b) the probability of being convicted of any non-drunk-driving crime; c) the number of convictions for a drunk-driving crime; and d) the number of convictions for any crime other than drunk driving. 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. Here, we restrict the sample to individuals tried for a drunk-driving crime committed in the 24-month period before the signing of the reform to test the sensitivity of our results to a change in the method used to estimate the trend in the outcome variables.

**Figure A.7.3:** Impact of custodial vs. non-custodial sentences on drunk-driving and other crimes



*Notes:* This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) at the extensive and intensive margins 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. These two outcomes are measured every 3 months from the date when the drunk-driving case was settled in court. Here, we restrict the sample to individuals tried for a drunk-driving crime committed in the 24-month period before the signing of the reform to test the sensitivity of our results to a change in the method used to estimate the trend in the outcome variables.

**Figure A.7.4: Impact of custodial vs. non-custodial sentences on labor market attachment**



*Notes:* 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. Data availability constraints prevent us from calculating the impact on outcome a) beyond 12 years. Here, we restrict the sample to individuals tried for a drunk-driving crime committed in the 24-month period before the signing of the reform to test the sensitivity of our results to a change in the method used to estimate the trend in the outcome variables.

## Appendix A.8. Difference-in-differences results

**Table A.8.1:** Impact on crime (extensive margin)

	All			OLS		RF		IV			
	N	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</i>											
2 years before	8211	0.41	0.49	0.02	0.01	0.00	0.01	-0.01	0.06	0.02	0.05
Within 5 years	8211	0.56	0.50	0.04	0.01 ***	0.00	0.01	-0.06	0.06	-0.02	0.05
Within 10 years	8211	0.68	0.47	0.03	0.01 **	-0.01	0.01	-0.10	0.05 *	-0.06	0.04
Within 15 years	8211	0.74	0.44	0.02	0.01 *	-0.02	0.01 **	-0.11	0.04 *	-0.08	0.04 **
<i>Panel B: Probability of being convicted again for drink-driving</i>											
2 years before	8211	0.16	0.37	0.03	0.01 **	0.01	0.01	0.03	0.05	0.05	0.05
Within 5 years	8211	0.25	0.43	0.03	0.01 ***	0.01	0.01	0.03	0.05	0.05	0.05
Within 10 years	8211	0.36	0.48	0.03	0.01 ***	0.01	0.01	0.04	0.05	0.06	0.05
Within 15 years	8211	0.42	0.49	0.03	0.01 **	0.02	0.01	0.06	0.05	0.08	0.05
<i>Panel C: Probability of being convicted again for any crime other than drink-driving</i>											
2 years before	8211	0.31	0.46	0.00	0.01	0.00	0.01	-0.03	0.04	-0.01	0.04
Within 5 years	8211	0.47	0.50	0.02	0.01	-0.01	0.01	-0.09	0.05 *	-0.06	0.05
Within 10 years	8211	0.59	0.49	0.02	0.01	-0.01	0.01	-0.10	0.06 *	-0.07	0.05
Within 15 years	8211	0.65	0.48	0.01	0.01	-0.02	0.01 **	-0.12	0.05 *	-0.09	0.04 **
Period FE				YES		YES		YES		YES	
Month-of-crime & district court FE				YES		YES		YES		YES	
Additional Cov.				YES		YES		NO		YES	
Trend				NO		NO		NO		NO	
F-test											438

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



**Table A.8.2: Impact on crime (intensive margin)**

	All			OLS		RF		IV			
				(1)		(2)		(3)		(4)	
	N	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Total number of convictions</i>											
2 years before	8211	0.67	1.03	0.03	0.03	0.01	0.02	-0.01	0.11	0.06	0.10
Within 5 years	8211	1.40	1.90	0.10	0.05 **	-0.02	0.04	-0.23	0.21	-0.09	0.19
Within 10 years	8211	2.41	3.05	0.12	0.08	0.04	0.06	-0.04	0.32	0.19	0.27
Within 15 years	8211	3.24	4.08	0.11	0.10	0.07	0.07	0.03	0.41	0.33	0.32
<i>Panel B: Number of drink-driving convictions</i>											
2 years before	8211	0.18	0.42	0.03	0.01 **	0.01	0.01	0.03	0.05	0.05	0.05
Within 5 years	8211	0.32	0.63	0.05	0.02 ***	0.01	0.02	0.02	0.07	0.05	0.07
Within 10 years	8211	0.54	0.89	0.08	0.03 ***	0.03	0.02	0.08	0.10	0.12	0.10
Within 15 years	8211	0.73	1.10	0.10	0.03 ***	0.03	0.02	0.08	0.10	0.15	0.10
<i>Panel C: Number of convictions other than drink-driving convictions</i>											
2 years before	8211	0.48	0.88	-0.01	0.02	0.00	0.02	-0.05	0.08	0.00	0.07
Within 5 years	8211	1.08	1.70	0.05	0.04	-0.03	0.04	-0.23	0.19	-0.13	0.17
Within 10 years	8211	1.85	2.72	0.04	0.07	0.01	0.05	-0.12	0.29	0.06	0.24
Within 15 years	8211	2.49	3.66	0.00	0.09	0.03	0.06	-0.09	0.38	0.14	0.29
Period FE				YES		YES		YES		YES	
Month-of-crime & district court FE				YES		YES		YES		YES	
Additional Cov.				YES		YES		NO		YES	
Trend				NO		NO		NO		NO	
F-test											438

*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 (a dummy variable indicative of whether or not a defendant was tried before the reform); 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). Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A.8.3: Impact on non-drunk-driving crimes, decomposition (extensive margin)**

	All			OLS		RF		IV			
				(1)		(2)		(3)		(4)	
	N	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
<i>Panel A: Probability of being convicted again for a crime violent</i>											
2 years before	8211	0.05	0.22	0.00	0.01	-0.01	0.00	-0.03	0.02	-0.03	0.02
Within 5 years	8211	0.10	0.30	-0.01	0.01	-0.01	0.01	-0.06	0.03 *	-0.05	0.03
Within 10 years	8211	0.14	0.35	0.00	0.01	-0.01	0.01	-0.04	0.04	-0.03	0.04
Within 15 years	8211	0.16	0.37	0.00	0.01	-0.01	0.01	-0.04	0.04	-0.03	0.04
<i>Panel B: Probability of being convicted again for a property crime</i>											
2 years before	8211	0.12	0.32	0.01	0.01	0.01	0.01	0.03	0.03	0.04	0.03
Within 5 years	8211	0.17	0.38	0.02	0.01 *	0.01	0.01	0.01	0.04	0.03	0.04
Within 10 years	8211	0.23	0.42	0.02	0.01	0.00	0.01	-0.02	0.04	0.00	0.04
Within 15 years	8211	0.26	0.44	0.02	0.01	0.00	0.01	-0.04	0.05	-0.02	0.04
<i>Panel C: Probability of being convicted again for any other crime (excluding drunk driving)</i>											
2 years before	8211	0.20	0.40	-0.01	0.01	0.00	0.01	-0.01	0.04	0.00	0.04
Within 5 years	8211	0.37	0.48	0.01	0.01	-0.01	0.01	-0.08	0.06	-0.06	0.05
Within 10 years	8211	0.49	0.50	0.00	0.01	-0.01	0.01	-0.09	0.07	-0.06	0.06
Within 15 years	8211	0.56	0.50	0.00	0.01	-0.01	0.01	-0.10	0.06 *	-0.07	0.05
Period FE				YES		YES		YES		YES	
Month-of-crime & district court FE				YES		YES		YES		YES	
Additional Cov.				YES		YES		NO		YES	
Trend				NO		NO		NO		NO	

*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 (a dummy variable indicative of whether or not a defendant was tried before the reform); finally, we report the Instrumental Variables (IV) estimates obtained by instrumenting the dummy variable indicating whether or not individual *i* received a custodial sentence by our instrument. For each category of estimates, we report estimates obtained when a trend, a dummy variable indicative of whether or not the crime was committed in the 12 month-period preceding the entering into force of the reform, month of crime and district court fixed effects, and the whole conditioning set are added to the estimated equation (columns 1, 2, and 4). For IV estimates, we also report the results obtained when the conditioning set is not added to the estimated equation (column 3). Standard errors are clustered at the district court and individual levels.

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

**Table A.8.4: Impact on non-drunk-driving crimes, decomposition (intensive margin)**

	All			OLS		RF		IV			
				(1)		(2)		(3)		(4)	
	N	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	8211	0.05	0.22	0.00	0.01	-0.01	0.00	-0.03	0.02	-0.03	0.02
Within 5 years	8211	0.12	0.40	-0.01	0.01	-0.01	0.01	-0.07	0.04 *	-0.06	0.04
Within 10 years	8211	0.23	0.66	0.00	0.02	-0.01	0.01	-0.06	0.07	-0.04	0.07
Within 15 years	8211	0.27	0.74	0.01	0.02	0.00	0.02	-0.03	0.08	0.00	0.07
<i>Panel B: Number of property crimes</i>											
2 years before	8211	0.15	0.44	0.02	0.01	0.01	0.01	0.05	0.04	0.07	0.04 *
Within 5 years	8211	0.29	0.75	0.06	0.02 **	0.01	0.02	0.03	0.09	0.05	0.08
Within 10 years	8211	0.48	1.18	0.08	0.04 **	0.04	0.02	0.13	0.12	0.17	0.11
Within 15 years	8211	0.62	1.50	0.08	0.05 *	0.05	0.03	0.16	0.16	0.22	0.14
<i>Panel C: Number of other crimes (excluding drunk driving)</i>											
2 years before	8211	0.27	0.59	-0.02	0.01	0.00	0.01	-0.03	0.06	0.00	0.06
Within 5 years	8211	0.64	1.06	-0.01	0.03	-0.03	0.02	-0.21	0.11 *	-0.15	0.11
Within 10 years	8211	1.12	1.64	-0.05	0.04	-0.03	0.03	-0.23	0.18	-0.13	0.16
Within 15 years	8211	1.55	2.17	-0.11	0.05 **	-0.04	0.04	-0.34	0.24	-0.21	0.20
Period FE				YES		YES		YES		YES	
Month-of-crime & district court FE				YES		YES		YES		YES	
Additional Cov.				YES		YES		NO		YES	
Trend				NO		NO		NO		NO	

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

**Table A.8.5: Impact on labor market attachment**

	All			OLS		RF			IV			
	N	Mean	S.d.	(1)		(2)			(3)		(4)	
				Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	
<i>Panel A: Has a job</i>												
2 years before	8211	0.64	0.48	0.00	0.01				-0.04	0.05	-0.05	0.03
Within 5 years	8211	0.47	0.50	-0.05	0.01 ***	-0.04	0.01 ***		-0.20	0.04 ***	-0.20	0.04 ***
Within 10 years	8211	0.34	0.47	-0.04	0.01 ***	-0.02	0.01		-0.08	0.05	-0.08	0.05
<i>Panel B: Cumulative number of weeks spent on unemployment-related benefits</i>												
2 years before	8211	27.39	34.85	0.87	0.32 ***	0.43	0.29		1.72	3.42	2.02	1.37
Within 5 years	8211	74.56	82.94	15.85	1.58 ***	2.15	1.58		8.95	9.00	10.21	7.29
Within 10 years	8211	123.12	140.84	26.63	2.99 ***	5.80	2.67 **		25.02	14.45 *	27.58	12.47 **
Within 15 years	8211	169.42	193.21	35.20	4.10 ***	8.10	3.45 **		34.60	18.83 *	38.49	15.90 **
<i>Panel C: Cumulative earnings (in 1,000 DKK)</i>												
2 years before	8211	322.54	303.67	-1.53	2.74	-5.48	2.09 ***		-13.86	29.36	-26.05	9.99 ***
Within 5 years	8211	817.60	832.21	-70.90	15.23 ***	-22.80	16.59		-91.30	90.44	-108.34	76.43
Within 10 years	8211	1471.90	1505.78	-127.23	28.12 ***	-38.11	31.18		-160.89	166.29	-181.12	143.46
Within 15 years	8211	1805.01	1911.79	-123.63	35.86 ***	52.13	39.77		266.69	225.46	247.78	187.37
Period FE				YES		YES			YES		YES	
Month-of-crime & district court FE				YES		YES			YES		YES	
Additional Cov.				YES		YES			NO		YES	
Trend				NO		NO			NO		NO	
F-test												438

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