Curriculum Vitae

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Current position

Post-doctoral Fellow, Aarhus University, Denmark — July 2017--
Trygfonden's Center for Child Research Fellow (since 2017)
J-PAL Post Primary Education Initiative Invited Researcher (since 2015)

Education

Ph.D. in Economics, Aarhus University, Denmark — 2014-2017
Dissertation title: Essays in the Economics of Crime and Development Economics

M.Sc. in Economics (Economic Analysis and Policy), École des Hautes Etudes en Sciences Sociales and Paris School of Economics, France — 2007-2009

B.A. in Management, École Normale Supérieure de Cachan, France — 2006-2007

École Normale Supérieure de Cachan, entrance examinations in "Economics and Management": accepted and ranked 2nd — 2006-2009

B.A. in Econometrics, Paris I-Panthéon Sorbonne, France — 2003-2006

Research

Research Interests: Economics of Crime and Development Economics

Working Papers

- *Custodial vs. Non-Custodial Punishments: Measuring the Impact of an Anticipated Reform* (**Job Market Paper**)
- *Measuring the Impact of Incarceration on Life Trajectories* (with M. Rosholm and M. Simonsen), submitted
- *Evaluating the Impact of Entrepreneurship Edutainment in Egypt: an Experimental Approach with Peer Effects* (with G. Barsoum, B. Crépon, D. Gardiner, and W. Parienté), submitted [AEA RCT Registration] [J-PAL]

Work in Progress

Development Economics:
- *Measuring the Impact of Early Childhood Interventions and their Persistency: Experimental Evidence from a Large-Scale Preschool Construction Program in Côte d'Ivoire*
- *The impact of secondary school scholarships on girls' skill development and female empowerment in Niger* (with H. Giacobino and H. Huillery) [J-PAL] [AEA RCT Registration]

- *Empowering Adolescent Girls in the Sahel: Evidence from a Multi-Country RCT of the Sahel Women Empowerment and Demographic Dividend Project* (with O. Bertelli, I. Botea, H. Giacobino, H. Huillery, J. Kazianga, E. Koussoube, M. Olapade, and L. Rouanet)

Economics of Crime:
- *Effects of in-person therapy on children growing up in families with an alcohol abuse problem, an experimental approach* (with M. Simonsen)
- *Using Machine Learning to Predict Child Maltreatment* (with M. Rosholm)

Other fields:
- *Impact and Mechanisms: Why Consulting Matters in Human Capital Intensive Organizations. Evidence from a Field Experiment on Teacher Coaching* (with S. Calmar and H. Skyt Nielsen) [AEA RCT Registration]

Research Grants

- *Evaluating the Effects of Women Empowerment Interventions in Niger* (with H. Giacobino and H. Huillery), US$ 46,232, **J-PAL's Post-Primary Education Initiative** (2017)
- *Effects of in-person therapy on children growing up in families with an alcohol abuse problem – an experimental approach* (with M. Simonsen), DKK 5,000,000 (approximately US$ 750,000), **TrygFonden** (2017)
- *Dorms and Peers Effects: Exploring its persistence and mechanisms in Indian universities* (with M. Rao and B. Srinivasan), US$ 10,000, **J-PAL's Post-Primary Education Initiative** (2015)
- *Investigating the existence of biases in Danish courts of justice (*with M. Rosholm and M. Simonsen), DKK 350,000 (approximately US$ 54,000), **TrygFonden** (2015)
- *Evaluating the Effects of Entrepreneurship Edutainment in Egypt* (with G. Barsoum, B. Crépon, D. Gardiner, and W. Parienté), US$ 347,647, **3ie** (2014)

Relevant Experience

Lecturer
- Introduction to Empirical Economics (undergraduate), Fall 2017 & 2018
  Evaluation scores: 4.0/5 (2017), 4.1/5 (2018)
- Microeconomic Analysis of Markets, Organizations, and Behavior (undergraduate), Spring 2017

Teaching Assistant
- Microeconomic Theory (undergraduate), Spring 2015
- Introduction to Microeconomics (undergraduate), Fall 2015

Research Positions
- J-PAL South Asia (2011-2012), Research Manager, Chennai & New Delhi, India
- J-PAL Europe (2010-2011), Research Manager, Paris, France
- IPA Kenya (2009-2010), Research Associate, Busia, Kenya
- J-PAL Europe (2008-2009), Research Associate, Paris, France

Miscellaneous

Research stay abroad: Visiting Joseph Doyle at MIT (2016, Spring semester)

Conferences, Seminars(*) & Workshops(*)
- 2018: 10th Transatlantic Workshop on the Economics of Crime*, *27-28 Sep., Sciences Po, France*; ESPE, *25-27 June, Antwerp, Belgium*; 13th Nordic Summer Institute in Labour Economics*, *11-12 June, Helsinki, Finland*; AFSE, *14-16 May, Paris, France*
- 2017: 13th IZA Conference: Labor Market Policy Evaluation*, *5-6 Oct., Bonn, Germany*

- 2016: Rockwool Foundation Seminar*, *5 Dec., Copenhagen, Denmark*; Danish Graduate Program in Economics, *11-12 Nov., Middelfart, Denmark*; TryFonden's Centre for Child Research Workshop*, *6 Oct., Aarhus, Denmark*
- 2015: Danish Graduate Program in Economics, *19-20 Nov., Sandbjerg, Denmark*; ILO/J-PAL Evaluating youth employment programs: an executive course*, *22-26 Jun. Turin, Italia*
- 2014: ILO Workshop on the monitoring and evaluation of labour market policies*, *16-18 Sep. Hammamet, Tunisia*; Doha Evidence Symposium, *6-9 Mar. Doha, Qatar*

Scholarships
- Aarhus University & TrygFonden's Center for Child Research Ph.D. Scholarship (2014-2017)
- École Normale Supérieure de Cachan's Four-Year Scholarship (2006-2009), Economics and Management department

Language: French (native), English (fluent), Danish & Spanish (notions)

Proudest achievement: Zoé (Ebba) Beaujard-Michel, 15 months

References

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Working paper summaries

Custodial vs. Non-Custodial Punishments: Measuring the Impact of an Anticipated Reform **(Job Market Paper)**

Abstract: I study a large-scale reform of the Danish legislation implemented in 2000, whereby jail time was replaced by a probation period for most drink-driving crimes. Analyzing the implementation of the reform, I find sharp discontinuities in the way these cases were handled in criminal courts in the months preceding the reform. These discontinuities are partly driven by wealthier defendants who anticipated that they might avoid prison by postponing their trial until after the reform. To bypass the ensuing selection in the nature of the defendants tried around the reform, I measure the impact of the reform using an instrumental variable strategy which exploits a feature of the Danish legislation generating exogenous variation in the probability to be incarcerated among individuals arrested in the 12-month period preceding the reform based on the date of their crime. Overall, I find that substituting a probation period for an unconditional prison sentence did not increase offenders' subsequent criminal activity but weakened their labor market attachment.

Measuring the Impact of Incarceration on Life Trajectories (with M. Rosholm and M. Simonsen), submitted

Abstract: We measure the impact of incarceration on offenders' crime- and labor- related outcomes over a 10-year period following conviction. In order to do so, we exploit the random allocation of criminal cases to judges with varying incarceration propensities. We find that incarceration increases the share of offenders convicted of subsequent crime in the short run as well as the number of crimes they commit. We also find that these results are not driven by petty crimes or any bias in judges' sentencing behavior. Incarceration also reduces offenders' labor market attachment.

Evaluating the Impact of Entrepreneurship Edutainment in Egypt: an Experimental Approach with Peer Effects (with G. Barsoum, B. Crépon, D. Gardiner, and W. Parienté), submitted

Abstract: We measure the impact of an edutainment program broadcast on a popular Egyptian television channel and specifically designed to promote entrepreneurship among young adult viewers. In order to do so, we implemented a randomized controlled trial following a non-symmetric encouragement design to measure the impact of the intervention on viewers' attitudes towards self-employment, entrepreneurship-related knowledge, professional aspirations, and professional choices. Our design relies on cheap and easily scalable encouragements and allows us to identify the importance peer effects within groups of friends. We find that the show had some impact on viewers' general attitudes toward self-employment and, in particular, on gender-related beliefs. Its impact is otherwise limited. We also find evidence of complex peer effects within groups of friends, alternately amplifying and mitigating the impact of the show.

On-going projects

Development Economics:

Measuring the Impact of Early Childhood Interventions and their Persistency: Experimental Evidence from a Large-Scale Preschool Construction Program in Côte d'Ivoire
In short: I measure the impact of a large-scale preschool construction program implemented in 136 rural villages of Côte d'Ivoire. Half of them were randomly selected to receive a preschool. Children are tracked over a period of four years to measure whether or not the effects of attending preschool fade out during the first primary school years.

The impact of secondary school scholarships on girls' skill development and female empowerment in Niger (with H. Giacobino and H. Huillery)
In short: In many developing countries, gender inequalities begin at a young age and influence individuals' long-term trajectories. In particular, social norms can lead to girls dropping out of school, marrying, and having children early in life. Women with less education may then experience reduced empowerment and limited job opportunities. In turn, this can create a cycle of gender inequality: participating less in the labor force may increase economic dependence on men, and strengthen the social norms that contribute to women dropping out of school. Can encouraging girls to stay in school and to develop skills improve their future economic and social trajectories? In order to answer these questions, we evaluate the impact of secondary school scholarships for girls on skill development, empowerment, and job opportunities.

Empowering Adolescent Girls in the Sahel: Evidence from a Multi-Country RCT of the Sahel Women Empowerment and Demographic Dividend Project (with O. Bertelli, I. Botea, H. Giacobino, H. Huillery, J. Kazianga, E. Koussoube, M. Olapade, and L. Rouanet)
In short: We measure the impact of adolescent development club designed to improve adolescent girls' human capital in six West African countries (Côte d'Ivoire, Mali, Niger, Mauritania, Burkina Faso and Chad). In collaboration with the local Governments, local NGOs, and the World Bank, we implement randomized controlled trials so as to investigate the impact of the program as well as the different mechanisms through which they can achieve (or not) greater women empowerment.

Economics of Crime:

Effects of in-person therapy on children growing up in families with an alcohol abuse problem, an experimental approach (with M. Simonsen)
In short: We study the importance of psychological distress as a determinant of crime. To do so, we measure the impact of a therapy-based intervention targeting young Danes in need of psychological help. The intervention is evaluated using a randomized controlled trial using a rotation design.

Using Machine Learning to Predict Child Maltreatment (with M. Rosholm)
In short: In collaboration with the Danish Child Protection Services, we use machine-learning algorithms to predict the occurrence of a specific type of crime, namely child maltreatment. The objective of the project is to develop a tool case workers can use when they receive reports that a child may be suffering from maltreatment.

Other fields:

Impact and Mechanisms: Why Consulting Matters in Human Capital Intensive Organizations. Evidence from a Field Experiment on Teacher Coaching (with S. Calmar and H. Skyt Nielsen)
In short: Recent studies have demonstrated substantial effects of consultancy on learning and productivity (Allen et al., 2011; Bloom et al., 2013; Hanna et al., 2014). However, the channels through which the effect of consultancy materializes and the relative strength of the different channels remain to be clarified. In order to study this question, we implement a randomized controlled trial investigating the impact of consultancy in the form of a one-year coaching program aiming to improve teacher skills through peer coaching.

Custodial vs. Non-Custodial Punishments: Measuring the Impact of an Anticipated Reform

Bastien Michel

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Job Market Paper

November 13th, 2018

Abstract

This paper studies a large-scale reform of the Danish legislation implemented in 2000, whereby jail time (a custodial sentence) was replaced by a probation period (a non-custodial sentence) for most drink-driving crimes. First, I show that defendants and courts of justice anticipated the reform and that, as a consequence, a large share of trials were postponed until after the reform. I also show that the identity of the individuals who had their case postponed until after the reform was not random and that this introduced differences in the nature of the individuals tried before and after the reform, as well as inequities between defendants tried around that time. Second, I develop an innovative instrumental variable approach in order to measure the impact of the reform on offenders' post-sentencing criminal activities and labor market attachment. This approach exploits variation in the probability of being incarcerated among offenders tried for an alleged drink-driving crime committed in the 12-month period preceding the reform. Indeed, the combination of a specific feature of Danish legislation (guaranteeing that defendants tried after a reform for a crime committed prior to it must be tried under the more lenient of the two laws) and the extensive processing time for drink-driving cases when the reform was implemented (6 months on average) ensured that the closer to the reform a crime was committed, the higher the probability for the defendant to be tried under the new law and placed on probation (instead of being incarcerated). I show that this source of variation is exogenous and find that substituting a probation period for an unconditional prison sentence did not increase offenders' subsequent criminal activity but had a strong positive effect on their labor market attachment. Finally, I provide additional evidence that Difference-in-Difference estimators perform poorly in the presence of selection (including when donut-hole approaches are used).

JEL Codes: K14, K4

Acknowledgements: I would like to thank Timo Hener, Randi Hjalmarsson, Nicolai Kristensen, Elena Mattana, Anna Piil Damm, Michael Rosholm, and Marianne Simonsen for their useful comments and suggestions, as well as the District Courts in Aarhus and Odense and the Danish Prison and Probation Service for supplying information about relevant institutional details. Funding from TrygFonden's Centre for Child Research is greatly appreciated. I would also like to thank seminar participants at Aarhus University and the Rockwool Foundation for their useful comments. The usual disclaimer applies.

I. Introduction

Driving under the influence of alcohol (referred to as *drink-driving* hereafter) raises important public health and economic issues in most countries. Indeed, it significantly increases the risk of a road traffic crash and, ultimately, the likelihood that a serious injury or death will occur. In particular, 25% of all road fatalities in the European Union were due to alcohol in 2015 (European Commission, 2015), representing around 6,400 fatalities.[1] In the United States, this share was estimated at 28%, representing 10,265 fatalities in 2015 (NHTSA, 2017).[2] In consequence, drink-driving represents a significant cost for countries around the world. For instance, in the United States, the economic cost of all alcohol-impaired crashes was estimated at 44 billion dollars for the sole year of 2010 (NHTSA, 2017).

Despite the scope of the problem, little robust evidence is available on how best to sanction drink-drivers so as to limit recidivism,[3,4] in particular on the relative impact of custodial and non-custodial sanctions.[5] Indeed, while considerable evidence has already been gathered on the deterrent effect of different legal sanctions when applied to non-road traffic offenders (see Chalfin and McCrary (2017) for a review on the subject), what we know may not apply to drink-drivers, who are believed to be particularly affected by substance abuse problems. A notable exception is Hansen (2015), who exploited discontinuities in the severity of the punishment based on administrative blood alcohol content thresholds used to define illegal degrees of intoxication to measure the impact of incarceration and incarceration length on the recidivism rate. Using data from the State of Washington (USA), he found evidence consistent with the deterrence theory (Becker, 1968) suggesting that the more severe the punishment, the less likely a defendant was to recidivate and, in particular, that drivers liable to

[1] It was estimated that 80% of these deaths could have been avoided had all the drivers been sober (Calinescu and Adminaite, 2018).
[2] In low and middle-income countries, a survey of studies conducted found that alcohol was present in the blood of 4% to 69% of injured drivers, 18% to 90% of crash-injured pedestrians and 10% to 28% of injured motorcyclists (WHO, 2007).
[3] Although prevention policies have managed to reduce the number of road traffic accidents involving alcohol in many countries over the past 50 years.
[4] While evidence on the actual share of convicted drink-drivers suffering from an alcohol abuse problem is not very conclusive but suggests that it may be important (for instance, see Vingilis (1983), Lapham et al. (2001), and Chou et al. (2006)). In the context of Denmark, a study revealed that two thirds of all drink-driving offenders were suffering from an alcohol abuse problem in 1990 (Kramp et al., 1990).
[5] Indeed, a growing body of literature has recently been casting some doubts upon the universal superiority of custodial sanctions over non-custodial ones to prevent recidivism (for reviews on the subject, see Nagin et al. (2009) and Chalfin and McCrary (2017)). For instance, exploiting the random allocation of criminal cases to judges with varying incarceration propensities, Aizer and Doyle (2015) found that juvenile incarceration results in a higher adult incarceration rate in the US. Using a similar identification strategy in a Danish setting, Michel et al. (2018) reached a similar conclusion. On a related topic, Di Tella and Schargrodsky (2013) compared the impact of incarceration and electronic monitoring and found that the latter reduced offenders' rate of recidivism. Furthermore, the cost of custodial sentences is generally substantially higher than the cost of non-custodial ones, suggesting that non-custodial sanctions could be more cost-effective than custodial ones to fight drink-drivers' recidivism.

incarceration were less likely to recidivate than drivers not liable to any legal sanction.[6] Using a similar identification strategy and Swedish administrative data, Hinnerich et al. (2016) found that incarceration was also more effective at reducing the number of subsequent crimes committed by offenders when compared to probation. However, the regression discontinuity design used by these two studies may only identify local average treatment effects specific to drivers arrested with a blood alcohol content around the administrative thresholds.

Many empirical studies in this field of research (and in others) attempt to provide more general answers to questions similar to the ones raised above by using legislative changes and comparing individuals affected by a policy before and after its reform. In such studies, the key assumption is then that the former mimic what would have happened to the latter had the reform not been implemented. Among other things, an important condition for this assumption to be met is that the reform must not have been anticipated by the stakeholders (the individuals affected by the policy or the institutions in charge of implementing the policy), as it is likely to introduce selection in the nature of the individuals affected by the policy around the time of the reform. Some studies attempt to mitigate the consequences of this possible source of selection by implementing so-called donut-hole approaches, which consist in excluding from the analysis individuals affected by the policy around the time of its reform. However, because there is currently limited evidence documenting such a selection mechanism, this threat is frequently overlooked in quasi-experimental studies exploiting reforms as a source of identification. Moreover, although conceptually appealing, evidence is also lacking on the effectiveness of such donut-hole approaches to get rid of any selection.

In this paper, I provide new evidence on these questions by studying a large-scale reform of the Danish legislation implemented in 2000, whereby jail time (a custodial sentence) was replaced by a probation period (a non-custodial sentence) for most drink-driving crimes.[7] I start by analyzing the implementation of the reform and find evidence that stakeholders (defendants and courts of justice) modified their behavior in the weeks *preceding the reform* in anticipation of it. In practice, these anticipations materialized through a sharp drop in the number of cases tried and a linear decrease in the share of defendants placed on probation from the moment the law was signed (but before it actually entered into force). A closer look at the stakeholders' incentives suggests that both defendants

[6] Investigating possible explanatory mechanisms, the author concluded that these effects could not be attributed to either an incapacitation effect or a rehabilitation one but rather to a specific deterrence effect. This suggests that unconditional prison sentences are effective at deterring recidivism.
[7] As further discussed below, the probation period was associated with either a mandatory participation in a rehabilitation program or some community service (depending on whether the offender suffered from alcohol addiction).

and courts of justice had motives for postponing drink-driving cases until after the reform: the former to avoid prison, the latter to reduce the number of cases which might have to be retried. Indeed, an important feature of Danish legislation guarantees that defendants tried after a reform for a crime committed prior to it must be tried under the more lenient of the two laws. In the context of the reform at hand, this means that most individuals tried for a crime committed prior to the reform faced the risk of being incarcerated if tried before the reform, but 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 their case to be retried if they are still in prison when the reform enters into force. Furthermore, I also show that the identity of the individuals who had their case postponed until after the reform was not random. In particular, wealthier defendants were more likely to have their trial delayed and to avoid prison. In turn, this implies that differences were generated in the nature of the defendants tried before and after the reform, and that inequities were generated between defendants. From a methodological point of view, this also raises some questions with respect to the performance of traditional quasi-experimental estimators in the context of this reform.

In order to gather new evidence on the relative impact of incarceration and probation on offenders' post-sentencing criminal activities and labor market attachment, I use a quasi-experimental approach that allows me to bypass comparability issues which would arise if I was to compare individuals tried before and after the reform. More specifically, I develop an instrumental variable approach which uses random variation in the probability of receiving an unconditional prison sentence in the group of offenders arrested for a drink-driving crime committed in the 12-month period preceding the reform. Indeed, I show that the combination of the above-mentioned feature of Danish legislation and the extensive processing time for drink-driving cases when the reform was implemented (6 months on average) generated significant variation in the probability of these defendants being tried after the reform and placed on probation – again, as opposed to being tried before the reform and receiving an unconditional prison sentence. This variation is based on the distance between the date of their crime and the date of the reform. Hence, the closer to the reform a crime was committed, the higher the probability for the defendant to be tried under the new law and placed on probation instead of being incarcerated.[8] I show that this source of variation in offender's probability of receiving an

[8] This approach is loosely related to the one used in Drago et al. (2009) who used the Collective Clemency Bill passed by the Italian Parliament in July 2006 to measure the impact of suspended sentence length on recidivism. This reform reduced the length of the prison sentence of all inmates who had committed a crime before May 2, 2006. As a consequence, about 40 percent of the prison population of Italy were released from prison on August 1, 2006 under the condition that they would have to serve the remaining of their sentence

unconditional prison sentence is independent of offenders' and cases' characteristics, and use it as a source of identification to measure the impact of the reform. The selection in the nature of the offenders tried around the time of the reform only marginally affects the set of individuals on which the effects are estimated.

This approach allows me to gather first-hand evidence on the relative impact of custodial and non-custodial prison sentences on post-sentencing criminal activities and labor-market attachment up to 10 years after the decision of justice. I also provide additional evidence on the performance of Difference-in-Difference estimators in the presence of selection issues. Using Denmark's rich administrative datasets, I show that in contrast with prior available evidence and standard predictions from the deterrence theory, substituting a probation period for an unconditional prison sentence does not have any impact on offenders' probability of committing another drink-driving crime or on the number of drink-driving crimes they commit. However, incarceration seems to have a small criminogenic effect as it increases the probability for a defendant to commit other road traffic crimes and the number of such crimes. Moreover, incarceration appears to have a strong negative effect on offenders' labor market attachment, significantly reducing both the annual number of days worked and their annual earnings. Finally, I also find that Difference-in-Difference estimators can reach very different conclusions, including when donut-hole approaches are implemented. This is the case for the impact of incarceration on offenders' labor market attachment, for which these estimators find completely opposite effects (positive ones).

This paper contributes to three different areas of the literature. First, it contributes to the growing literature documenting justice systems' sources of dysfunction (Vidmar, 2011; Danziger et al., 2011; Abrams et al., 2012; Anwar et al., 2012; Anwar et al., 2014; Philippe and Ouss, 2018). Indeed, I provide evidence questioning the degree of equity with which justice systems handle cases in times when complicated legislative changes are passed. Second, this paper contributes to the literature on the relative impact of custodial and non-custodial sentences (Chalfin and McCrary, 2017) and, in particular, to the limited number of studies focusing on drink-drivers (Hansen, 2015; Hinnerich et al., 2016). Indeed, I provide evidence showing that drink-drivers do not always behave in accordance to the deterrence theory and that non-custodial sentences can be at least as effective as custodial ones in preventing offenders' recidivism. This questions the specificity of the estimates found by the above-

if they were to commit another crime in the 5 year-period following their release. Interestingly, the length of offenders' suspended sentence varied depending on inmate's prison entry date, which the researchers argued is exogenous and which they used to measure the impact of suspended sentence length on recidivism.

mentioned studies using a regression discontinuity design. Finally, this paper adds to the already vast literature on impact evaluation methods (Heckman et al., 1999; Blundell and Costa Dias, 2000; Imbens and Wooldridge, 2009). Indeed, my paper provides additional evidence that salient contextual changes (such as a legislative reform, a program scale-up, etc.) can be anticipated by their stakeholders and that, in such a context, traditional quasi-experimental estimators should be used with caution.

The rest of the paper is organized as follows: in section II, I provide information on the reform and the context in which it was implemented; in section III, I provide a description of the data I use; in section IV, I describe the implementation of the reform; in section V, I describe my identification strategy; in section VI, I present my results; finally, in section VII is the conclusion.

II. Background

II.A Drink-driving legislation in Denmark (until 2000)

In the last quarter of the 20th century, the Danish legislation on drink-driving crimes was gradually strengthened with the introduction of an administrative blood alcohol content threshold at 0.08 mg/l in 1976 and until its lowering to 0.05 mg/l in 1998.[9] In turn, these reforms significantly increased the number of individuals who faced and received a prison sentence for a drink-driving crime, as well as the associated costs for the tax-payers.

In 1999, all individuals facing a prison sentence had to be tried in a court of justice (as is still the case) and almost all individuals tried for an alleged drink-driving crime were tried in first instance in one of Denmark's 84 district courts.[10] As displayed in *Table 1,* very few of the defendants tried for a drink-driving crime avoided a conviction. Indeed, only 1.3% of the defendants tried in 1999 managed to avoid all of the legal sanctions displayed in the table. Among those who were sanctioned, the severity of the punishment varied quite substantially depending on the characteristics of the offense (driver's level of impairment at the moment of the crime and whether or not aggravating circumstances could be found[11]) and the number of prior drink-driving convictions. Still, most of the

[9] For comparison purposes, it is worth noting that in Denmark, the minimum drinking age and minimum purchase age for alcoholic beverages are both 18, which is also the minimum licensing age for drivers of private vehicles.

[10] At that time, the vast majority of criminal cases were tried in a district court in first instance. The only few drink-driving cases which were tried in one of the two High Courts in first instance were those for which a prosecutor had requested more than four years of imprisonment. These very rare cases fall out of the scope of this study.

[11] For instance, aggravating circumstances were found when the driver had previously had their driving license suspended, caused personal injury, damaged property, or exposed a person or their property to such injuries or damages through gross disregard of road safety.

individuals tried for an alleged drink-driving crime received an unconditional prison sentence (71.9%).[12] In 95.3% of these cases, the length of the unconditional prison sentence remained below 60 days.[13] Fines and suspensions of the driving license were also frequently used to sanction drink-drivers, as they were requested in 27.8% and 30.8% of the cases respectively. Conversely, conditional prison sentences (probation) and community work were either rarely used or not used at all to sanction drink-drivers.

Offenders who received an unconditional prison sentence usually served their time in one of the country's open prisons[14] – low security prisons where fences, walls, and barriers are minimal, offering softer detention conditions for the inmates.[15] However, offenders suffering from an alcohol abuse problem could request to benefit from a *pardon scheme* as part of which their unconditional prison sentence could be commuted to a two-year probation period and a mandatory participation in a yearlong rehabilitation program (described below).[16,17]

II.B The 2000 reform

In 2000, a reform was passed introducing cheaper, more lenient sentences against drink-drivers.[18] As part of it, unconditional prison sentences of no more than 60 days were replaced by a two-year probation period and a fine, combined with either mandatory participation in a yearlong rehabilitation

[12] For instance, the sanction for a first drink-driving crime could range from a simple fine for offenders with a BAC between 0.05 and 0.08 mg/l to imprisonment for offenders with a BAC over 0.20 mg/l, along with a suspension or revocation of their driving license. Incarceration could also be requested against drivers with lower BAC levels if the offense constituted a repeat offense and/or if aggravating circumstances could be found.

[13] The length of the prison sentence only exceeded 60 days for very severe cases, as displayed in *Table A.1* (placed in the appendix), which describes the sanctions incurred by a defendant based on the nature of its crime.

[14] Individuals serving a prison sentence inferior to four years and presenting no security threats to prison staff and other inmates are usually incarcerated in open prisons.

[15] In practice, prisoners can apply for early release on parole after having served two-thirds of their sentence (as long as more than two months have been served), and most of them are released at that time (Kyvsgaard, 2004). Under exceptional circumstances (such as health reasons, specific conditions in inmates' family, etc.) prisoners may be released after having served half of their sentence but, again, as long as they have served more than two months.

[16] Indeed, from 1990, drink-driving offenders with "a strong need for rehabilitation" who were sentenced to no more than 40 days of incarceration could apply to the Danish Prison and Probation Service to see their unconditional prison sentence commuted into a two-year probation period, starting with mandatory participation in a yearlong rehabilitation program. The sanction came together with a fine and could also be combined with either a suspension or a revocation of offenders' driving license. In 1994, the pardon scheme was extended to drink-driving offenders sentenced to no more than 60 days of incarceration but remained applicable to offenders exhibiting a "strong need for rehabilitation" only. Towards the end of the 1990s, the share of offenders enrolled in the program who were granted a pardon upon successful completion of the program remained between 75 and 80%.

[17] According to the Prison and Probation service, only around 750 offenders were pardoned each year under the pardon scheme (Kriminalforsorgens årsberetning, 1998 and 1999), suggesting that around 70% of drink-driving offenders who received an unconditional prison sentence were incarcerated. Using a different data source, Clausen (2007) estimated that 58.2% of all drink-driving offenders sentenced to no more than 60 days did not benefit from the pardon scheme in the 18-month period preceding the end of the reform. This lower rate does not change the conclusion that a large share of offenders did not benefit from the pardon scheme prior to the change in the legislation in 2000.

[18] Nielsen and Kyvsgaard (2007) reported that, in 2004, the total cost associated with the alternative sanction averaged at 8.300 DKK per offender (including the costs associated with the offenders' supervision and rehabilitation program), while the cost associated with the prison sentence they would have served had they been convicted prior to the reform averaged at 15.800 DKK per offender.

program (identical in every single way to the one offered as part of the pardon scheme just mentioned above) or community service.[19]

The choice of mandatory participation in a rehabilitation program or community service was left to the judges based on whether or not the offender suffered from an alcohol abuse problem, with participation in the rehabilitation program to be requested against offenders exhibiting an alcohol abuse problem. As part of this program, offenders were to take a drug producing an acute sensitivity to ethanol and to participate in an alcohol treatment program.[20] Offenders were monitored throughout the duration of the treatment and the following one-year probation period.[21] Probation officers would verify that the terms of the probation were being respected and, in particular, were to control offenders' drug intake and participation in the alcohol treatment program during the first phase of the scheme.[22] Community service was to be requested against offenders who did not exhibit such an alcohol problem and was substituted to the former sentences at the following rate: 30 hours for 10 to 14 days of imprisonment, 40 hours for 20-30 day sentences, 60 hours for 40 to 50 days in jail.

When interpreting the impact of the reform in light of Becker's deterrent theory (1968), it is important to note that this reform was probably perceived as a softening of the legislation by the vast majority of offenders – as intended by the administration. Indeed, while the incarceration conditions in Scandinavian prisons are considered to be quite exceptional by American and European standards (Lappi-Seppälä, 2007; Pratt, 2008; Pratt and Eriksson, 2011; Ward et al., 2013), inmates remain subject to important freedom restrictions and other usual discomforts associated with imprisonment, even when they are incarcerated in an open prison. Moreover, while the length of the probation period imposed on offenders after the reform is substantial in comparison to the rather short duration of the incarceration spell faced by offenders before the reform, the share of offenders who broke the terms of their parole remained limited, suggesting that few offenders perceived incarceration as preferable to the alternative sanction. In the 6-month period following the reform, only 10.1% of the offenders who were placed on parole were incarcerated at least once in the subsequent 2-year period.

[19] More information on the reform can be found on the following webpage (in Danish): https://www.retsinformation.dk/Forms/R0710.aspx?id=88116

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

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

[22] These information related to the offenders' supervision when placed on probation were found on the following webpage of the Prison and Probation Service's website (accessed on June 1st, 2017): http://www.kriminalforsorgen.dk/Sp%C3%B8rgsm%C3%A5l-om-alternativer-til-frihedsstraf-7089.aspx#FAQ53

The reform did not apply systematically to offenders who had already been placed on probation for a drink-driving crime more than once or to those who were on probation at the time of the crime for an alcohol-related crime. Moreover, the reform also left unchanged the punishments incurred by the offenders facing no prison sentence, as well as by those facing more than 60 days of imprisonment, who kept on serving their prison sentence after the reform (mostly offenders facing extreme aggravating circumstances, as well as extreme repeat drink-drivers).

Finally, as is often the case for important reforms that require a certain level of preparation, a few months elapsed between the moment the law was signed and the moment it entered into force: while the law was signed by the Parliament on April 4th, 2000, it only entered into force on July 1st, 2000 (referred to as *the date of the reform* hereafter).[23]

III. Data[24]

In order to document the impact of the reform, I 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. I use these datasets to identify alleged drink-driving crimes committed and tried around the time of the reform, to compute my outcome variables, and to create the set of control variables I use as covariates (hereafter referred to as "*conditioning set*").

I provide further information on each of the datasets used as part of this study in *Table A.2* (placed in the appendix).

III.A Administrative datasets

Information on crime, charges, and sanctions

These databases include detailed information on individuals' involvement in criminal activities since 1981. In particular, they include information on all crimes reported to the police, including information about the identity of the alleged perpetrators, as well as the date and nature of the *main* crime (in cases where several crimes were reported concomitantly against a single person). They also

[23] For instance, the 1998 law on drink-driving crimes mentioned earlier was signed on June 10th, 1997 but only entered into force on March 1st, 1998.
[24] The administrative registers used as part of this project are the following ones: BEF, FAM, IDAN, IEPE, INDH, KRAN, KRIN, KRSI, and UDDA. Descriptions of the different registers can be found on the following webpage: http://www.dst.dk/da/TilSalg/Forskningsservice/Data/Register_Variabeloversigter

include information on all charges pressed by a prosecutor, including the identity of the individuals against which the charges were pressed, and the date and nature of the *main* charge (in cases where several charges were pressed concomitantly against a single person).[25] 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 (imprisonments, fines, withdrawals, and acquittals) and its severity (fine amounts, conditional and unconditional prison sentence length, etc.).

Unfortunately, in the case of drink-driving crimes, drivers' blood alcohol content at the time of their arrest is not available in the datasets. Furthermore, whether or not an offender was actually incarcerated and the length of their incarceration spell is only imprecisely observed. That is why my analysis uses whether or not an individual received an unconditional prison sentence to evaluate the impact of incarceration. However, I also construct a proxy for whether or not an individual was actually incarcerated and obtain results similar to those displayed below when it is used as my variable of interest. [26,27]

Information on labor market attachment

These databases also include information on all residents' labor market attachment. These labor market information are measured and collected every year in November by Statistics Denmark. I extract individuals' annual number of days worked, as well as their annual earnings before tax and any social contributions (which is inflated to 2015 prices using Statistics Denmark's Consumer Price Index). I use these variables to describe individuals' post-sentencing labor-market attachment.

III.B Outcome variables

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

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[25] 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 a crime. For instance, 60 different codes can be used to categorize drink-driving crimes and charges (29 of which were effectively encountered during the study period).

[26] My proxy for whether or not an individual was incarcerated captures whether an individual spent at least 10 days in prison – 10 days being the minimum duration of prison sentences requested for a drink-driving crime, as displayed in *Table A.1* (placed in the appendix). To account for the fact that the length of the incarceration spell is imprecisely estimated, I also consider as having been incarcerated individuals who I observe were only incarcerated for 9 days.

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

First, I use these administrative records to compute various outcomes indicative of offenders' post-sentencing criminal activity. I start by assessing the relative impact of incarceration and probation on offenders' involvement in subsequent drink-driving crimes. In order to do so, I estimate whether or not individuals were convicted again of another drink-driving crime and if they did the number of such crimes they committed. Then, in order to investigate any criminogenic effect of incarceration, I also compute separate outcomes indicating whether or not individuals were convicted again of any other road traffic crimes or non-road traffic crimes and, in each case, the number of such crimes they committed. I measure the impact of the reform on these outcomes at different time horizons, from 3 months to 10 years, from the date when the drink-driving case was settled in court.[28]

Second, I also use these administrative registers to compute outcomes indicative of individuals' attachment to the labor market at different time horizons following the ruling. More specifically, I focus on the relative impact of incarceration and probation on the annual number of days worked, as well as on annual earnings before tax and any social contributions. I measure the impact of the reform on these outcomes at different time horizons, from 1 to 10 years, from the date when the drink-driving case was settled in court.

III.C Control variables

Finally, I use these registers to compute individuals' pre-crime characteristics, which I use as control variables when estimating the impact of the reform on my outcome variables. More specifically, I use two types of information in my conditioning set. First, I use variables indicative of the characteristics of the trial, such as whether the defendant was a juvenile at the time of the crime and the nature of the main charge (using a detailed 7-digit drink-driving charge code). Second, I also include defendants' background information, such as their gender, age at the time of the trial, immigration status (as per Statistics Denmark's typology: "immigrants", "descendant of immigrants", or "rest of the population"), their past criminal activity (the number of convictions in the 5-year period preceding their crime for other drink-driving crimes, other road traffic crimes, and non-road traffic crimes), marital status, highest educational achievement, type of job held, and annual earnings (before tax and any social contributions).

[28] In order to measure the *net* impact of incarceration, I 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.

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 my sample.[29]

IV. Implementation of the reform

In order to understand how to measure the impact of the reform, I start by studying its implementation. I find that stakeholders (courts of justice and defendants) anticipated the entering into force of the reform and modified their behavior in the preceding weeks. I also find that the identity of the defendants who reacted in anticipation of the reform was not random and that inequities were generated between defendants.

IV.A Anticipations

In *Figure 1*, I describe how the reform was implemented through the evolution of the following four indicators between 1999 and 2001: a) the number of alleged drink-driving crimes resulting in a trial committed every week; b) the number of drink-driving cases tried every week in district courts; c) the share of defendants tried for drink-driving who received an unconditional prison sentence by week of trial; d) the share of defendants tried for drink-driving who were actually incarcerated by week of trial.[30] For each year, the first dotted vertical line marks week 14 (the week when the law was signed in 2000) and the second one marks week 26 (the week when it entered into force in 2000). The only reform implemented during these three years occurred in 2000.[31]

Strikingly, the evolution of these indicators reveals that the handling of the drink-driving cases in district courts changed drastically in the weeks preceding the entering into force of the reform. Indeed, the number of cases tried each week dropped significantly from 90.2 cases on average in the five weeks preceding the signing of the law to 28.0 cases on average during the transition period (after the law was signed but before it entered into force) – representing a 69.0% decrease. This is so despite

[29] The variables included in the conditioning set are all available from 1986.

[30] Again, this variable captures whether an individual has spent at least 10 days in prison – 10 days being the minimum duration of prison sentences requested for a drink-driving crime. To account for the fact that the length of the incarceration spell is imprecisely estimated, I also consider as having been incarcerated individuals who I observe were only incarcerated for 9 days.

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

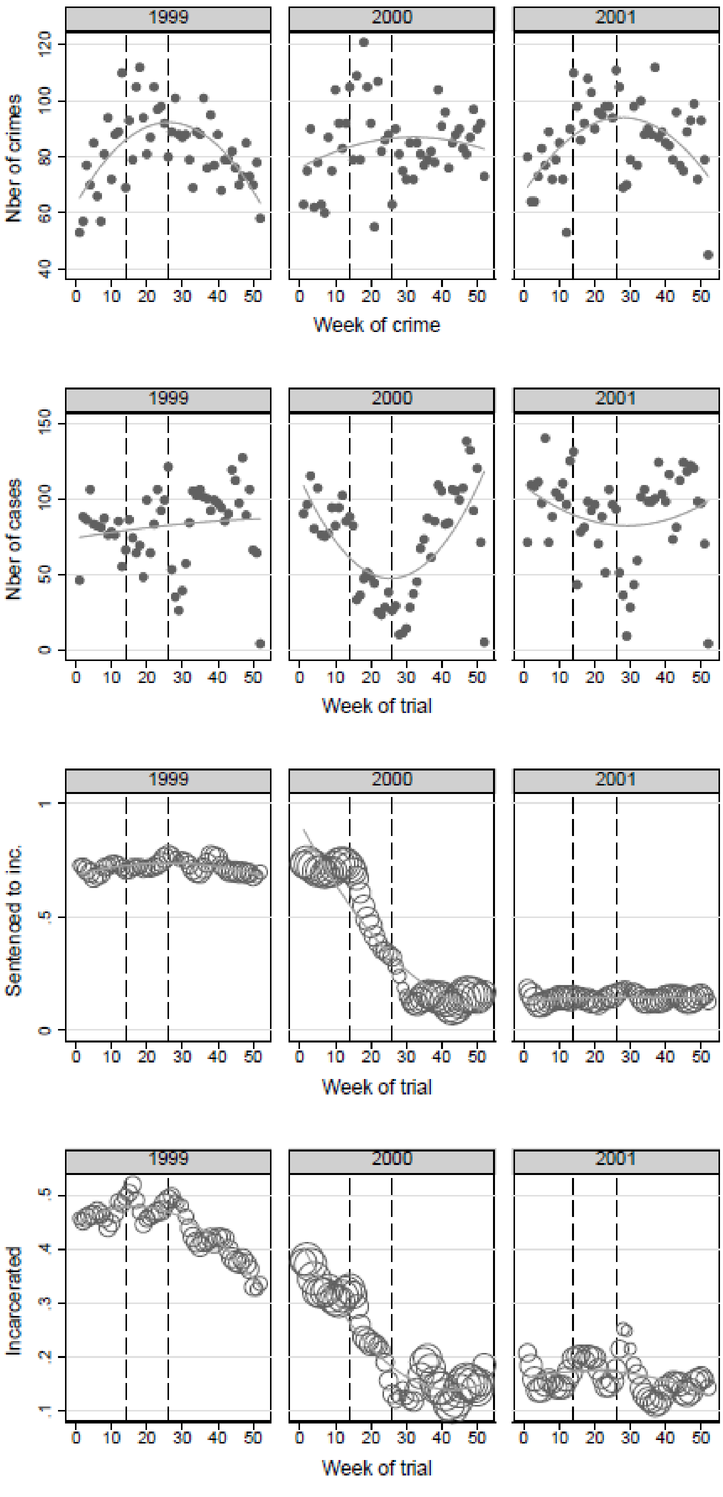

Figure 1 – Implementation of the drink-driving legislation reform

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

the fact that there was no such variation in the number of alleged crimes resulting in a trial committed in the preceding months or in the number of cases tried compared to the same period in adjacent years.[32] This suggests that stakeholders (courts of justice and defendants) anticipated the change in the legislation and that, as a consequence, a large share of trials were postponed until after the reform. In turn, this implies that a group of offenders who should have been tried before the reform was tried after. In total, under the assumption that the same number of drink-driving cases would have been tried between weeks 14 and 26 in 1999, 2000, and 2001, I estimate that roughly 48.1% of the drink-driving cases which should have been tried during the transition period were in fact postponed until after the reform.[33]

The share of drink-drivers receiving an unconditional prison sentence also decreased substantially from the moment the bill was signed, although, this time, the decline did not materialize through a sharp discontinuity but rather through a linear decrease. Overall, the share of defendants receiving an unconditional prison sentence decreased progressively from around 73.6% on average in the three weeks preceding the signing of the law to 34.5% on average in the three weeks preceding the entering into force of the reform – representing a 53.1% decrease. Under the assumption that the same number of drink-driving cases would have been tried between weeks 14 and 26 in 1999 and 2000, and that the type of crimes tried during the two periods would have been similar, I estimate that 61.1% of the drink-driving cases which should have been tried during the transition period and as part of which an unconditional prison sentence would have been rendered were in fact postponed until after the reform.[34]

The share of drink-drivers who were actually incarcerated also decreased substantially from the moment the bill was signed, although from a lower initial level (for reasons detailed above): the share of defendants who were actually incarcerated before the reform is around a third lower than the share of defendants who received an unconditional prison sentence. Interestingly, while the evolution of

[32] The number of cases tried in the week following July 1st is low for all three years. This is a result of judges' summer vacation period, during which the number of cases tried in district courts goes down substantially.
[33] In order to reach this figure, I assume that in the absence of the reform, the number of drink-driving cases tried in 2000 would have been equal to the average number of such cases tried in the same weeks in 1999 (1,071) and 2001 (1,123) – 1,097. However, only 569 drink-driving cases were tried during the transition period, suggesting that around 528 were postponed – which represents 48.1% of what would have been the total number of drink-driving cases tried during that period.
[34] In order to reach this figure, I assume that in the absence of the reform, the number of drink-driving cases resulting in the offender receiving a prison sentence during the transition period would have been equal to the number of such cases tried during the same period in 1999 (782). Only 303 of the drink-driver cases tried during the transition period received an unconditional prison sentence, which suggests that around 478 offenders who should have been tried during the transition period and should have received an unconditional prison sentence did not – representing 61.1% of what would have been the total number of drink-drivers to receive an unconditional prison sentence during that period.

this indicator exhibits a pattern similar to the one described above, it seems that the Prison and Probation Service in charge of enforcing the sanctions rendered by the district courts anticipated the entering into force of the reform even further: the share of individuals incarcerated started to decrease a year before the entering into force of the reform. Again, under the assumption that the same number of drink-driving cases would have been tried between weeks 14 and 26 in 1999 and 2000, and that the type of cases tried during the two periods would have been similar, I estimate that 72.1% of the drink-driving cases which should have been tried during the transition period and as part of which the defendant would have been incarcerated were in fact postponed until after the reform. As a consequence, their defendants avoided prison.[35] Not all drink-drivers benefitted from the same chance as 143 offenders received an unconditional prison sentence during the transition period and were actually incarcerated – 90% of whom received an unconditional prison sentence inferior to 60 days.

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

IV.B Inequities

Additional evidence suggests that the type of defendants who reacted in anticipation of the reform and got their case postponed until after the reform was not random. In turn, this implies that inequities were generated between defendants. In order to investigate this question, I focus on individuals tried between January 1st, 1999 and December 31st, 2000, and regress different variables indicative of

[35] In order to reach this figure, I assume that in the absence of the reform, the number of drink-driving cases resulting in the offender being incarcerated during the transition period would have been equal to the number of such cases tried during the same period in 1999 (513). Only 143 of the drink-driver cases tried during the transition period led to the actual incarceration of the offender, which suggests that around 370 offenders who should have been tried during the transition period and incarcerated, were not – representing 72.1% of what would have been the total number of drink-drivers actually incarcerated during that period.

individuals' criminal priors and labor market attachment on a constant, a dummy variable indicating when a case was tried in 2000, quarter fixed effects, and interactions between the year dummy and the quarter fixed effects.

In *Table 2*, I report the coefficients associated with the year dummy (which capture changes in the composition of the cases tried in the first quarter of 1999 and 2000) and the three interaction terms (which capture differential changes in the composition of the cases tried in the first quarter and those tried in the 2^{nd}, 3^{rd}, and 4^{th} quarters respectively). In particular, the coefficients associated with the interaction term of the year and 2^{nd} quarter dummies allow me to capture differential changes occurring during the transition period (starting the week the reform was signed and ending the week it entered into force). I also report the associated standard errors, which are clustered at the district court and individual level.

While I do not find any evidence that there was a change in the nature of the cases tried in the first and fourth quarters between 1999 and 2000 (columns 1 and 4), I find strong evidence suggesting it did during the 2^{nd} quarter of the year 2000 (column 2). Indeed, I find that the average annual earnings and number of days worked were lower among defendants tried in the 2^{nd} quarter of the year 2000. Similarly, defendants with a full-time job accounted for a smaller share of the individuals tried during this period. Overall, these suggest that wealthier individuals managed to postpone their case until after the reform more frequently than other defendants – presumably because they had access to better legal counsel. I also find that defendants who had at least one prior drink-driving conviction in the previous two years accounted for a smaller share of the individuals tried in the 2^{nd} quarter of the year 2000, while those who had at least one prior conviction for any non-drink-driving crime accounted for a greater share. While these results are more complicated to interpret, they further suggest that the way district courts handled drink-driving cases during the transition period generated differences in the nature of the defendants tried before and after the reform. Furthermore, because individuals with recent drink-driving arrests were among the least likely to be considered eligible for benefitting from the new law, it appears unlikely that the selection mechanism originated from an attempt by the district courts to restrict the set of cases tried before the reform to cases whose outcome did not depend on the entering into force of the reform. Finally, some compositional changes are also observed for the third quarter (column 3), which merely reflect the fact that the number of cases remained lower than usual in the aftermath of the reform.

Generally, these findings question the degree of consistency with which drink-driving cases were handled in district courts and, eventually, the level of equity with which defendants were treated by the justice system during the transition period. From a methodological point of view, this also raises some questions with respect to the performance of traditional quasi-experimental estimators in the context of this reform – a point to which I will come back to at the end of this paper.

V. Identification strategy

V.A The Difference-in-Difference approach

A first natural method to estimating the impact of the reform would consist in using a Difference-in-Difference approach comparing the variation in my outcomes of interest between individuals tried for a drink-driving crime in the 12-month period preceding the reform and those tried in the 12-month period following it.[36]

Such Difference-in-Difference estimates can be obtained through the estimation of the following equation:

$$\Delta y_i^t = \delta^{DiD} PRE_i + X_i\beta + \mu T_i + \mu_m + \mu_c + \varepsilon_i \quad \textbf{(1)}$$

In this equation, Δy_i^t measures the change in the value of the outcome of interest for individual i between a reference period and time t (from 3 months to 10 years after the ruling). For crime outcomes, the value of the outcome in the reference period is equal to 0 given that they are all cumulative outcomes capturing individuals' involvement in criminal activities from the date the sentence is rendered. For labor market outcomes, the value of the outcome in the reference period is equal to the level of these outcomes at the end of the year preceding the trial.

Moreover, PRE_i is a dummy variable taking the value 1 if an individual was tried before the reform and 0 otherwise. Although the reform substituted a probation period for an unconditional prison sentence, this paper focuses on the relative impact of the latter when compared to the former (and not the other way around) for comparison purposes. Moreover, X_i is a vector containing all covariates included in the conditioning set described above, T_i is a time trend, and μ_m are month-of-crime fixed effects allowing me to control for seasonal variations. Finally, μ_c are district court fixed effects, which

[36] A slightly different strategy consists in comparing individuals who committed a crime before a reform with those who committed a similar crime after the reform.

allow me to control for differential characteristics across geographical areas and, in particular, for possible differences in the amount of resources devoted to fight drink-driving crimes across police districts.[37]

The parameter of interest is δ^{DiD}, an Intent-To-Treat (ITT) estimate. It identifies the relative impact of being tried under the old law, as opposed to being tried under the new one, on Δy_i^t. For these parameters to identify the causal impact of the reform, one has to assume that the variation in the outcome across groups of offenders tried before and after the reform would have been identical had the reform not been implemented. The impact of the reform on the subset of offenders who were actually affected by the reform (those who received an unconditional prison sentence of up to 60 days) can be obtained by dividing the ITT estimates by the difference between the share of defendants who received an unconditional prison sentence in the two periods.

Although widely-used, these Difference-in-Difference estimators remain subject to various potential pitfalls. In particular, selection in the nature of the individuals arrested or tried before and after the reform, similar to the one highlighted just above, is likely to introduce a bias in the estimation of the reform's impact.

V.B Alternative approach

Motivation

In order to measure the causal impact of the reform on offenders' post-sentencing criminal activities and labor market attachment, I opt for an alternative strategy which consists in comparing individuals tried for a drink-driving crime committed *before* the reform and differing only by their probability of receiving an unconditional prison sentence. This allows me to bypass the comparability problem which would arise if I was to compare individuals tried before and after the reform for reasons just detailed.[38]

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[37] At the time of the 2000 reform, a police district usually encompassed a few district court jurisdictions.

[38] Traditional quasi-experimental estimators typically raise additional selection problems. In particular, one concern is that the entering into force of the new law might have been accompanied (at least for a time) by more frequent police controls to compensate for the reduction in the expected costs of the punishment through an increase in 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, potential offenders 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 drink-driving crimes. Furthermore, conditional on individuals internalizing changes in the legislation, the reform should also have induced a modification in the characteristics of the individuals arrested for a drink-driving crime after the law was passed. Indeed, the lowering of the cost associated with drink-driving crimes should mechanically have led a new range of individuals to commit drink-driving crimes (those incurring lower benefits from committing a crime and/or higher costs if caught), thereby increasing the overall number of drink-driving crimes.

My approach relies on two features of the justice system which, combined together, create exogenous variation in the probability of incarceration for offenders arrested just before a reform introducing a more lenient legislation for the crime they committed. The first of these two features is the characteristic of Danish legislation mentioned earlier, which 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 (Danish criminal code, §3).[39] Hence, individuals tried after July 1st, 2000 for a drink-driving crime committed before that date were tried under the new law. The second of these two features is the significant time gap between the moment a crime is committed and the moment the decision of justice is rendered by a district court. Around the time of the reform, this time gap for alleged drink-driving crimes was substantial (and almost entirely driven by the case processing time in district courts): on average, 6 months passed between the moment a prosecutor would press charges against an alleged drink-driver and the moment a district court rendered its decision. 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.

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

In *Figure 2,* I investigate the strength of this approach. In order to do so, I organize the data based on the week when the crime was committed (hereafter referred to as "*week of crime*"), instead of the week when the sentence was rendered, and I depict the following indicators (as in *Figure 1*): a) the average time gap between the moment an alleged crime was committed and the moment the decision of justice was rendered by a district court by *week of crime*; b) the share of cases tried after July 1st, 2000 (the date when the reform officially entered into force) by *week of crime*; c) the share of defendants tried for an alleged drink-driving crime who received an unconditional prison sentence by

[39] See Langsted et al. (2014) for an explanation of the functioning of that law (p.36).

week of crime; d) the share of defendants tried for an alleged drink-driving crime who were actually incarcerated by *week of crime*. [40]

The evolution of these indicators suggests that, combined together, these two features introduced significant variation in the probability of receiving an unconditional prison sentence among individuals tried for a drink-driving crime committed in the 12-month period *preceding* the reform, based on the date of their crime. To start with, the average processing time for drink-driving cases varied between 150 and 250 days prior to the reform. As individuals' arrest date got closer to the reform in the 12-month period preceding it, an increasingly larger share of them was tried after, under the new law. Consequently, the share of defendants who received an unconditional prison sentence by week of crime started going down from July 1999 from slightly less than 80% to less than 20% right after the reform. The same pattern is observed for the share of defendants who were actually incarcerated following their trial – although the decrease starts earlier and is more pronounced.

In this approach, the selection in the nature of the offenders tried around the time of the reform should only marginally affects the set of individuals on which the effects are estimated.

Sampling strategy

Building on these observations, I construct a sample of defendants including all individuals charged for a drink-driving crime committed in the 12-month period preceding the entering into force of the reform (between July 1st, 1999 and June 30th, 2000). I also include in my sample individuals charged for a drink-driving crime committed 13 to 24 months before the entering into force of the reform (between July 1st, 1998 and June 30th, 1999), which allows me to control for seasonal variations using both a time trend, year and month fixed effects. Restricting my sample to defendants tried in one of the 84 district courts operating in the country at that time, I obtain a sample of 8,409 cases and defendants (including 8,012 distinct individuals[41]).

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

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

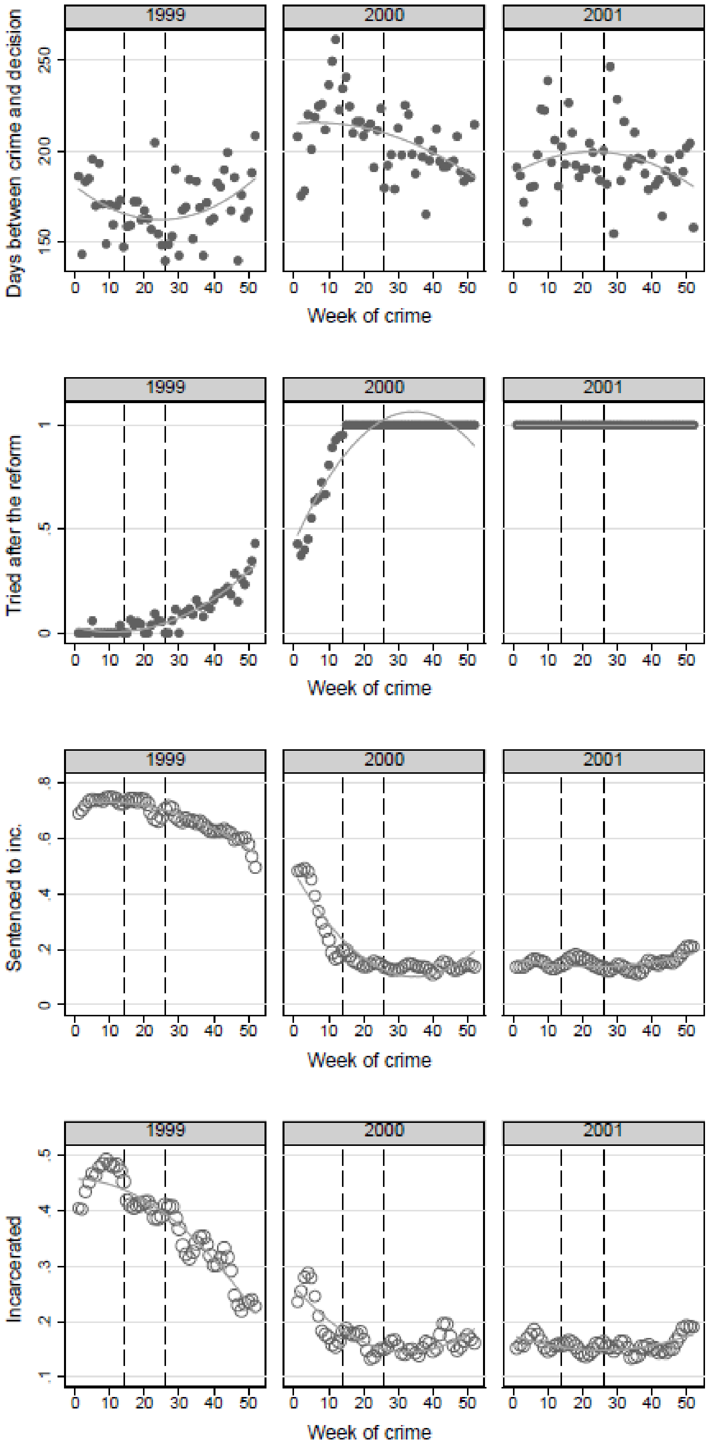

Figure 2 – Motivation for the instrumental variable approach
This figure depicts the evolution of the following four indicators around the time of the reform: a) the average time gap between the moment a crime is committed and the moment the decision of justice is rendered by a district court by *week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

In *Table 3*, I provide a description of the characteristics of the defendants included in my sample. They are predominantly males in their late thirties. While close to 46.6% of them had a permanent full-time job at the end of the year preceding the date of their crime, 42.0% were not working. Moreover, 18.3% of the defendants had already had at least one conviction for a drink-driving crime in the previous 2 years, and 50.0% had already had at least one conviction for a drink-driving crime before that. Few of them were in a partnership (28.2%). Defendants born abroad and descendants of immigrants represent 5.1% and 0.4% of the sample respectively – slightly less than their actual share in the overall population, 5.4% and 1.4% respectively.

Econometric specifications

In order to report on the impact of the reform, I will first show the standard Ordinary-Least-Squares estimates (OLS) I obtain when estimating the following linear model:

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

where $y_{i,t}$ is the crime-related outcome of interest for individual *i* measured at time *t* (from 3 months to 10 years after the ruling); ucd_i is a dummy variable indicating whether individual i received an unconditional prison sentence as part of their trial;[42] X_i is a vector including all variables in the conditioning set; T_i is a time trend indicating the number of days between the moment individual *i* committed their crime and the moment the reform entered into force on July, 1st 2000 (the unit for this variable is 100 days); P_i is a period dummy taking the value 1 if individual i's crime was committed in the 12-month period preceding the reform and 0 if it was committed earlier; μ_m and μ_c are fixed effects indicating the month when individual *i* committed their crime and the district court where they were tried.

The coefficient δ^{OLS} is the parameter of interest. However, for a number of reasons, the ucd_i variable is likely to be endogenous in this specification. Indeed, as displayed in *Table 3*, the offenders who are put on probation and those who receive an unconditional prison sentence differ significantly and, unless all differences across these two groups are controlled for (which seems unlikely to occur), OLS estimators are likely to yield biased estimates.

[42] Again, although the reform substituted a probation period for an unconditional one, this paper focuses on the relative impact of the latter when compared to the former (and not the other way around) for comparison purposes.

Next, I will report the reduced form estimates (RF) derived from the estimation of the following equation:

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

where the $(P_i * T_i)$ variable, which captures the differential effect of the T_i variable (indicating the number of days between the time individual i committed their crime and the time the reform entered into force) for crimes committed in the 12-month period preceding the day the reform entered into force, when compared to crimes committed in the 13 to 24 months before the reform. I use this variable as my instrument, I_i. *Figure 2* suggests that while there is no particular reason to expect μ_1 to be statistically different from 0 in this equation, δ^{RF} might be if the nature of the sanctions imposed on the offenders before and after the reform has an impact on $y_{i,t}$, as the probability of being incarcerated is positively correlated with the time gap between the moment the crime was committed and the entering into force of the reform in the 12-month period preceding it.

Finally, I will report the instrumental variable (IV) estimates obtained by instrumenting ucd_i by my instrument I_i using a Two-Stage-Least-Squares estimation procedure. The coefficient δ^{IV} measures the impact of receiving an unconditional prison sentence (as opposed to a probation period) on *compliers*, the subset of defendants for whom the time of their crime in the 12-month period preceding the entering into force of the reform had an impact on whether or not they were incarcerated – *i.e.* offenders who were sentenced to serve 1 to 60 days in prison.

Variation in defendants' probability of being incarcerated

In *Table 4*, I estimate the impact of having committed a drink-driving crime closer to the date of the reform on the probability for a defendant to receive an unconditional prison sentence (*Panel A*) and a proxy for whether or not a defendant was actually incarcerated (*Panel B*).

In order to do so, I regress the binary variable indicative of the trial outcome on my instrument and an increasingly exhaustive set of control variables. From column (1) to column (4), I enrich the set of control variables by adding the following covariates successively and incrementally: a time trend, period, month-of-crime and district court fixed effects (column 1), dummy variables indicative of the exact nature of the drink-driving charge (column 2), information about the criminal case (column 3), and defendant characteristics (column 4).

As expected, I find that having committed a crime closer to the date of the reform substantially reduces the probability for a defendant to receive an unconditional prison sentence when the crime was committed in the 12-month period preceding the entering into force of the reform. Indeed, in that period, delaying their drink-driving crime by 100 days would have reduced defendants' probability of receiving an unconditional prison sentence by 19.9 percentage points. Similarly, it would have decreased their probability of actually being incarcerated by 5.5 percentage points.[43]

Furthermore, both the magnitude and significance level of these estimates are robust to the inclusion of covariates in the regression, suggesting that, in the 12-month period preceding the entering into force of the reform, the time gap between the day a defendant supposedly committed their crime and the moment the reform entered into force is independent of their characteristics and those of their case.

Instrument validity

These results suggest that the number of days separating the moment an alleged drink-driving crime was committed in the 12-month period preceding the reform and the date of the reform may be used as an instrument to capture the impact of the reform (ITT estimates) and to measure the impact of substituting a probation period for an unconditional prison sentence (TOT estimates).

Still, for this instrument to be valid, it has to meet the following standard conditions: independence, exclusion, and monotonicity. While the independence and exclusion assumptions are crucial for both ITT and TOT estimates, the monotonicity assumption only matters for TOT estimates.

a) Independence

The independence assumption implies that the instrument is independent of defendants' background characteristics and potential outcomes (once a time trend, period, month-of-crime and district court fixed effects are controlled for).

In order to further investigate the validity of this assumption, I study whether or not defendants' pre-crime and case characteristics are correlated with the instrument. I do so by regressing each of the

[43] The impact of the time gap between the day the reform entered into force and the moment the crime was committed in the 12-month period preceding the reform is actually slightly underestimated when the proxy for whether or not a defendant was actually incarcerated following their trial is used as the dependent variable. Indeed, the share of offenders actually incarcerated started going down in the reference period, more than a year before the entering into force of the reform (as displayed in *Figure 2*). Regressing my proxy for whether or not a defendant was incarcerated on a constant, district court fixed effects, dummy variables indicative of the exact nature of the drink-driving charge, information on the criminal case, and defendant characteristics yields a first-stage coefficient of 0.073 (and a standard error of 0.008).

background variables displayed in the left column of *Table 3* on the instrument, the time trend, period, month-of-crime and district court fixed effects. For each regression, I report the coefficients and standard errors associated with the instrument in *Table 3*.

I find that the coefficients associated with the instrument are systematically small and largely insignificant, suggesting that the independence assumption is likely to be met.

b) Exclusion

The exclusion restriction implies that the timing of the crime *itself* does not have any direct impact on my outcome variables (defendants' crime and labor outcomes up to ten years after the completion of their trial).

One concern is that the risk of recidivism and/or prospects of employment might vary across defendants based on the timing of their crime or the date of their sanction. However, the inclusion in my sample of individuals tried for a drink-driving crime committed 13 to 24 months before the reform allows me to mitigate the consequences of this potential problem by controlling for trend and seasonality effects.

c) Monotonicity

The monotonicity assumption implies that the probability of receiving an unconditional prison sentence decreased for all offenders as their crime was committed closer to the reform in the 12 months preceding it.

In order to investigate the validity of this assumption, I estimate the first-stage equation for various subgroups of the sample: males, females, individuals aged below 25, individuals aged above 25, individuals with prior drink-driving convictions, individuals without any prior drink-driving convictions, etc. I report the coefficients and standard errors associated with each of the subgroups in *Table A.3 (*placed in the appendix). I find that the coefficients are all positive and statistically significant (as well as very similar in magnitude). This suggests that problems arising due to non-monotonicity are probably limited as well.

VI. Results

I measure the impact of the reform on individuals' post-sentencing crime and labor market outcomes using the strategy described in the previous section. My main estimates are displayed in *Tables 5.A-*

5.C (crime outcomes) and in *Table 6* (labor market outcomes). Additional IV estimates are displayed in *Figures 3.a*, *3.b*, and *4*.

In each table, OLS estimates are reported in columns (1) and (2), RF estimates in columns (3) and (4), and IV estimates in columns (5) and (6). For each category of estimates, the first and second columns only differ in the set of covariates included in the estimated equation: in the former case, I only include the time trend, the period dummy (indicating whether or not a crime was committed in the 12-month period preceding the reform), month-of-crime and district court fixed effects; in the latter case the whole conditioning set is added as well (again, this set provides information on the nature of the criminal case and defendants' background characteristics).

For comparison purposes, I also report the estimates obtained using the Difference-in-Difference approach detailed earlier, displayed in *Tables 7.A-7.C* (crime outcomes) and in *Table 8* (labor market outcomes). In these tables, I also indicate the estimates obtained when attempting to solve the selection issue by discarding from the sample drink-driving cases tried within 3 and 6 months of the reform (using the so-called "donut-hole" approach). The whole conditioning set is systematically added to the estimated equation.

VI.A Effects of incarceration on crime outcomes

I assess the relative impact of incarceration as compared to probation on drink-drivers' post-sentencing criminal activities, as measured by their probability of being convicted of another drink-driving crime, any other road traffic crimes, and any non-road traffic crimes (the *extensive* margin). For each type of crime, I also estimate the relative impact of incarceration on the number crimes committed (the *intensive* margin). All of these outcomes are measured from the date the sentence was rendered.

In *Table 5.A*, I report on the impact of incarceration on drink-driving crimes. All estimates (OLS, RF, and IV) suggest that incarceration does not have any impact on offenders' probability of committing any drink-driving crimes, or on the number of drink-driving crimes they commit. Indeed, all coefficients are close to 0 and none but one is statistically significant at the 5% level.

In *Tables 5.B* and *5.C*, I study whether or not incarceration has any criminogenic effect by measuring its impact on the likelihood that offenders commit any another road traffic crimes or non-road traffic crimes, as well as on the number of such crimes they commit. OLS estimates suggest that there is a negative relationship between incarceration and these other crime outcomes, both at the extensive and intensive margins. Indeed, most of these coefficients are negative and statistically significant at the 5% level. For instance, they suggest that incarceration decreases the probability for an offender to commit any other road traffic crimes by 5.0 percentage points within 5 years (representing a 17.5% decrease at the mean) and reduces the number of such crimes they commit by 0.109 crime (representing a 25.3% decrease at the mean) – both results are statistically significant at the 5% level.

However, as already mentioned earlier, these estimates are likely to be biased given that it is unlikely that my specification controls for all differences between the offenders who received an unconditional prison sentence and the offenders who received a conditional one. Indeed, RF and IV estimates provide a completely different picture, which suggests that incarceration has no effect on non-road-traffic crimes but fosters other road-traffic crimes. In the case of other road traffic crimes, coefficients increase in the five-year period following the sentencing, at which point they indicate that incarceration increases the probability for an offender to commit any other road-traffic crimes by 9.8 percentage points (representing a 34.4% increase at the mean) and the number of such crimes they commit by 0.163 crimes (representing a 37.9% increase at the mean) – both results are statistically significant at the 5% level. In the case of non-road traffic crimes, point estimates are small in magnitude and not statistically significant, suggesting that incarceration does not have any impact on the probability for offenders to commit any non-road-traffic crimes or on the number of such crimes they commit.

These results contrast with the limited available evidence about the impact of legal sanctions on drink-drivers' involvement in subsequent criminal activities. Indeed, although not entirely comparable due to differences in the alternative to incarceration or in the nature of the outcome used, prior evidence suggest that harsher punishments tend to reduce offenders' probability of committing another drink-driving crime (Hansen, 2015), and that incarceration is more effective than probation in reducing the number of road traffic crimes and other types of crimes committed (Hinnerich et al., 2016). My results also contrast with theoretical predictions from the standard deterrence theory (Becker, 1968) which, in line with Hansen's findings, would have predicted that substituting a milder probation period for an unconditional prison sentence would foster offenders' subsequent criminal activities. On the contrary, my results suggest that conditional prison sentences are as effective as unconditional ones at preventing future crimes, and that unconditional prison sentences (even short ones) have some criminogenic effects.

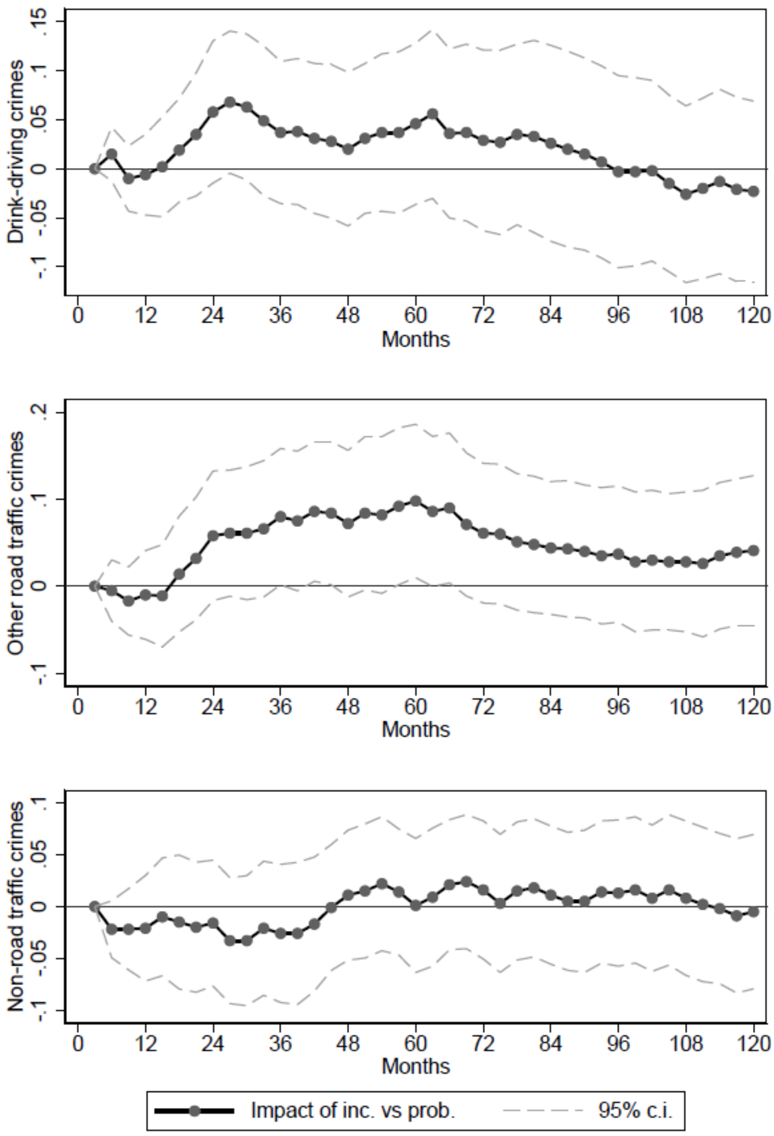

Figure 3.a – Impact of incarceration vs. probation on crime outcomes (extensive margin)

This figure depicts the cumulative impact of incarceration (measured by my IV estimates) on the following outcomes: a) the probability of being convicted of a drink-driving crime; b) the probability of being convicted of any other road traffic crime; and c) the probability of being convicted of any non-road traffic crime. I measure crime outcomes every 3 months from the date when the drink-driving case was settled in court.

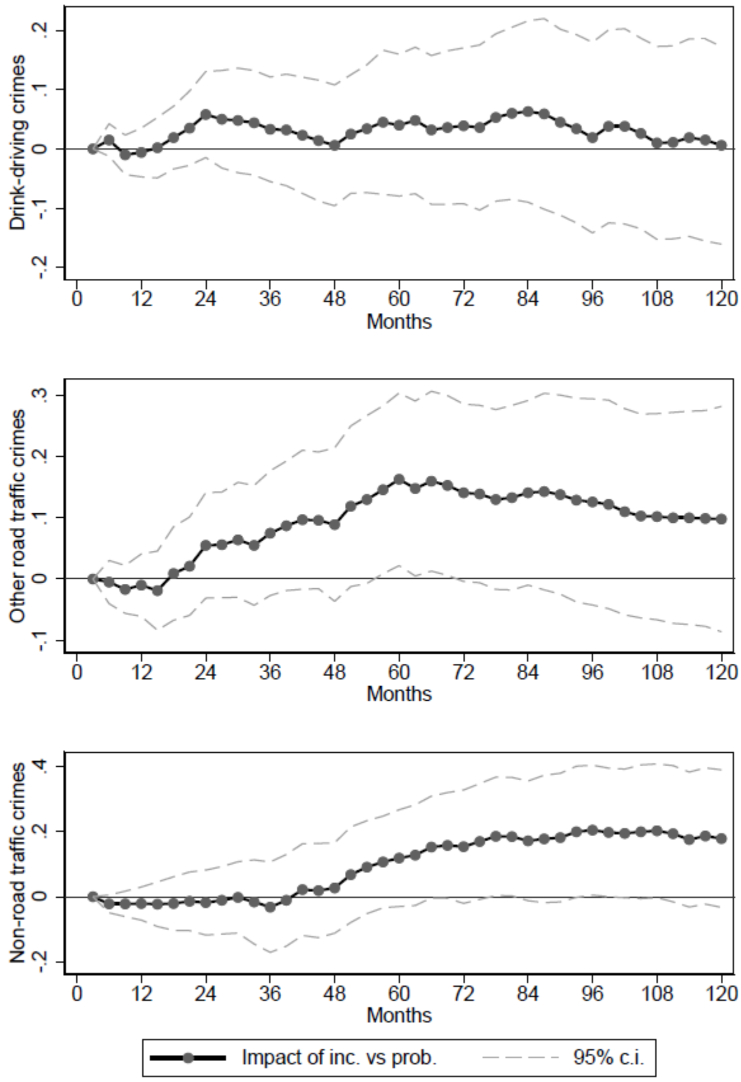

Figure 3.b – Impact of incarceration vs. probation on crime outcomes (intensive margin)

This figure depicts the cumulative impact of incarceration (measured by my IV estimates) on the following outcomes: a) the number of drink-driving crimes committed; b) the number of other road traffic crimes committed; and c) the number of non-road traffic crimes committed. I measure crime outcomes every 3 months from the date when the drink-driving case was settled in court.

Finally, I investigate the performance of the Difference-in-Difference estimators described above. As displayed in *Tables 7.A-7.C,* these estimates suggest that incarceration has no impact on any of the investigated outcomes: coefficients are all close to 0 (including TOT estimates) and are rarely statistically significant at the 5% level. Overall, while the conclusions drawn from the Difference-in-Difference estimates and from my instrumental variable approach are similar with respect to the impact of incarceration on drink-driving and non-road traffic crimes, they differ in some other dimensions, in particular in relation to other road traffic crimes.

VI.B Effects of incarceration on labor market outcomes

Finally, I assess the relative impact of incarceration as compared to probation on offenders' subsequent labor market outcomes, which I display in *Table 6*. More specifically, I measure the impact of incarceration on the annual number of days worked (*Panel A*), and annual earnings before tax and any social contributions (*Panel B*).

Very strikingly, both the number of days worked and earnings decrease from the moment the decision of justice is rendered. For instance, the number of days worked decreases from 129.5 days at the end of the year following the trial to 69.2 days 10 years after. While part of this evolution is mechanically driven by individuals entering retirement, this pattern also seems to suggest that being convicted of a drink-driving crime adversely affects offenders' labor market attachment.

Regarding the relative impact of incarceration and probation, while OLS estimates suggest that incarceration does not have any impact on any of these two outcomes, RF and IV estimates suggest that receiving an unconditional prison sentence significantly weakens individuals' labor market attachment and does so on a long-term basis. Indeed, despite the limited length of the incarceration spells, offenders who experience incarceration work significantly fewer days every year from the moment they receive their unconditional prison sentence than those who were only placed on probation. More, this result holds for almost every year following the decision of justice. As a consequence, they also earn much less every year. For instance, 5 years after the decision of justice, individuals who received an unconditional prison sentence work on average 40.6 days less than individuals who received a conditional prison sentence – representing a 34.5% decrease at the mean. They also earn 31,519 kroners less than individuals who were placed on probation – representing a 24.4% decrease at the mean.

It is interesting to note that differences in individuals' labor market attachment between offenders who received a prison sentence and those who were placed on probation are negative but remain small in the first two years following the decision of justice. However, they widen from the 3rd year, which marks the end of the probation period for most of the offenders who received a conditional prison sentence. These observations suggest that probation constitutes an important disruption in individuals' lives, which negatively affects their integration on the labor market, but that its longer term effects are relatively less serious than those associated with an incarceration spell (even a short one).

Finally, incarcerated offenders' labor market attachment seems to relatively improve towards the end of the period (around 8 years after the decision of justice) before worsening again at the end of the period studied. While this pattern remains puzzling, it is interesting to note that, for most offenders in the sample, the 9th and 10th years after the decision of justice correspond to 2009 and 2010, which were marked by a financial crisis and a sharp rise in the unemployment rate.[44] In turn, although further research is needed before any definitive conclusions can be drawn, these observations seem to suggest that while the negative impact of incarceration on offenders' employment prospects might fade out with time, they remain more liable to economic downturns.

As displayed in *Table 8*, Difference-in-Difference estimators all provide a completely different picture, suggesting that individuals who received an unconditional prison sentence worked more and earned more. Implementing donut-hole approaches does not improve the estimates.

[44] The unemployment rate evolved as follows in Denmark between 2007 and 2011: 3.8% in 2007, 3.4% in 2008, 6,0% in 2009, 7.5% in 2010, and 7.6% in 2011.

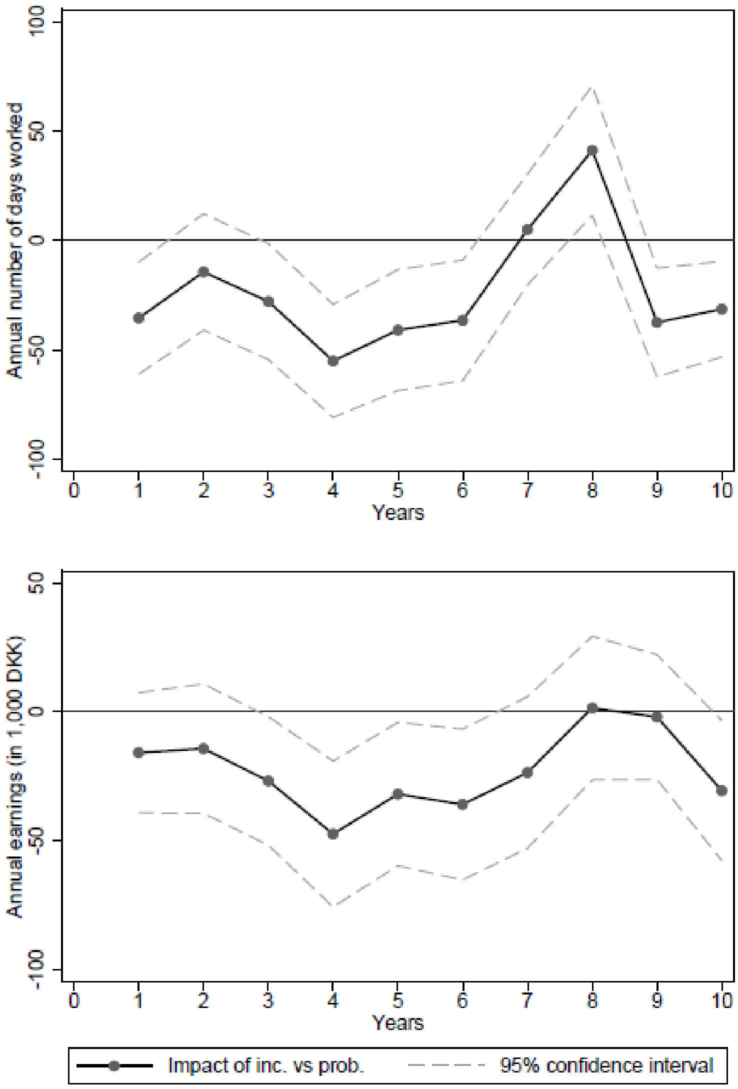

Figure 4 – Impact of incarceration vs. probation on labor outcomes
This figure depicts the impact of incarceration (measured by my IV estimates) on the following two outcomes: a) annual number of days worked; b) annual earnings before tax and any social contributions. I measure labor outcomes every year starting at the end of the year following the date when the drink-driving case was settled in court.

VII. Conclusion

In this article, I study a large-scale reform of the Danish legislation implemented in 2000, whereby jail time was replaced by a probation period (combined with a fine and either a mandatory participation in a yearlong rehabilitation program or community work) for most drink-driving crimes. The study reaches several conclusions.

First, I 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 drink-driving legislation studied here, I show that the way drink-driving cases were handled in district courts changed drastically in the weeks preceding the entering into force of the reform (from the moment the law was signed) and that a large share of the cases which should have been tried before the reform was actually tried after. This implies that a group of offenders who should have been tried before the reform were tried after and, as a consequence, avoided prison. Furthermore, I show that the identity of the individuals who had their case postponed was not random and that, for instance, wealthier defendants were more likely to have their trial delayed until after the reform. This evidence adds to the growing literature documenting inconsistencies in the functioning of justice systems (Danziger et al., 2011; Vidmar, 2011; Anwar et al., 2012; Anwar et al., 2014; Philippe and Ouss, 2018). From a policy perspective, it suggests that it would be advisable to synchronize the passing and entering into force of a new law or, whenever possible, to monitor more closely the way cases are handled in such periods so as not to introduce any avoidable source of inequities in the justice system.

Second, I show that in contrast with prior available evidence and standard predictions from the deterrence theory, substituting a probation period for an unconditional prison sentence did not have any impact on offenders' probability of committing another drink-driving crime or on the number of drink-driving crimes committed up to 10 years after the decision of justice. Moreover, while this change did not have any impact either on non-road traffic crimes, incarceration appears to increase the probability for a defendant to commit other road traffic crimes and the number of such crimes they commit. In addition, incarceration appears to have a strong negative effect on offenders' labor market attachment, reducing both their annual number of days worked and their annual earnings. Although the peculiar nature of the compliers on which the impact of the reform is estimated rules out any definitive conclusions, my findings suggest that drink-drivers do not always respond to the incentives provided by the legislation in accordance with the deterrence theory and that non-custodial sentences can be at least as effective (and much cheaper) as custodial ones in preventing recidivism.

Finally, I provide additional evidence of the inconstant performance of Difference-in-Difference estimators in the presence of selection issues. Indeed, I find that Difference-in-Difference estimators can reach conclusions that are very different from those discussed above. For instance, they suggest that incarceration improves offenders' labor market attachment. From a methodological perspective, this also draws attention to stakeholders' potential reactions to salient changes in their environment and further stresses that traditional quasi-experimental estimators should be used with caution (including those implementing a donut-hole approach).

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Tables & Figures

Table 1: Drink-driving trial outcomes in cases tried in 1999

	Drink-driving crimes tried in 1999		
	#Obs.	Mean	S.d.
No sanction	4,249	0.013	0.115
Prison	4,249	0.726	0.446
Prison, cond.	4,249	0.007	0.085
Prison, cond. (length)	4,249	0.199	3.169
Prison, uncond.	4,249	0.719	0.449
Prison, uncond. (length)	4,249	18.129	25.402
Between 1 and 60 days	4,249	0.685	0.465
Over 60 days	4,249	0.052	0.221
Imprisoned	4,249	0.437	0.496
Community work	4,249	0.000	0.000
Fine	4,249	0.278	0.448
Fine amount	4,249	1624.441	3109.778
Driv. lic. suspended	4,249	0.308	0.462
Driv. lic. suspended (length)	4,249	1.851	2.774
Appeal	4,249	0.017	0.128

Notes: In this table, I provide a description of the trial outcomes of the drink-driving cases tried in 1999.

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

	Whole sample			Year 2000			Weeks 14-26			Weeks 27-39			Weeks 40-52		
Variables	#Obs.	Mean	S.d.	Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.	
Criminal background															
Nber of past convict.	7,894	4.514	4.772	0.092	0.828		0.659	0.349	*	-0.345	0.299		0.137	0.281	
0-2 years priors															
Any past convict.	7,894	0.407	0.491	0.018	0.073		0.027	0.033		-0.010	0.028		-0.042	0.031	
Nber of past convict.	7,894	0.647	0.962	0.019	0.184		0.064	0.067		0.001	0.062		-0.081	0.062	
Any past DD convict.	7,894	0.177	0.382	0.003	0.056		-0.041	0.020	**	-0.021	0.024		-0.007	0.019	
Nber of past DD convict.	7,894	0.196	0.441	0.012	0.063		-0.047	0.023	**	-0.019	0.028		-0.016	0.019	
>2 years priors															
Any past convict.	7,894	0.793	0.405	-0.004	0.082		0.060	0.027	**	-0.007	0.029		0.044	0.020	**
Nber of past convict.	7,894	3.869	4.378	0.076	0.724		0.594	0.306	*	-0.348	0.273		0.214	0.248	
Any past DD convict.	7,894	0.491	0.500	-0.042	0.078		0.000	0.031		-0.070	0.043		0.032	0.031	
Nber of past DD convict.	7,894	0.849	1.153	0.014	0.177		0.007	0.074		-0.201	0.091	**	0.050	0.067	
Attachment to the labor market															
Active	7,842	0.756	0.429	0.019	0.067		-0.019	0.027		-0.001	0.030		-0.027	0.024	
Employed	7,842	0.645	0.479	0.001	0.083		-0.024	0.031		-0.012	0.036		-0.028	0.028	
Unemployed	7,842	0.112	0.315	0.018	0.050		0.005	0.018		0.011	0.020		0.001	0.018	
Full-time employ. (>29h/week)	7,894	0.469	0.499	0.072	0.076		-0.086	0.032	***	-0.086	0.036	**	-0.031	0.026	
Part time employ. (15-29h/week)	7,894	0.053	0.223	0.026	0.037		0.048	0.018	***	0.043	0.016	***	0.009	0.015	
Limited empl. <15h/week)	7,894	0.029	0.169	-0.039	0.025		-0.004	0.010		0.007	0.011		0.001	0.009	
Employ. w/ partial unempl. during the year	7,894	0.009	0.096	-0.015	0.017		0.006	0.006		0.004	0.007		0.005	0.006	
Unknown	7,894	0.022	0.146	0.005	0.024		0.013	0.010		0.012	0.008		0.000	0.009	
None	7,894	0.418	0.493	-0.049	0.082		0.023	0.030		0.020	0.038		0.016	0.026	
Nber of days worked	7,894	149.648	162.488	19.187	26.033		-21.613	9.736	**	-8.333	12.244		-12.065	9.599	
Earnings	7,894	160.407	159.095	13.006	24.795		-25.605	9.994	**	-18.280	13.221		-10.984	11.020	

Notes: In this table, I describe the characteristics (mean and standard deviation) of the defendants tried between January 1st, 1999 and December 31st 2000. I 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, I regress 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. I report the coefficient and standard error associated with the year dummy and the interaction variables. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Sample description and balance checks

Variables	Whole sample #Obs.	Mean	S.d.	Instrument Coeff.	s.e.	
Defendant characteristics						
Female	8,409	0.084	0.278	0.001	0.006	
Juvenile, when the crime was committed	8,409	0.003	0.058	0.001	0.001	
Age when the decision was rendered	8,409	38.291	11.068	-0.260	0.227	
Immigration status						
Immigrant	8,409	0.051	0.220	-0.007	0.004	
Descendant	8,409	0.004	0.063	0.000	0.001	
Rest of the population	8,409	0.945	0.228	0.007	0.004	
Family status						
Single	8,409	0.498	0.500	-0.001	0.011	
In partnership	8,409	0.282	0.450	0.003	0.009	
Separated	8,409	0.194	0.396	0.002	0.007	
Widow	8,409	0.014	0.117	0.001	0.003	
Unknown	8,409	0.012	0.109	-0.006	0.003	**
Education status						
Months of educ. required to obtain diploma	8,100	141.971	30.709	0.109	0.675	
Primary education	8,409	0.527	0.499	-0.004	0.011	
Secondary education	8,409	0.379	0.485	0.001	0.010	
Higher education	8,409	0.059	0.235	0.001	0.005	
Unknown	8,409	0.035	0.185	0.003	0.004	
Attachment to the labor market						
Earnings	8,409	160.297	158.905	2.482	3.500	
Nber of days worked	8,409	149.815	162.686	1.518	3.535	
Full-time employ. (>29h/week)	8,409	0.466	0.499	0.013	0.010	
Part time employ. (15-29h/week)	8,409	0.053	0.224	0.001	0.005	
Limited empl. <15h/week)	8,409	0.030	0.171	-0.003	0.003	
Employ. w/ partial unempl. during the year	8,409	0.010	0.100	-0.003	0.002	
Unknown	8,409	0.022	0.146	0.004	0.003	
None	8,409	0.420	0.494	-0.013	0.010	
Criminal case characteristics						
Mixed court trial	8,409	0.033	0.179	-0.005	0.003	
Nber of past convict.	8,409	4.587	4.847	-0.020	0.094	
0-2 years priors						
Any past convict.	8,409	0.414	0.493	0.004	0.011	
Nber of past convict.	8,409	0.659	0.966	0.008	0.020	
Any past DD convict.	8,409	0.183	0.387	0.002	0.008	
Nber of past DD convict.	8,409	0.202	0.446	0.002	0.009	
>2 years priors						
Any past convict.	8,409	0.794	0.405	0.004	0.008	
Nber of past convict.	8,409	3.930	4.453	-0.027	0.083	
Any past DD convict.	8,409	0.500	0.500	0.006	0.010	
Nber of past DD convict.	8,409	0.861	1.155	0.020	0.023	

Notes: In this table, I describe the characteristics (mean and standard deviation) of the set of defendants included in my sample and report how defendants' characteristics are correlated with the instrument. The estimates describing the differential characteristics are obtained by regressing each of the variables displayed in the left column of the table on a constant, my instrument, 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, and month of crime and district court fixed effects. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Reform and unconditional prison sentence

	(1)	(2)	(3)	(4)
Section A: Probability of receiving an unconditional prison sentence				
Instrument	0.200***	0.199***	0.199***	0.199***
	(0.011)	(0.010)	(0.010)	(0.010)
Mean	0.574	0.574	0.574	0.574
Observations	8,409	8,409	8,409	8,409
R-squared	0.217	0.234	0.285	0.296
Section B: Probability of being incarcerated				
Instrument	0.055***	0.055***	0.055***	0.055***
	(0.009)	(0.008)	(0.008)	(0.008)
Mean	0.346	0.346	0.346	0.346
Observations	8,409	8,409	8,409	8,409
R-squared	0.077	0.085	0.098	0.109
Trend	YES	YES	YES	YES
Period FE	YES	YES	YES	YES
Month of crime FE	YES	YES	YES	YES
District court FE	YES	YES	YES	YES
Charge FE	NO	YES	YES	YES
Case Charact.	NO	NO	YES	YES
Def. Charact.	NO	NO	NO	YES

Notes: In this table, I measure the impact of my instrument (the number of days between the moment an individual committed their crime and the time the reform entered into force, in 100 days) on the probability for them to receive an unconditional prison sentence (*Panel A*) and to be actually incarcerated (*Panel B*). For each of these outcomes, I regress the dependent variable on my 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 exact nature of the charge are added as well; in column (3), I add information on the criminal case; in column (4), I add defendant characteristics. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 5.A: Impact on drink-driving crimes

	Whole sample			OLS						RF				IV			
				(1)			(2)			(3)		(4)		(5)		(6)	
	N	Mean	S.d.	Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
Panel A: Probability of being convicted again for drink-driving																	
Within 6 months	8,409	0.014	0.119	0.007	0.003	**	0.006	0.003	**	0.003	0.003	0.003	0.003	0.015	0.013	0.015	0.014
Within 1 year	8,409	0.045	0.207	0.014	0.006	**	0.006	0.006		-0.001	0.004	-0.001	0.004	-0.007	0.021	-0.006	0.022
Within 2 years	8,409	0.105	0.307	0.020	0.008	**	0.006	0.009		0.011	0.008	0.012	0.008	0.055	0.037	0.059	0.037
Within 5 years	8,409	0.242	0.428	0.015	0.011		-0.004	0.011		0.010	0.009	0.009	0.009	0.050	0.041	0.046	0.042
Within 10 years	8,409	0.355	0.478	0.026	0.012	**	0.004	0.012		-0.002	0.010	-0.005	0.010	-0.009	0.048	-0.024	0.047
Panel B: Number of drink-driving convictions																	
Within 6 months	8,409	0.014	0.119	0.007	0.003	**	0.006	0.003	**	0.003	0.003	0.003	0.003	0.015	0.013	0.015	0.014
Within 1 year	8,409	0.045	0.207	0.014	0.006	**	0.006	0.006		-0.001	0.004	-0.001	0.004	-0.007	0.021	-0.006	0.022
Within 2 years	8,409	0.105	0.307	0.020	0.008	**	0.006	0.009		0.011	0.008	0.012	0.008	0.055	0.037	0.059	0.037
Within 5 years	8,409	0.316	0.628	0.045	0.016	***	0.012	0.016		0.009	0.013	0.008	0.013	0.042	0.060	0.040	0.061
Within 10 years	8,409	0.539	0.882	0.086	0.023	***	0.037	0.023		0.004	0.019	0.001	0.017	0.022	0.091	0.005	0.085
Trend				YES			YES			YES		YES		YES		YES	
Period FE				YES			YES			YES		YES		YES		YES	
Month-of-crime FE				YES			YES			YES		YES		YES		YES	
District court FE				YES			YES			YES		YES		YES		YES	
Additional Cov.				NO			YES			NO		YES		NO		YES	
F-test																367.899	

Notes: In this table, I report: Ordinary Least Squares (OLS) estimates derived from the regression of the outcome variable on a dummy variable indicating whether or not individual i received an unconditional prison sentence (equation 2); Reduced-Form (RF) estimates derived from the regression of the outcome variable on my instrument (equation 3); finally, I report the Instrumental Variables (IV) estimates obtained by instrumenting the dummy variable indicating whether or not individual i received an unconditional prison sentence by my instrument. For each category of estimates, I first report estimates obtained when only 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, and month of crime and district court fixed effects are added to the regression, and, second, those obtained when covariates are also added. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 5.B: Impact on other road traffic crimes

	Whole sample			OLS						RF						IV					
				(1)			(2)			(3)			(4)			(5)			(6)		
	N	Mean	S.d.	Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.	
Panel A: Probability of being convicted for a road traffic crime (excluding drink-driving)																					
Within 6 months	8,409	0.031	0.173	-0.010	0.004	***	-0.009	0.004	**	-0.001	0.004		-0.001	0.004		-0.003	0.018		-0.005	0.018	
Within 1 year	8,409	0.078	0.268	-0.027	0.009	***	-0.026	0.009	***	-0.001	0.005		-0.002	0.005		-0.006	0.025		-0.010	0.026	
Within 2 years	8,409	0.154	0.361	-0.035	0.012	***	-0.039	0.013	***	0.013	0.008		0.012	0.008		0.064	0.039	*	0.058	0.038	
Within 5 years	8,409	0.285	0.451	-0.046	0.013	***	-0.050	0.013	***	0.023	0.010	**	0.019	0.009	**	0.114	0.049	**	0.098	0.045	**
Within 10 years	8,409	0.391	0.488	-0.056	0.011	***	-0.057	0.011	***	0.013	0.010		0.008	0.009		0.065	0.050		0.041	0.044	
Panel B: Number of convictions for road traffic crimes (excluding drink-driving)																					
Within 6 months	8,409	0.031	0.173	-0.010	0.004	***	-0.009	0.004	**	-0.001	0.004		-0.001	0.004		-0.003	0.018		-0.005	0.018	
Within 1 year	8,409	0.078	0.268	-0.027	0.009	***	-0.026	0.009	***	-0.001	0.005		-0.002	0.005		-0.006	0.025		-0.010	0.026	
Within 2 years	8,409	0.181	0.450	-0.049	0.014	***	-0.050	0.014	***	0.013	0.009		0.011	0.009		0.063	0.045		0.055	0.044	
Within 5 years	8,409	0.430	0.810	-0.109	0.021	***	-0.107	0.023	***	0.037	0.016	**	0.033	0.015	**	0.185	0.079	**	0.163	0.072	**
Within 10 years	8,409	0.723	1.169	-0.199	0.030	***	-0.178	0.031	***	0.028	0.022		0.020	0.019		0.138	0.109		0.098	0.094	
Trend				YES			YES			YES			YES			YES			YES		
Period FE				YES			YES			YES			YES			YES			YES		
Month-of-crime FE				YES			YES			YES			YES			YES			YES		
District court FE				YES			YES			YES			YES			YES			YES		
Additional Cov.				NO			YES			NO			YES			NO			YES		
F-test																			367.899		

Notes: In this table, I report: Ordinary Least Squares (OLS) estimates derived from the regression of the outcome variable on a dummy variable indicating whether or not individual *i* received an unconditional prison sentence (equation 2); Reduced-Form (RF) estimates derived from the regression of the outcome variable on my instrument (equation 3); finally, I report the Instrumental Variables (IV) estimates obtained by instrumenting the dummy variable indicating whether or not individual *i* received an unconditional prison sentence by my instrument. For each category of estimates, I first report estimates obtained when only 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, and month of crime and district court fixed effects are added to the regression, and, second, those obtained when covariates are also added. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 5.C: Impact on non-road traffic crimes

	Whole sample			OLS						RF				IV				
				(1)			(2)			(3)		(4)		(5)		(6)		
	N	Mean	S.d.	Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	
Panel A: Probability of being convicted for a non-road traffic crime																		
Within 6 months	8,409	0.021	0.144	-0.002	0.004		0.002	0.004		-0.005	0.003	-0.004	0.003	-0.023	0.015	-0.022	0.014	
Within 1 year	8,409	0.060	0.238	-0.003	0.006		0.005	0.006		-0.005	0.006	-0.004	0.005	-0.026	0.030	-0.021	0.026	
Within 2 years	8,409	0.132	0.339	-0.011	0.009		-0.002	0.007		-0.004	0.007	-0.003	0.006	-0.019	0.033	-0.016	0.031	
Within 5 years	8,409	0.258	0.438	-0.027	0.011	**	-0.018	0.010	*	0.000	0.007	0.000	0.007	0.000	0.033	0.001	0.033	
Within 10 years	8,409	0.346	0.476	-0.036	0.011	***	-0.029	0.011	***	-0.002	0.007	-0.001	0.008	-0.009	0.034	-0.006	0.038	
Panel B: Number of convictions for non-road traffic crimes																		
Within 6 months	8,409	0.021	0.144	-0.002	0.004		0.002	0.004		-0.005	0.003	-0.004	0.003	-0.023	0.015	-0.022	0.014	
Within 1 year	8,409	0.060	0.238	-0.003	0.006		0.005	0.006		-0.005	0.006	-0.004	0.005	-0.026	0.030	-0.021	0.026	
Within 2 years	8,409	0.177	0.505	-0.020	0.013		-0.005	0.011		-0.005	0.011	-0.004	0.010	-0.025	0.056	-0.019	0.051	
Within 5 years	8,409	0.492	1.083	-0.079	0.028	***	-0.042	0.026	*	0.020	0.020	0.024	0.015	0.099	0.098	0.119	0.076	
Within 10 years	8,409	0.866	1.751	-0.141	0.044	***	-0.080	0.040	**	0.028	0.029	0.036	0.022	0.141	0.143	0.179	0.109	*
Trend				YES			YES			YES		YES		YES		YES		
Period FE				YES			YES			YES		YES		YES		YES		
Month-of-crime FE				YES			YES			YES		YES		YES		YES		
District court FE				YES			YES			YES		YES		YES		YES		
Additional Cov.				NO			YES			NO		YES		NO		YES		
F-test																367.899		

Notes: In this table, I report: Ordinary Least Squares (OLS) estimates derived from the regression of the outcome variable on a dummy variable indicating whether or not individual i received an unconditional prison sentence (equation 2); Reduced-Form (RF) estimates derived from the regression of the outcome variable on my instrument (equation 3); finally, I report the Instrumental Variables (IV) estimates obtained by instrumenting the dummy variable indicating whether or not individual i received an unconditional prison sentence by my instrument. For each category of estimates, I first report estimates obtained when only 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, and month of crime and district court fixed effects are added to the regression, and, second, those obtained when covariates are also added. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Impact on annual number of days worked and earnings

	Whole sample			OLS				RF				IV			
				(1)		(2)		(3)		(4)		(5)		(6)	
	N	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
Panel A: Number of days worked															
Within 1 year	8,409	129.501	159.780	-5.691	3.650	-4.530	3.403	-4.708	3.043	-7.008	2.623 ***	-23.492	14.992	-35.223	13.056 ***
Within 2 years	8,409	128.117	160.621	-0.135	4.287	1.452	3.531	-0.517	2.893	-2.795	2.770	-2.579	14.170	-14.051	13.616
Within 3 years	8,409	123.293	160.320	-2.687	4.402	0.102	4.190	-3.278	2.997	-5.507	2.737 **	-16.357	14.712	-27.679	13.514 **
Within 4 years	8,409	118.026	159.195	-3.721	3.867	0.448	3.421	-8.448	3.073 ***	-10.879	2.712 ***	-42.153	14.909 ***	-54.684	13.131 ***
Within 5 years	8,409	117.703	159.507	-5.854	4.331	-1.556	3.830	-5.532	3.153 *	-8.069	2.889 ***	-27.601	15.336 *	-40.559	14.075 ***
Within 6 years	8,409	120.342	159.473	-7.839	4.626 *	-3.544	4.092	-4.635	3.302	-7.190	2.990 **	-23.128	15.808	-36.143	14.178 **
Within 7 years	8,409	120.407	159.948	-5.405	4.070	-1.034	3.868	3.731	3.016	1.105	2.642	18.616	14.915	5.553	13.103
Within 8 years	8,409	100.011	153.577	-0.770	3.432	1.884	3.684	10.471	3.524 ***	8.271	3.164 ***	52.246	16.814 ***	41.572	15.272 ***
Within 9 years	8,408	77.946	142.091	-5.538	3.264 *	-2.680	3.259	-5.973	2.734 **	-7.406	2.603 ***	-29.806	13.411 **	-37.228	12.750 ***
Within 10 years	8,407	69.179	136.326	-6.982	3.409 **	-3.459	3.149	-5.005	2.260 **	-6.194	2.267 ***	-24.986	11.077 **	-31.150	11.192 ***
Panel B: Annual earnings															
Within 1 year	8,409	138.755	157.374	0.046	3.636	-1.524	2.269	-0.764	2.936	-3.105	2.460	-3.813	14.350	-15.607	11.922
Within 2 years	8,409	136.581	158.471	-0.008	4.001	-0.326	2.775	-0.219	3.021	-2.773	2.634	-1.092	14.820	-13.938	12.861
Within 3 years	8,409	130.743	159.238	-0.585	4.055	0.324	3.316	-2.726	2.889	-5.264	2.575 **	-13.604	14.176	-26.458	12.779 **
Within 4 years	8,409	126.535	158.446	-5.368	4.298	-2.718	3.547	-6.698	3.283 **	-9.358	2.930 ***	-33.420	16.111 **	-47.037	14.413 ***
Within 5 years	8,409	128.956	163.540	-6.664	4.252	-2.887	3.283	-3.542	3.207	-6.270	2.909 **	-17.671	15.689	-31.519	14.268 **
Within 6 years	8,409	133.934	168.489	-9.888	4.550 **	-4.922	3.899	-4.291	3.508	-7.054	3.132 **	-21.411	16.994	-35.459	15.051 **
Within 7 years	8,409	138.052	172.923	-10.411	4.731 **	-5.933	4.162	-1.562	3.599	-4.591	3.120	-7.794	17.565	-23.077	15.138
Within 8 years	8,409	135.594	175.152	-6.448	4.296	-2.301	3.598	3.392	3.368	0.385	2.887	16.922	16.655	1.933	14.294
Within 9 years	8,409	123.820	171.656	-6.053	4.196	-1.494	3.566	2.486	3.036	-0.296	2.534	12.404	14.762	-1.490	12.482
Within 10 years	8,409	110.753	165.789	-11.745	4.286 ***	-5.587	3.386 *	-3.474	3.162	-5.990	2.889 **	-17.334	15.356	-30.107	13.971 **
Trend				YES		YES		YES		YES		YES		YES	
Period FE				YES		YES		YES		YES		YES		YES	
Month-of-crime FE				YES		YES		YES		YES		YES		YES	
District court FE				YES		YES		YES		YES		YES		YES	
Additional Cov.				NO		YES		NO		YES		NO		YES	

Notes: In this table, I report: Ordinary Least Squares (OLS) estimates derived from the regression of the outcome variable on a dummy variable indicating whether or not individual i received an unconditional prison sentence (equation 2); Reduced-Form (RF) estimates derived from the regression of the outcome variable on my instrument (equation 3); finally, I report the Instrumental Variables (IV) estimates obtained by instrumenting the dummy variable indicating whether or not individual i received an unconditional prison sentence by my instrument. For each category of estimates, I first report estimates obtained when only 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, and month of crime and district court fixed effects are added to the regression, and, second, those obtained when covariates are also added. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 7.A: Difference-in-Difference, impact on drink-driving crimes

	DiD						DiD (excluding ± 3 months around the reform)						DiD (excluding ± 6 months around the reform)					
	Sample description			ITT		TOT	Sample description			ITT		TOT	Sample description			ITT		TOT
	N	Mean	S.d.	Coeff.	s.e.	Coeff.	N	Mean	S.d.	Coeff.	s.e.	Coeff.	N	Mean	S.d.	Coeff.	s.e.	Coeff.
Panel A: First-stage, probability of receiving an unconditional prison sentence																		
First-stage	8,290	0.399	0.490	0.619	0.024 ***		7,053	0.412	0.492	0.713	0.026 ***		4,620	0.408	0.491	0.717	0.034 ***	
Panel B: Probability of being convicted for a drink-driving crime																		
Within 6 months	8,290	0.012	0.108	0.006	0.003 *	0.010	7,053	0.012	0.110	0.008	0.004 *	0.011	4,620	0.014	0.116	0.012	0.007	0.016
Within 1 year	8,290	0.043	0.202	0.011	0.008	0.017	7,053	0.042	0.201	0.008	0.008	0.012	4,620	0.040	0.195	0.004	0.014	0.006
Within 2 years	8,290	0.106	0.308	0.016	0.013	0.026	7,053	0.106	0.308	0.019	0.016	0.027	4,620	0.104	0.306	0.019	0.020	0.026
Within 5 years	8,290	0.242	0.428	0.013	0.013	0.021	7,053	0.241	0.428	0.017	0.016	0.024	4,620	0.239	0.427	0.019	0.034	0.026
Within 10 years	8,290	0.350	0.477	0.009	0.012	0.014	7,053	0.350	0.477	0.009	0.017	0.013	4,620	0.342	0.475	0.024	0.031	0.034
Panel C: Number of drink-driving crimes committed																		
Within 6 months	8,290	0.012	0.108	0.006	0.003 *	0.010	7,053	0.012	0.110	0.008	0.004 *	0.011	4,620	0.014	0.116	0.012	0.007	0.016
Within 1 year	8,290	0.043	0.202	0.011	0.008	0.017	7,053	0.042	0.201	0.008	0.008	0.012	4,620	0.040	0.195	0.004	0.014	0.006
Within 2 years	8,290	0.106	0.308	0.016	0.013	0.026	7,053	0.106	0.308	0.019	0.016	0.027	4,620	0.104	0.306	0.019	0.020	0.026
Within 5 years	8,290	0.312	0.620	0.032	0.021	0.052	7,053	0.311	0.618	0.032	0.024	0.045	4,620	0.310	0.620	0.032	0.046	0.045
Within 10 years	8,290	0.529	0.870	0.034	0.024	0.055	7,053	0.530	0.871	0.019	0.030	0.026	4,620	0.523	0.875	0.013	0.063	0.018
Trend				YES						YES						YES		
Month-of-crime FE				YES						YES						YES		
District court FE				YES						YES						YES		
Additional Cov.				YES						YES						YES		

Notes: In this table, I report different Difference-in-Difference estimates capturing the impact of the reform on the outcome of interest. The first estimates use all drink-driving cases tried within 12 months of the reform, the second and third estimates use the same sample but exclude all cases tried within 3 and 6 months of the reform respectively. In each case, I obtain these estimates by regressing the outcome on a dummy variable indicating whether the case was tried after the reform, a trend, month of crime and district court fixed effects, as well as the whole conditioning set (equation 1). TOT estimates are obtained by dividing the ITT estimates by the first-stage coefficients. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 7.B: Difference-in-Difference, impact on other road-traffic crimes

	DiD						DiD (excluding ± 3 months around the reform)						DiD (excluding ± 6 months around the reform)					
	Sample description			ITT		TOT	Sample description			ITT		TOT	Sample description			ITT		TOT
	N	Mean	S.d.	Coeff.	s.e.	Coeff.	N	Mean	S.d.	Coeff.	s.e.	Coeff.	N	Mean	S.d.	Coeff.	s.e.	Coeff.
Panel A: First-stage, probability of receiving an unconditional prison sentence																		
First-stage	8,290	0.399	0.490	0.619	0.024 ***		7,053	0.412	0.492	0.713	0.026 ***		4,620	0.408	0.491	0.717	0.034 ***	
Panel B: Probability of being convicted for a drink-driving crime																		
Within 6 months	8,290	0.028	0.166	0.011	0.007	0.017	7,053	0.028	0.166	0.014	0.008 *	0.019	4,620	0.029	0.168	0.013	0.014	0.018
Within 1 year	8,290	0.078	0.268	0.011	0.011	0.017	7,053	0.079	0.269	0.014	0.013	0.019	4,620	0.081	0.274	0.002	0.018	0.003
Within 2 years	8,290	0.158	0.365	-0.001	0.013	-0.002	7,053	0.161	0.367	0.001	0.017	0.002	4,620	0.160	0.367	0.007	0.021	0.010
Within 5 years	8,290	0.292	0.455	-0.001	0.016	-0.002	7,053	0.293	0.455	0.014	0.019	0.020	4,620	0.292	0.455	-0.004	0.031	-0.006
Within 10 years	8,290	0.398	0.490	-0.011	0.017	-0.017	7,053	0.399	0.490	-0.004	0.021	-0.005	4,620	0.402	0.490	-0.025	0.031	-0.035
Panel C: Number of drink-driving crimes committed																		
Within 6 months	8,290	0.028	0.166	0.011	0.007	0.017	7,053	0.028	0.166	0.014	0.008 *	0.019	4,620	0.029	0.168	0.013	0.014	0.018
Within 1 year	8,290	0.078	0.268	0.011	0.011	0.017	7,053	0.079	0.269	0.014	0.013	0.019	4,620	0.081	0.274	0.002	0.018	0.003
Within 2 years	8,290	0.188	0.460	0.000	0.014	0.000	7,053	0.188	0.457	0.000	0.020	0.000	4,620	0.189	0.461	-0.012	0.028	-0.017
Within 5 years	8,290	0.446	0.831	0.003	0.029	0.004	7,053	0.446	0.830	0.029	0.032	0.041	4,620	0.450	0.843	-0.004	0.060	-0.006
Within 10 years	8,290	0.739	1.186	0.007	0.038	0.012	7,053	0.736	1.183	0.009	0.045	0.013	4,620	0.744	1.196	-0.018	0.079	-0.025
Trend				YES						YES						YES		
Month-of-crime FE				YES						YES						YES		
District court FE				YES						YES						YES		
Additional Cov.				YES						YES						YES		

Notes: In this table, I report different Difference-in-Difference estimates capturing the impact of the reform on the outcome of interest. The first estimates use all drink-driving cases tried within 12 months of the reform, the second and third estimates use the same sample but exclude all cases tried within 3 and 6 months of the reform respectively. In each case, I obtain these estimates by regressing the outcome on a dummy variable indicating whether the case was tried after the reform, a trend, month of crime and district court fixed effects, as well as the whole conditioning set (equation 1). TOT estimates are obtained by dividing the ITT estimates by the first-stage coefficients. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 7.C: Difference-in-Difference, impact on non- road-traffic crimes

	DiD						DiD (excluding ± 3 months around the reform)						DiD (excluding ± 6 months around the reform)					
	Sample description			ITT		TOT	Sample description			ITT		TOT	Sample description			ITT		TOT
	N	Mean	S.d.	Coeff.	s.e.	Coeff.	N	Mean	S.d.	Coeff.	s.e.	Coeff.	N	Mean	S.d.	Coeff.	s.e.	Coeff.
Panel A: First-stage, probability of receiving an unconditional prison sentence																		
First-stage	8,290	0.399	0.490	0.619	0.024 ***		7,053	0.412	0.492	0.713	0.026 ***		4,620	0.408	0.491	0.717	0.034 ***	
Panel B: Probability of being convicted for a drink-driving crime																		
Within 6 months	8,290	0.020	0.138	0.006	0.004	0.009	7,053	0.019	0.137	0.006	0.005	0.009	4,620	0.020	0.140	0.002	0.010	0.003
Within 1 year	8,290	0.059	0.236	0.011	0.008	0.018	7,053	0.059	0.235	0.004	0.010	0.006	4,620	0.061	0.239	0.002	0.017	0.003
Within 2 years	8,290	0.131	0.337	0.010	0.012	0.016	7,053	0.128	0.334	0.013	0.015	0.019	4,620	0.133	0.340	-0.004	0.022	-0.005
Within 5 years	8,290	0.259	0.438	-0.019	0.014	-0.030	7,053	0.257	0.437	-0.026	0.019	-0.037	4,620	0.261	0.439	-0.042	0.021 **	-0.059
Within 10 years	8,290	0.350	0.477	-0.021	0.016	-0.034	7,053	0.345	0.476	-0.029	0.019	-0.041	4,620	0.350	0.477	-0.088	0.029 ***	-0.123
Panel C: Number of drink-driving crimes committed																		
Within 6 months	8,290	0.020	0.138	0.006	0.004	0.009	7,053	0.019	0.137	0.006	0.005	0.009	4,620	0.020	0.140	0.002	0.010	0.003
Within 1 year	8,290	0.059	0.236	0.011	0.008	0.018	7,053	0.059	0.235	0.004	0.010	0.006	4,620	0.061	0.239	0.002	0.017	0.003
Within 2 years	8,290	0.175	0.503	0.009	0.017	0.014	7,053	0.172	0.501	0.016	0.020	0.023	4,620	0.178	0.503	-0.003	0.030	-0.004
Within 5 years	8,290	0.498	1.098	-0.012	0.040	-0.019	7,053	0.492	1.092	-0.017	0.046	-0.024	4,620	0.496	1.092	-0.028	0.065	-0.039
Within 10 years	8,290	0.878	1.778	-0.031	0.059	-0.050	7,053	0.865	1.774	-0.032	0.074	-0.045	4,620	0.879	1.780	-0.078	0.109	-0.109
Trend				YES						YES						YES		
Month-of-crime FE				YES						YES						YES		
District court FE				YES						YES						YES		
Additional Cov.				YES						YES						YES		

Notes: In this table, I report different Difference-in-Difference estimates capturing the impact of the reform on the outcome of interest. The first estimates use all drink-driving cases tried within 12 months of the reform, the second and third estimates use the same sample but exclude all cases tried within 3 and 6 months of the reform respectively. In each case, I obtain these estimates by regressing the outcome on a dummy variable indicating whether the case was tried after the reform, a trend, month of crime and district court fixed effects, as well as the whole conditioning set (equation 1). TOT estimates are obtained by dividing the ITT estimates by the first-stage coefficients. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Difference-in-Difference, impact on annual number of days worked and earnings

	DiD						DiD (excluding ± 3 months around the reform)						DiD (excluding ± 6 months around the reform)					
	Sample description			ITT		TOT	Sample description			ITT		TOT	Sample description			ITT		TOT
	N	Mean	S.d.	Coeff.	s.e.	Coeff.	N	Mean	S.d.	Coeff.	s.e.	Coeff.	N	Mean	S.d.	Coeff.	s.e.	Coeff.
Panel A: First-stage, probability of receiving an unconditional prison sentence																		
First-stage	8,290	0.399	0.490	0.619	0.024 ***		7,053	0.412	0.492	0.713	0.026 ***		4,620	0.408	0.491	0.717	0.034 ***	
Panel B: Variation in the number of days worked																		
Within 1 year	8,290	-13.030	159.886	9.072	4.797 *	14.658	7,053	-13.291	159.596	4.354	6.296	6.109	4,620	-12.306	157.925	3.223	9.681	4.497
Within 2 years	8,290	-15.360	170.362	13.654	5.280 ***	22.062	7,053	-15.488	170.854	13.810	6.768 **	19.377	4,620	-15.890	169.632	15.981	9.763	22.294
Within 3 years	8,290	-17.651	176.327	1.414	5.444	2.284	7,053	-16.483	175.942	-0.311	6.837	-0.436	4,620	-13.675	176.998	3.722	11.075	5.193
Within 4 years	8,290	-21.303	177.456	1.634	5.991	2.640	7,053	-20.755	176.660	-1.711	6.799	-2.400	4,620	-20.432	177.313	-6.205	9.683	-8.656
Within 5 years	8,290	-20.343	181.329	2.116	6.640	3.419	7,053	-19.837	180.238	-2.946	6.840	-4.133	4,620	-19.867	180.037	-1.676	10.189	-2.338
Within 6 years	8,290	-17.957	183.626	2.384	5.623	3.852	7,053	-18.092	182.710	1.274	5.966	1.788	4,620	-19.207	182.409	-5.671	8.444	-7.912
Within 7 years	8,290	-28.080	187.975	14.271	6.100 **	23.058	7,053	-30.697	187.733	12.870	6.987 *	18.059	4,620	-40.139	187.355	35.459	10.276 ***	49.468
Within 8 years	8,290	-52.724	191.176	22.300	5.998 ***	36.031	7,053	-50.797	191.521	22.800	8.595 ***	31.990	4,620	-44.643	191.646	43.338	11.611 ***	60.460
Within 9 years	8,290	-69.042	186.968	10.832	5.274 **	17.502	7,053	-68.297	186.641	7.400	5.946	10.383	4,620	-65.857	185.342	12.292	9.374	17.148
Within 10 years	8,290	-74.317	186.640	3.094	5.996	4.999	7,053	-74.171	186.816	0.881	7.122	1.237	4,620	-73.800	184.887	-4.948	9.778	-6.903
Panel C: Variation in annual earnings																		
Within 1 year	8,290	-17.384	127.330	6.633	3.920 *	10.717	7,053	-18.351	127.454	5.993	5.050	8.409	4,620	-18.907	125.696	5.995	7.042	8.364
Within 2 years	8,290	-21.214	141.696	11.390	4.183 ***	18.404	7,053	-21.768	142.774	11.755	5.606 **	16.494	4,620	-21.997	143.004	16.311	7.395 **	22.755
Within 3 years	8,290	-25.588	151.132	4.476	4.913	7.233	7,053	-25.431	151.077	3.709	5.980	5.204	4,620	-23.379	151.267	5.995	7.025	8.363
Within 4 years	8,290	-27.193	156.154	0.093	5.174	0.151	7,053	-27.068	156.161	-2.458	7.461	-3.449	4,620	-26.661	156.639	-2.658	9.443	-3.708
Within 5 years	8,290	-21.304	165.564	-4.371	5.194	-7.062	7,053	-21.316	165.258	-4.257	6.268	-5.973	4,620	-20.598	164.885	-10.171	7.668	-14.190
Within 6 years	8,290	-15.469	172.904	-6.208	5.616	-10.030	7,053	-16.645	172.913	-6.882	7.224	-9.656	4,620	-18.069	172.433	-8.197	7.847	-11.436
Within 7 years	8,290	-13.322	178.422	-0.484	6.558	-0.781	7,053	-14.643	178.235	-1.235	9.445	-1.733	4,620	-17.473	176.230	3.423	9.959	4.776
Within 8 years	8,290	-22.415	182.709	9.800	5.016 *	15.834	7,053	-24.452	182.048	11.133	7.384	15.621	4,620	-28.208	180.651	19.437	9.053 **	27.116
Within 9 years	8,290	-37.509	183.548	13.136	4.948 ***	21.224	7,053	-37.573	183.237	14.901	7.291 **	20.908	4,620	-35.817	182.998	29.100	8.914 ***	40.597
Within 10 years	8,290	-45.769	183.701	2.473	4.810	3.995	7,053	-46.182	183.564	3.980	7.195	5.585	4,620	-44.645	182.891	10.400	8.430	14.509
Trend				YES						YES						YES		
Month-of-crime FE				YES						YES						YES		
District court FE				YES						YES						YES		
Additional Cov.				YES						YES						YES		

Notes: In this table, I report different Difference-in-Difference estimates capturing the impact of the reform on the outcome of interest. The first estimates use all drink-driving cases tried within 12 months of the reform, the second and third estimates use the same sample but exclude all cases tried within 3 and 6 months of the reform respectively. In each case, I obtain these estimates by regressing the outcome on a dummy variable indicating whether the case was tried after the reform, a trend, month of crime and district court fixed effects, as well as the whole conditioning set (equation 1). TOT estimates are obtained by dividing the ITT estimates by the first-stage coefficients. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Appendix

Table A.1: Legislation on drink-driving crimes following the 1998 reform

BAC (mg/l)		1st offense		2nd offense		3rd offense		4th offense		5th offense	6th offense	7th offense	8th offense
		DUI	Aggr. DUI	DUI	Aggr. DUI	DUI	Aggr. DUI	DUI	Aggr. DUI				
(0.50;0.80]	Fine	4.000 kr.	5.000 kr.	5.000 kr.	8.000 kr.	-	-	-	-				
	Prison	-	-	-	-	10 days	14 days	14 days	20 days				
	License susp.	-	Cond.	-	Cond.	-	Cond.	-	Cond.				
(0.80;1.20]	Fine	1 month's net pay	1 month's net pay	-	-	-	-	-	-	-			
	Prison	-	-	10 days	14 days	min. 30 days		min. 40 days		min. 60 days	min. 3 months	min. 4 months	min. 6 months
	License susp.	Conditional	1 year	3 years	3 years	10 years*		Life suspension		Life suspension			
(1.20;1.50]	Fine	1 month's net pay	1 month's net pay	-	-	-	-	-	-	-			
	Prison	-	-	14 days	20 days	min. 30 days		min. 40 days		min. 60 days	min. 3 months	min. 4 months	min. 6 months
	License susp.	1 year	2 years	3 years	3 years	10 years*		Life suspension		Life suspension			
(1.50;2.00]	Fine	1 month's net pay	-	-	-	-	-	-	-	-			
	Prison	-	14 days	14 days	30 days	min. 30 days		min. 40 days		min. 60 days	min. 3 months	min. 4 months	min. 6 months
	License susp.	2 years	2.5 years	5 years	5 years	10 years*		Life suspension		Life suspension			
(2.00;2.50]	Fine	-	-	-	-	-	-	-	-	-			
	Prison	14 days	20 days	20 days	30 days	min. 30 days		min. 40 days		min. 60 days	min. 3 months	min. 4 months	min. 6 months
	License susp.	2.5 years	3 years	5 years	5 years	10 years*		Life suspension		Life suspension			
>2.50	Fine	-	-	-	-	-	-	-	-	-			
	Prison	20 days	30 days	30 days	40 days	min. 30 days		min. 40 days		min. 60 days	min. 3 months	min. 4 months	min. 6 months
	License susp.	2.5 years	3 years	5 years	5 years	10 years*		Life suspension		Life suspension			

* Aggravating circumstances can lead to life suspension

Table A.2: Additional information on the variables used

Conditioning set	**Description** N.B.: *Names in italics refer to variables made available by Statistics Denmark. More information can be found on each of these variables on their website: https://www.dst.dk*
Offender's number of convictions in the 2 years preceding the crime	Computed from the date of crime. Top-coded at the 99th percentile. Variable source: we calculated this information ourselves using the exhaustiveness of the registers. Information on the date of the crime was retrieved from the *SIG_GER1DTO* variable.
Offender's number of convictions prior to the 2-year period preceding the crime	Computed from the date of crime. Top-coded at the 99th percentile. Variable source: we calculated this information ourselves using the exhaustiveness of the registers. Information on the date of the crime was retrieved from the *SIG_GER1DTO* variable.
Whether or not the offender was a juvenile at the time of the crime	Computed using information on the date of birth of the offender, as well as on the date of the crime. Variable source: information on the offenders' date of birth was retrieved from the *FOED_DAG* variable and information on the date of the crime from the *SIG_GER1DTO* variable.
Nature of the trial (mixed court trial or single judge trial with confession)	Variable source: information on the nature of the trial was retrieved from the *AFG_AFGMAADKO* variable.
Offender's gender	Variable source: information on the gender of the offender was retrieved from the *AFG_KOEN* variable.
Offender's age at the time of the trial	Variable source: information on the gender of the offender was retrieved from the *AFG_AFGALD* variable.
Offender's immigration status	Dummy variables indicative of the following four groups of individuals: - Immigrants - Descendants of immigrants - Unknown status - Rest of the population Variable source: information on the immigration status of the offender was retrieved from the *IE_TYPE* variable.

Offender’s marital status	Dummy variables indicative of the following five groups of individuals: - Single - In a partnership - Separated - Widow - Unknown status Measured at the end of the year preceding the offender’s crime. Variable source: information on the martial status of the offender was retrieved from the *CIVST* variable.
Offender’s highest educational achievement	Dummy variables indicative of the following four groups of individuals: - Primary education - Secondary education - Higher education - Unknown highest educational achievements Measured at the end of the year preceding the offender’s crime. Variable source: information on the education status of the offender was retrieved from the *HFFSP2* variable.
Offender’s months of education	Indicative of the minimum number of months of education required for anyone to obtain the highest educational achievement obtained by the offender. Measured at the end of the year preceding the offender’s crime. Variable source: information on the months of education of the offender was retrieved from the *PRIA* variable.
Offender’s earnings	Earnings before tax and any other social contributions. Top-coded each year at the 99th percentile. Missing values were given the value 0. Measured at the end of the year preceding the offender’s crime. Variable source: information on the offender’s earnings was retrieved from the *LOENMV* variable.
Offender’s employment status	Dummy variables indicative of the following six groups of individuals: - Full-time employment (>29 hours per week) - Part-time employment (15-29 hours per week) - Limited employment (<15 hours per week)

	- Employment with partial unemployment throughout the year - Unknown employment status - No employment Measured at the end of the year preceding the offender’s crime. Variable source: information on the employment status of the offender was retrieved from the *JOBKAT* variable.

Outcome variables	**Description** *<u>N.B.:</u> Names in italics refer to variables made available by Statistics Denmark. More information can be found on each of these variables on their website: https://www.dst.dk*
Offender's number of convictions following their trial	Computed from the date of trial. We exclude from the computation of this variable convictions related to crimes committed prior to the trial of interest or convictions registered under the same criminal case identifier. Top-coded at the 99^{th} percentile. Variable source: we calculated this information ourselves using the exhaustiveness of the registers. Information on the date of each crime was retrieved from the *SIG_GER1DTO* variable.
Whether or not an offender was convicted following their trial	Computed from the above variable.
Offender's earnings	Salary before tax and any other social contributions. Top-coded each year at the 99^{th} percentile. Missing values were given the value 0. Measured at the end of the year following the offender's trial. Variable source: information on the offender's earnings was retrieved from the *LOENMV* variable.

Table A.3: Reform and unconditional prison sentence by subgroups

Subgroups		Number of observations	Mean	Instrument
DD priors	*No prior conviction*	5,516	0.493	0.183***
				(0.011)
	Prior conviction(s)	2,893	0.728	0.229***
				(0.028)
Other priors	*No prior conviction*	4,149	0.562	0.162***
				(0.020)
	Prior conviction(s)	4,260	0.585	0.233***
				(0.026)
Age	*Below 25*	1,000	0.455	0.150***
				(0.011)
	25 and above	7,409	0.590	0.206***
				(0.000)
Gender	*Female*	707	0.636	0.233***
				(0.010)
	Male	7,702	0.568	0.197***
				(0.064)
Employment status	*Inactive*	2,058	0.573	0.174***
				(0.014)
	Unemployed	912	0.607	0.220***
				(0.013)
	Employed	5,368	0.569	0.207***
				(0.013)
Education	*Higher education*	3,678	0.591	0.229***
				(0.012)
	Lower education	4,434	0.561	0.177***
				(0.015)

Notes: In this table, I estimate the first stage equation for various subgroups of the sample. More specifically, a dummy variable indicative of whether or not a defendant received an unconditional prison sentence is regressed on my instrument, 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, dummy variables indicative of the exact nature of the charge, information on the criminal case, and defendant characteristics. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Reform and other trial outcomes

									Sample				
	(1)			(2)				(3)			(4)		
	DD crimes tried 0-6 months *before* the reform			DD crimes tried 0-6 months *after* the reform				DD crimes committed 0-24 months *before* the reform			Correlation between trial outcomes & instrument		
	#Obs	Mean	S.d.	#Obs	Diff.	S.e.		#Obs	Mean	S.d.	Diff.	S.e.	
Prison	1,754	0.668	0.471	1,943	0.016	0.009		8,409	0.733	0.443	0.001	0.009	
Prison, uncond.	1,754	0.661	0.474	1,943	-0.508	0.010	***	8,409	0.574	0.495	0.200	0.011	***
Prison, uncond. (length)	1,754	16.558	23.675	1,943	-10.439	0.451	***	8,409	15.042	25.390	3.764	0.558	***
Imprisoned	1,754	0.305	0.461	1,943	-0.168	0.007	***	8,409	0.346	0.476	0.055	0.009	***
Prison, cond.	1,754	0.008	0.089	1,943	0.506	0.009	***	8,409	0.155	0.362	-0.193	0.009	***
Prison, cond. (length)	1,754	0.392	5.798	1,943	10.865	0.278	***	8,409	3.575	10.317	-4.066	0.260	***
Community work	1,754	0.000	0.000	1,943	0.290	0.006	***	8,409	0.085	0.278	-0.108	0.005	***
Fine	1,754	0.327	0.469	1,943	0.511	0.008	***	8,409	0.420	0.494	-0.200	0.011	***
Fine amount	1,754	1988.427	3464.473	1,943	2692.778	60.081	***	8,409	2429.665	3541.650	-1120.090	66.350	***
Driv. lic. suspended	1,754	0.290	0.454	1,943	0.000	0.008		8,409	0.309	0.462	-0.007	0.010	
Driv. lic. Suspended (duration)	1,754	1.741	2.724	1,943	0.001	0.045		8,409	1.854	2.773	-0.041	0.058	
Appeal	1,754	0.010	0.098	1,943	0.028	0.003	***	8,409	0.020	0.140	-0.001	0.003	

Notes: In columns (1) and (2), I describe the outcomes of drink-driving cases tried in the 6 month period before the entering into force of the reform and compare these with the outcomes of drink-driving cases tried in the following 6 month period. In order to do the latter, I regress trial outcomes separetely on a constant, a dummy variable taking the value 1 when a case was tried after the reform and district court fixed effects. The coefficients associated with the dummy variable are reported together with their standard errors. In columns (3) and (4), I describe the outcomes of the trial of drink-driving cases included in my sample and investigate the extent to which these outcomes are correlated with my instrument. In order to do so, I regress separetely trial outcomes separately on a constant, the instrument, a trend, 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, month of crime fixed effects, and district court fixed effects. The coefficients associated with the instrument are reported together with their standard errors. Results are presented for all individuals included in our sample (Panel A), as well as more specifically for individuals who received an unconditional prison sentence (Panel B). Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.