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RESEARCH REPORT

STUDY PAPER 192

AUGUST 2023

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# Court Delays and Criminal Recidivism: Results from Danish Administrative Data and a Policy Reform

Lars Højsgaard Andersen

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Published by:

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# **Court Delays and Criminal Recidivism: Results from Danish Administrative Data and a Policy Reform**

Lars Højsgaard Andersen

## **Abstract**

Delays at court are an everlasting and potentially consequential reality of criminal justice systems, although most would agree that the timely adjudication of cases is needed from both administrative, judicial, and individual perspectives. This paper uses administrative data and a policy reform in Denmark in 2007 to measure the unconfounded association between court delays – or, more specifically, time to adjudication – and criminal recidivism within 5 years. Results show that although court delays do not push more people into recidivism, the delays matter for how many crimes recidivists end up committing. Also, criminality tends to be muted during the period from charge to adjudication (even in a context with low use of pretrial detention and no bail system), whereby court delays also matter for the timing of new crimes. Results thus imply that we should be wary of court delays as they may affect individual behavior for the worse.

## **Introduction**

Court delays and case backlogs represent fundamental problems. From an administrative point of view, case backlogs must be dealt with at some point, which could place substantial pressure on courts and legal personnel. From a justice point of view, case backlogs (and delays) could decrease the quality of judicial processes, such as if witnesses forget details of the case or if prosecutors accept plea bargains which they normally would not, to get cases off their desks. Last, from an individual point of view, court delays could lead to behavioral consequences, as awaiting trial has been shown to represent a particularly stressful period (e.g., Bacak, Andersen, and Schnittker, 2019).

With this paper, I focus on the latter (individual consequences) and zoom in on one especially important consequence of halting judicial processes on individual behavior: the risk of new crimes. If system changes that have administrative and justice related consequences matter for criminal behavior, this should be cause for concern not only for the individuals awaiting trial but also for public policy makers and administrators working in the criminal justice system. And there is indeed reason to suspect that court delays could matter for crime risks. Deterrence theory – a widely acknowledged and popular theoretical approach to understanding both the efficiency of criminal justice systems and individual criminal decision making (Paternoster 2010) – states that in addition to detection risks and the severity of punishment if caught, the celerity or swiftness of punishment is important for criminal decision making (e.g., Nagin and Pogarsky, 2001). Celerity essentially implies time discounting, whereby individuals downweigh the importance of punishment the further out in time it may hit them. Court delays thus obviously push this margin of the etiology of crime, leading to the expectation that court delays increase recidivism. Empirical support for the importance of celerity of punishment is somewhat unclear, however, just as qualitative research has shown that awaiting punishment can strain those charged with a crime almost as

much as the eventual punishment itself (Laursen, Mjåland, and Crewe 2020), and some may even prefer immediate sanctions over delayed ones (Jaynes and Wilson 2022). I return to this discussion shortly, but now emphasize the empirical challenge of measuring the impact of celerity of punishment for individuals' crime risks, which only adds to the challenge of understanding the role of time to adjudication. Challenge arises because time to adjudication and the risk of new criminal offenses are likely to be endogenously related. Cases that take longer to adjudicate may express other features of those cases and the involved persons, such as case complexity and the accused's willingness to cooperate with the police and prosecutors. There is thus reason to suspect that such other features (which are unobserved in the data) might also matter for the risk of new offenses – without being directly tied to time to adjudication. The direct comparison of rates of new criminal offenses among persons with short and long time to adjudication is thus likely to be biased, even when controlled for observed characteristics.<sup>1</sup>

In line with other work (e.g., Andersen 2020; Bacak, Andersen, and Schnittker 2019) that focuses on the causal impact of celerity of punishment, I apply a research design-driven setup to overcome the endogeneity challenge and provide estimates that are much less likely to be confounded. Specifically, I rely on administrative data from Denmark from before, during, and after a structural reform of the Danish courts in 2007 which in addition to changing the entire court system led to significant case backlogs. As outcome I merge administrative records of new criminal offenses that led to conviction for up to 60 months (5 years) after the original charge. The detailed nature of the data allows me to show results both during the

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<sup>1</sup> Loeffler and Nagin (2022) review well-identified (random judge assignment and sentencing thresholds) studies of the effects of imprisonment on recidivism and concludes that the endogeneity challenge may not be that consequential after all (for the imprisonment-recidivism link, at least) as long as one can control for relevant control variables. The problem is, of course, to know which variables are most important to control for and when enough control variables are added. In my setup, there is no administrative measure of drug use history or gang affiliation, for example, which could be important for the celerity-recidivism link. Thus, given the nature of the data for this study, a design-driven approach to obtaining unconfounded estimates must be considered superior to standard statistical control.

period from charge to adjudication (the period affected by the court delays) and during the period following adjudication. The study was pre-registered under the Open Science Framework ([https://osf.io/g9tzu/?view\\_only=df270db4466648c693613151f57da63f](https://osf.io/g9tzu/?view_only=df270db4466648c693613151f57da63f), date stamped on 05/07/2019; link is to a de-identified version for blind review).

In brief, results imply that after five years, rates of criminal reconviction are identical despite the reform and the resulting court delays. But importantly, the timing of new offenses varies. Criminal behavior among those with longer time to adjudication seems to be suppressed during the period from charge to conviction on the index case (when they are arguably under more intense criminal justice surveillance), but then accelerates post-conviction relative to those with shorter time to adjudication. But this levelling out is only present when focusing on the percentages that are reconvicted. Analyzing the average number of convictions per person, not just the percentage reconvicted, the results show a similar decline in the short run – but the post-conviction acceleration is so strong that after five years, those with longer time to adjudication have substantially intensified their criminal careers. Put directly, although court delays do not push more persons into reconvictions (no extensive margin response), those who reoffend end up reoffending more as time to adjudication increases (intensive margin response).

### **Why Court Delays Might Matter**

Court delays may affect several margins, such as ones related to cases and ones related to individual behavior. At the case level, court delays could have mechanical effects, such as (from a justice point of view) worse case outcomes because details of a case may be forgotten with time and witnesses may forget details. Conviction risks thus may simply decrease mechanically as the result of court delays. The same can be said for plea bargains in which

prosecutors may settle for less-than-ideal pleas to get cases off their desk, again implying that court delays may affect case outcomes.<sup>2</sup>

Of main interest to this paper stand the individual behavioral outcomes of court delays, as exemplified by the risk of new crimes. As mentioned in the Introduction, there is reason to suspect that court delays could affect individual (criminal) behavior, as deterrence decreases from lower celerity of the punishment when it occurs further away in time. But the main empirical knowledge we have regarding swiftness of punishment is that it seems to be the least important of the three deterrent properties of punishment (e.g., Nagin and Pogarsky 2001). Jaynes and Wilson (2022) even offer an alternative – and opposing – interpretation of time to adjudication. They used vignettes of sentencing scenarios on a sample of young adults to show that people prefer immediate sanctioning because delayed sanctions can invoke costly negative emotions. In this way, increasing time to adjudication could be seen as *de facto* increasing the total punishment (from sentence to time to adjudication + sentence). This interpretation resonates with qualitative research, which shows that awaiting the serving of a sentence functions like an additional punishment period, during which one cannot meaningfully engage in social or rehabilitative activities but is not yet fully a part of the corrections system either (Laursen, Mjåland, and Crewe 2020). Results in Andersen (2020) – documenting a causal increase in criminal recidivism from swifter incarceration among very young men – corroborate this interpretation, although that study seems to emphasize the timing effects of swifter punishment (to which I return shortly) over the relationship between time to adjudication and total sanction.

Another way in which court delays could affect individual (criminal) behavior is through stress, as the period from when someone faces criminal charges and until adjudication is

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<sup>2</sup> I later show that neither of these worries were true to any substantial degree during my data period.

likely to be characterized by several uncertainties; what sociological stress theory calls primary as well as secondary stressors (Pearlin, Menaghan, Lieberman, and Mullan, 1981; Thoits, 2010). In the context of this study, primary stressors relate to the case outcome, and cover issues surrounding guilt and the inherent uncertainty that surrounds trial. The endpoint of the period under study (time to adjudication) is without doubt very consequential for how a person's life may unfold, and the waiting may be unbearable. Secondary stressors pertain to the array of derived effects that could follow whichever decision awaits at the trial. Concern may cover safety, for example, as prisons – one likely outcome of a criminal case – are unsafe environments (or, at least the threat of victimization is always there). Other concerns may cover the future of one's relationship with family members or significant others (separation through incarceration has been shown to affect such ties, see e.g., Fallesen and Andersen 2017, Massoglia, Remster, and King, 2011), and the potential impact of having a criminal record on employability (Pager, 2003). Supporting the idea that stress may be an inherent feature of awaiting contact with the criminal justice system, Laursen, Mjåland, and Crewe (2020) use qualitative data from Norway – a context not too dissimilar from Denmark – to show that the period from conviction to incarceration is characterized by great uncertainty and powerlessness (their informants label the period a sentence before the sentence).

Last, court delays could also impact individual behavior through their effect on the timing of when a person is processed by the criminal justice system and perhaps gets sent to a correctional facility. The timing perspective grows out of the observation that court delays impact the age a person has at adjudication. For small delays, this impact is likely meager, yet for substantial court delays, age at adjudication could be more significantly affected. Considering the intimate relationship between age and crime, the timing issue is likely to not simply be an academic concern. Also relying on data from Denmark and a policy reform in

1994, Andersen (2020) confirms this, as rates of criminal recidivism increase substantially for youth who experience faster case processing (shorter period from conviction to incarceration, by which they were substantially younger when sanctioned).

As is clear from this summary of existing research, most prior studies focus on incarceration and when it occurs.<sup>3</sup> Being among the harshest types of punishment that criminal justice systems in the developed world enforces, this focus on incarceration makes sense. But incarceration only constitutes a fraction of the total punishments that are enforced as well as of types of criminal justice contact, leaving the results of prior studies to apply only to a subset of people and cases. In the current study, focus is on an increase in time to adjudication for *all* case types – ranging from cases that are acquitted to cases that lead to imprisonment for years – a focus that is more at par with whom are likely to be affected by court delays (i.e., everyone with some contact with the criminal justice system). This contribution is no small feature, as the most directly comparable studies (Andersen 2020, Bacak, Andersen, and Schnittker 2019) – because they also use data from Denmark and because they also use policy-induced variation in time to adjudication to produce estimates that are unconfounded by selection issues – pertain to a very limited subset of people who come into contact with the criminal justice system. Their studies focus only on young men (younger than age 20 at the time of the charge) who are about to experience imprisonment for the first time, in the mid-1990s. Their conclusions are thus unlikely to generalize to all people

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<sup>3</sup> Swiftiness of punishment also plays a significant role in probation practices promoted by project HOPE (Honest Opportunity Probation with Enforcement). HOPE focuses on providing swift, certain, and fair responses to violations of probation conditions for drug offenders in the US. Following promising initial results that echo the expectations from Deterrence Theory, the project was soon expanded to other areas than Hawaii where it started and to other (non-drugs) types of offenders (see Cullen, Pratt, Turanovic, and Butler, 2018 for a discussion of the expansion of HOPE). Based on the initial results from evaluations of HOPE (e.g., Hawken and Kleiman, 2009) we should thus expect time to adjudication to matter for the risk of new crimes, in the sense that longer time to adjudication increases the crime response. A recent multisite evaluation of HOPE later found, however, the discouraging result that HOPE was unlikely to have impacted rates of criminal recidivism to any meaningful degree (Lattimore, Dawes, MacKenzie, and Zajac, 2018).

(of all ages) who experience other types of contact with the criminal justice system (e.g., fines), the focus of this study.

### **Context, Data, and Methods**

Denmark falls under what Pratt (2008) labelled “Scandinavian Exceptionalism”, which refers to low incarceration rates and generally very humane conditions of confinement. The Danish imprisonment rate is just 63 per 100,000 (Walmsley, 2018), which reflects short sentences (60% of prison sentences in Denmark in 2018 were 4 months or shorter; about 10% exceeded 2 years; the shortest prison sentences in Denmark are just 7 days) as well as a broad adoption of noncustodial alternatives to imprisonment.

A few features of the Danish criminal justice system require special attention. Pretrial detention can only be used in cases where the accused potentially faces (if found guilty) a sentence of more than 1.5 years. And even then, pretrial detention can only be used if features of the case specifically require it (risk of collusion or flight risk) and the pretrial detention must be approved by a judge (and re-approved every 4 weeks). There is no money bail system in Denmark. These features imply that most people who are charged with a crime in Denmark avoid detention (but they may have been arrested for up to 24 hours) until they are summoned to court or to a plea bargain. And even then, after the adjudication and even for people sentenced to imprisonment, they are often free to move around until they receive a letter notifying them that it is time to serve the sentence. Just as time from charge to adjudication may constitute a significant period in Denmark, so too may the time from adjudication to when the sentence is served (for the fraction that serves a prison sentence). In my data, although time from charge to adjudication changes, time from adjudication to imprisonment does not (to which I return).

Also important is the fact that by Danish law, it is possible to get a sentence reduction if the case process has been prolonged in an unreasonable way relative to the specifics of the case and not per the fault of the charged person. Sørensen (2023) describes and discusses the law including recent amendments to it and concludes that during the period under focus in this study, the rule may (but not always) have been relevant in cases that exceed 2½ years of processing time but are otherwise not complex. As will be seen, this 2½ year criterion applies to very few cases in my data (even in the upper quartile of time to adjudication the processes on average dwarfs two years). Sørensen (2023) also observes that courts effectively interpret the length of a cases' processing time relative to the general length at the point, implying that sentence reductions because of long case processing times are unlikely to challenge my empirical setup.

### ***Policy Reform***

On January 1<sup>st</sup>, 2007, the Danish National Police and the Danish Courts went from 54 to 12 police jurisdictions and from 82 to 24 courts (The Reform of the National Police and Courts, 2006). This national restructuring aimed to increase the education level of police and of public prosecutors and to increase efficiency by streamlining registration practices and centralizing key administrative tasks. An official evaluation in 2009 by The Danish Public Accounts Committee (2009) found that although police response time to acute emergency calls had decreased as intended following the reform (decrease was 1:18 minutes from 2006 to 2008; the share of cases that had long response time also decreased), only few other aims of the reform had been met at that point. Rather, police spending had increased and the process of redistributing approximately 12,000 employees across the new jurisdictions had proven a challenge. Importantly, the restructuring led to significant and immediate court delays, which

is of little surprise considering the total overhaul of the police and courts which the reform imposed.

The policy reform may be thought of as a natural experiment because it separates two policy regimes, one prior to the reform and one after it. Ideally (from a research perspective), time to adjudication changed abruptly with the reform (because of court delays promoted by the restructuring), implying that persons charged just before and after the reform could be compared to obtain unbiased estimates of the impact of time to adjudication on outcomes. But in practice, the restructuring of the police and courts did not happen overnight, although January 1<sup>st</sup>, 2007, was the first official day under the new policy regime. Instead, the period from when the policy reform was proposed and approved and until January 1<sup>st</sup>, 2007 – and even later with the last police jurisdiction officially being governed under the new regime as late as February 1<sup>st</sup>, 2008 – was characterized by a gradual transition to the new policy regime. Court delays similarly grew gradually, not as a discontinuous jump on January 1<sup>st</sup>, 2007. This transition is clearly visible from Figure 1, which shows the average time to adjudication (in months) by quarter of the criminal charge during 2004-2012.

[Insert Figure 1 about here]

The vertical lines in Figure 1 mark when the policy reform was proposed (March 1<sup>st</sup>, 2006), approved by Parliament (June 8<sup>th</sup>, 2006), and formally enacted (January 1<sup>st</sup>, 2007, as mentioned). The short period from proposal to approval reflects that members of Parliament (MPs) were generally in agreement about the reform already at the point of the proposal, and the reform was approved by the MPs from all major parties (the bill had only 4 votes against it). Time to adjudication clearly responds to these dates; court delays start right when the policy reform was proposed and approved. After having increased from around 6 months prior to reform and up to 10 months in early 2008, the average time to adjudication does not

reach back to its pre-reform level until 2011/12. The colored lines in Figure 1 break down the overall time to adjudication by case types, and here the response to the reform is also clear. Court delays thus seems to have been the reality across all case types in Denmark.

From a research perspective, the reform-induced increase in time to adjudication may still represents a natural experiment that can be used for causal inference, yet the somewhat “fuzzy” trend in time to adjudication across the timing of the reform leads to challenges related to what the trend *would have been* in the absence of the reform, and challenges related to other changes (both general and related to the criminal justice system) that may have occurred over the same period. In the Methods section I return to how I handle these challenges empirically and specify how I exploit the trend in Figure 1 to try and measure the effect of time to adjudication on new convictions – in the Discussion section, I return to the challenge that other changes may have happened over the study period that could confound the reform-induced relationship between time to adjudication and criminal recidivism.

### ***Data***

Results presented in this paper build on administrative data. In Denmark, all residents have a unique identification number which identifies them in various types of contact with governmental and private agencies. The Danish National Police, for example, records all charges and adjudications and the Danish Prison and Probation Service records all incarcerations. They record this information in their respective administrative systems using the unique personal identification numbers and are then required to submit the records to the national statistical agency, Statistics Denmark. Statistics Denmark documents the data, including discontinuities in variable definitions across time and the like (for Statistics Denmark, including how to apply for data access, see <https://www.dst.dk/en>; for a list of

Danish registers (in Danish), see

<http://www.dst.dk/extranet/forskningvariabellister/Oversigt%20over%20registre.html>), and

makes the data available for research, given that the research project and the hosting institution are approved for access and complies with a range of strict data security measures (formalized in the Law of Statistics Denmark, see (in Danish)

<https://www.dst.dk/en/OmDS/lovgivning>). For details on Danish register data in the context of criminal justice studies, see Andersen (2018).

The natural experiment that I exploit in the analyses behind this study, as mentioned, unfolded from 2006 with increasing court delays. To include a data period before the natural experiment unfolded, I focus on data from 2005-2008 and the final data cover a total of 160,710 criminal charges handed down to 98,449 persons (some persons are charged more than once within the data window and separate persons charged on the same case are counted individually). I obtain the charges from the register of charges, which is recorded by the Danish National Police and public prosecutors and contain the full universe of formal charges in the country. I keep only criminal charges, which are the most serious ones, implying that the data do not include minor offenses like violations of the Traffic Act.<sup>4</sup>

### ***Variables.***

*Key independent variable: Time to adjudication.* I obtain the variable of specific interest by merging case outcomes to charges (using the combination of case and individual identification numbers to merge the charges register with the convictions register). Both charges and adjudications (verdicts (guilty and not guilty), plea bargains, case dismissals) are

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<sup>4</sup> I deleted 1,072 cases (0.4 percent of the data) to clean the administrative records from the Danish National Police (307 had missing information on the merged covariates and 765 had zero or negative time to adjudication).

recorded by date, and time to adjudication is simply the difference between them. To better be able to interpret the impact of time to adjudication, I focus on time to adjudication in months rather than days (but this is simply a matter of scaling the parameter estimates in the empirical model and is without consequences).

*Dependent variables: New criminal convictions.* The dependent variables measure new criminal convictions obtained from the convictions register and recorded by the Danish National Police. I include two measures of new criminal convictions, namely a binary and a count measure. Again, to gauge serious behavior, focus is on criminal offenses, and I exclude minor offenses from the dependent variables. The dependent variables then measure whether each person in the data had committed a criminal offense / the number of criminal offenses for which he or she was later convicted between the month following the “original” charge (on the case that got delayed) and up to 60 months after. In practice, 60 dummy variables and 60 count variables represent each of the 60 months after the original charge. The 60 dummies indicate whether (0 = “No” / 1 = “Yes”) the person had committed a new criminal offence that led to conviction by that follow up point. And the 60 count variables measure the total number of new criminal offenses that the person was later convicted of up to that follow up point (i.e., the outcomes are cumulated over the 60 months of follow up). Note that to keep court processes from skewing the outcome variables, I refer an offense to the month it was committed, although the conviction is at a later point (and may occur after the 60 months follow up).

*Control variables.* Control variables include a range of background characteristics of the charge and the charged person. Related to the charge that selects persons into my data, I merge the charges register and the convictions register to obtain crime type (violent crime, property crime, and a residual category of other crimes), sentence type (acquittal, fine, various types of probation, or imprisonment), sentence length (for charges that lead to prison

sentences), an indicator of whether the person was detained pretrial on the case, and an ID on which post-reform police jurisdiction handled the charge (dummy variables; pre reform jurisdictions were recoded to match post reform ones). From other registers, I add the following background characteristics, all measured prior to the criminal case that selects them into the data. From the population register I add sex (1=female, 0=male), age, family status (measured using two dummy variables: married and has children), and whether the person or his/her parents immigrated from a non-western country. From the education register, which is recorded by the Ministry of Education and reflects formal educational credentials, I add years of education (missing educational length is set to 0 years and a separate dummy variable indicates missing educational information) as well as dummies for the most common types of education (elementary schooling, upper secondary education, and vocational education). From the income register, I add prior earnings from work measured in DKK and adjusted for fluctuations in the wage and price structures in Denmark according to the official adjustment rates from the Danish Ministry of Treasury. I then use dummy variables to indicate whether the person earned under the lowest quartile or above the highest quartile of the distribution. The income register is recorded by the Danish Customs and Tax Administration and because all employers are required to report all salaries, fringes, bonuses, severance pays, board fees, stock options, salaries during leave, and even non-taxable salaries directly to the tax authorities (full third-party reporting), the earnings measure includes all income from legal labor work (but, obviously, I have no information about incomes that are off the books). For labor market attachment, I add three dummy variables obtained from the Ministry of Employment: employed, unemployed, and outside the labor force (definitions follow the International Labour Organization (ILO)), which are recorded ultimo November the year before the charge in question. To gauge mental health, I include two dummy variables which indicate whether the person has had any contact with psychiatric hospitals prior to the charge

(ever) and whether the person has had recent contact with psychiatric hospitals (defined as contact within the last year prior to the charge). Last, I add control variables for criminal history obtained from the different criminal justice registers that I already described: age at first conviction; number of previous convictions (dummy coded: 0, 1, 2, 3, 4, or 5+); and number of previous incarcerations (dummy coded: 0, 1, 2, or 3+; incarceration includes both pretrial detention and post-conviction incarceration but not brief arrests lasting less than 24 hours).

### ***Methods***

Time to adjudication and the risk of new criminal offenses are likely to be endogenously related, as already discussed. If, for example, cases that take longer to adjudicate express other features of those cases, such as their complexity, there is reason to suspect that such other features (which are unobserved in the data) might also matter for the risk of new offenses – without being directly tied to time to adjudication. The direct comparison of rates of new criminal offenses among persons with short and long time to adjudication is thus likely to be biased, even when controlled for observed characteristics.

To overcome this challenge, I use the 2007 policy reform to obtain variation in time to adjudication that is unrelated to observed as well as unobserved characteristics of the cases and persons. I use a two-stage model fitted using the two-stage least squares estimator (for a discussion of instrumental variables in criminology, see Bushway and Apel, 2013). In the first stage, I estimate for individual  $i$  on case  $c$  the following model of the impact of calendar time (quarter-by-year,  $yq$ ) on time to adjudication ( $time$ ) while controlling for the control variables ( $X$ ) that I just described and post-reform police jurisdiction fixed effects ( $p$ ). The error term ( $\tau$ ) is assumed to be independent and identically distributed (iid):

$$time_{ic} = \sigma yq_{ic} + \gamma \mathbf{X}_{ic} + \alpha p_c + \tau_{ic} \quad (1)$$

In practice,  $yq$  enters the model as linear, quadratic, and cubic terms to reflect the development in time to adjudication over the data period (cf. Figure 1). Substantial variation in time to adjudication for otherwise similar cases comes from *when* during the data period the charge was filed. Predicting  $\widehat{time}$  from Equation 1 thus singles out variation in time to adjudication that arose from the policy reform for otherwise comparable cases and individuals within the same (post-reform) policy jurisdictions. Therefore, in the second stage, Equation 2 measures the effect of time to adjudication (as defined from  $\widehat{time}$ ) on new criminal charges (*newcharge*):

$$newcharge_{tic} = \partial^t \widehat{time}_{ic} + \beta \mathbf{X}_{ic} + \omega p_c + \varepsilon_{tic} \quad (2)$$

Here,  $i$ ,  $c$ ,  $\mathbf{X}$ , and  $p$  are identical to Equation 1,  $\varepsilon$  is the error term that is iid, and  $t$  reflects that in practice, the model is fitted for each of the 60 months during the follow up period (note also that, as explained, *newcharge* is measured both as binary and count variables). Using this model, estimates of  $\partial^t$  reflect the effect of one additional month of time to adjudication on new convictions up to month  $t$  of the follow up period for the offenders whose cases were delayed because of the reform (a local average treatment effect). One advantage of running separate regressions for each month of the follow up is that it makes it possible to show how the effect materializes over the follow up period. Note that as mentioned, individuals may occur in the data on more than one case during the data window (as also indicated by subscript  $c$  in Equations 1 and 2), and I therefore cluster standard errors at the individual level. I also confirm that overall conclusions are insensitive to the potential threat of multiple hypothesis testing, using the sharpened p-value approach developed in Benjamini, Krieger, and Yekutieli (2006) and implemented in Anderson (2008).

In addition to the main results, the Results section also summarizes the results from several sensitivity checks and subgroup analyses. Moving away from instrumental variable estimation, the first check applies a classical pre / post analysis of the impact of the reform on rates of criminal recidivism. Here, data during the reform implementation period (2006-2008) are excluded from analysis, and reform regime is defined by whether someone was charged prior to reform implementation (2005) or after it (2009). Results from this exercise represent estimates of the average impact of the reform (an intention-to-treat parameter) but although these point estimates are not directly comparable to the main estimates (a local average treatment parameter), results should uniformly point in the same direction to support the main findings. Another sensitivity analysis weighs the data by “street time” to consider that the people in the data may be incarcerated (and hence incapacitated), emigrated, or deceased during the follow up period. The weights are obtained from combining information from the incarcerations register, the population register, and the death register. *Street time* thus measures the proportion of the follow up months up to the current one, that the person was in the country and not incarcerated or dead, i.e., the proportion of the follow up period that the person was at risk of new offenses. Last, I perform several subgroup analyses to evaluate the similarity of results across groups in the data. Here, I summarize the results by criminal history, by how fast after the offense a person was arrested, by whether the person was detained pretrial or not, by sentence length, by age, and by how extremely slow cases are handled in the data (at which percentile time to adjudication is top coded).

***Exclusion restriction.***

In addition to providing substantial variation in time to adjudication, which was previously shown to be the case, but which will also be proven in the Results section, Equations 1 and 2

require that the policy reform only affected new convictions through its impact on time to adjudication (this is known as the exclusion restriction). In support of this assumption, I analyzed whether the background characteristics of the charged persons and their cases vary systematically over the observation period. In practice, I did so by regressing time to adjudication on all observed background characteristics and post-reform police jurisdiction fixed effects in a dataset that was identical to my main data but taken from the period before the main data (i.e., 1995-2003), at a point when the reform could not have affected criminal justice processes. I then used the parameter estimates from that model to predict time to adjudication in the main data; the predictions effectively summarize whether we should expect time to adjudication to fluctuate over time simply because of the composition of the data on the background characteristics. Figure 2 shows that this is clearly not the case, as predicted time to adjudication is stable across the period and the slope of the predictions is meager (the slope is just -0.005).

[Insert Figure 2 about here]

As another test of the assumption, Table A1 reports the results from separate regressions of each of the background and case characteristics on the instrumental variable. Results indicate some significant time variation in 16 out of the 50 variables used to measure background characteristics (categorical variables treated as dummies), when evaluated from whether the related F-test exceeds 10. Yet the associated parameter estimates are numerically small, which is also reflected in very low predictive values of the models: R-squared is below 0.005 for all regressions and even below 0.001 for all but eight of the regressions. As such, although when considered separately, there are minor signs of fluctuations in the data over time (fluctuations that bear little predictive power), conditioning on the entire set of background characteristics as in Figure 2, this concern is tempered.

Also in support of the exclusion restriction, I analyzed trends in other features of the Danish criminal justice system over the data period and found no substantial responses to the policy reform in these features (number of convictions, reported crimes, filed cases, violent crimes, property crimes, and the police's case clearance rate and the rate of cases on which the charged and convicted offense differs (and admittedly imperfect gauge of prosecutor ambition on the case, which could decrease with the reform)); the trends in these features are shown in Figure A1). One exception concerns time from offense to charge (bottom right subfigure in Figure A1), which increases following the reform and could pose a threat to identification. I evaluate this threat as minor, however, for which there are two reasons. First, the timing of the increase in this time parameter does not match with the reform. Second, and more importantly, the period from offense to charge is generally very short in Denmark, and the increasing trend in the figure, which may seem dramatic because it is shown relative to its level in 2004, merely represents an increase in average time from offense to charge of 3 to 5 days (from 5-6 to 9-10 days). Although an increasing trend, this level should be compared to the increase of up to 150 days for time to adjudication, the period of interest to this paper.

Last, as still other features of the Danish context may have fluctuated alongside the policy reform, I run several robustness checks of the main findings. First, because unemployment and crime are notoriously connected (e.g., Raphael and Winter-Ebmer, 2001), I check whether results are robust to the trend in unemployment over the period. I estimate this rate as the percentage of the male workforce that were dependent on public benefits (excluding paternal leave and education benefits) in any given month. In the robustness check I normalize my outcome variables by this trend. Second, general trends in crime may also have impacted the risk of new convictions even absent the policy reform. As the policy reform was national and therefore does not straightforwardly offer a comparison group from Denmark, I check whether results are robust to such crime trends by normalizing the outcome

variables by the crime trend in Denmark's neighboring country, Sweden. For Sweden, the trend includes all conviction decisions ("lagföringsbeslut") for people (Statistics Sweden Table 40A). Last, because there may be differences in time trends in the data for Denmark and Sweden, I sought out two offense types in the Danish data whose average time to adjudication were not affected by the 2007 policy reform: Drug trafficking and fraud against unemployment insurance funds. I then reran the main results only on these offense types; signals of meaningful "effects" of policy induced court delays (that did not in fact take place for these cases) would indicate fundamental flaws in my analytical setup (which turns out not to be the case; results from these analyses are consistently zero).

Of course, I cannot firmly reject that there could be other changes to the policy climate and the macrostructural constraints that surround people in the data and are relevant to criminal recidivism risks but are not considered in my long list of robustness and sensitivity checks. For the same reason, results presented in this paper should be viewed as highly suggestive estimates, not cleanly identified causal effects. With the setup at hand there is no obvious way to work around this limitation – but results presented here should nonetheless be less subject to bias than straightforward comparisons of slow and fast cases. For that reason, my strategy is to return to the question of what other changes could have happened and how – if they did – they would likely cause bias in my results; I do so in the Discussion section.

## **Results**

Table 1 summarizes the data across background characteristics. The first table column reports summary statistics for the full dataset. Average time to adjudication is 249 days (a little more than eight months) and charges are almost uniformly distributed across the years in the data window. The composition of the data is as one would expect (young, poorly educated and predominantly male, with weak attachment to the labor market; a high proportion has prior

criminal justice contact). Most are charged with property crimes and most end up getting fined; less than one in five are imprisoned (and those who are imprisoned are, on average, sentenced to 7 months of imprisonment; comparatively short sentences as one would expect from the Danish context).

[Insert Table 1 about here]

Columns 2-4 of Table 1 report summary statistics across the distribution of time to adjudication. The means of most background characteristics are similar across this distribution, although a few important differences stand out. First (and obviously), time to adjudication differs. For the 25 percent of the data that have the shortest time to adjudication, the mean is just 40 days while for the 25 percent with the longest time to adjudication it is longer than 600 days (almost 21 months). The proportion of females is much higher in “fast” cases, perhaps implying that females tend to get charged with less serious (and thus likely less complex) crimes that are easier to solve. The same is true for the proportion of cases that result in fines (higher among the fast cases) which is counterbalanced by a higher proportion of cases that result in probation among the slower cases. Those who are charged with violent crimes have shorter time to adjudication whereas the proportion charged with property crimes increases with time to adjudication. Last, there are signs that cases that involve pretrial detainment are processed faster, which makes sense considering the strong incentive for entering plea bargain during pretrial detainment.

### ***Results from Instrumental Variable Models***

Table 2 summarizes the estimation results from the first stage model presented in Equation 1, and thus reports the estimated impact of when the charge occurs on time to adjudication.<sup>5</sup> The first table column does not control for anything and therefore represents the “raw” correlation between time to adjudication and when the charge was filed (corresponding to what was shown in Figure 1). The second column controls for police jurisdiction fixed effects to consider that reform implementation may have been handled differently across jurisdictions, just as there may be geographically related features that are unobserved in the data. The third table column adds background variables to control for compositional fluctuations over time. And the last table column adds control variables related to the criminal case in question, such as offense type.

[Insert Table 2 about here]

Across the table columns, the association between calendar time and time to adjudication is of the same functional form (signs and sizes of parameter estimates are similar) and adding control variables thus mainly increases the model’s predictive power (large increase in  $R^2$  across the models). But even more important for this study is the finding that *when* the charge occurs matters substantially for time to adjudication across all models. This is seen from the F-tests of excluded instruments, which in all instances exceed what would typically be considered the threshold for whether an instrument is weak (threshold F-value of 10; values in my setup exceed a thousand, which also removes weak instrument concerns raised in the more recent methodological literature, Lee, McCrary, Moreira, and Porter 2021).

Having established that the instrumental variable is a strong predictor of time to adjudication in the data, Figure 3 reports the results from the second stage model presented in Equation 2. Panel A shows results for the cumulative binary outcome for each of the 60

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<sup>5</sup> See Table A2 for the full set of parameter estimates.

months during the follow-up period; Panel B shows results for the cumulative count outcome. Both outcomes document an impact of time to adjudication on criminal recidivism, but the nature of this impact differs across the outcome variables in a meaningful way.

[Insert Figure 3 about here]

First, in the short run – within the first two years following the initial charge – increasing time to adjudication *decreases* criminal recidivism. Remarkably, this pattern corresponds to when most people in the data had been charged with a crime but had not yet been adjudicated (a period which Andersen, 2020 also found to decrease rates of new charges). Once most people in the data have reached adjudication, rates of new offenses increase steeply (95 % of the cases in the data have reached adjudication when the 2SLS estimates reported in Figure 3 cross zero).

Second, in the long run – five years following the initial charge – results differ by outcome definition. Results for the binary outcome document no long-term effect of time to adjudication on the share with new crimes. The parameter estimate is virtually zero and 95 % confidence intervals rule out estimates outside the narrow -0.002 to 0.002 window.

Considering that more than half of the people in the sample will have recidivated at this point, the related effect size would be tiny even if statistically significant. But results for the count outcome document a substantial increase in recidivism after five years, despite the initial decrease. Five years out, one additional month from charge to adjudication increases reconviction by (on average) 0.019 offenses, which is statistically significant ( $p < 0.001$ ). To give context to the size of this point estimate, average time to adjudication increased from around 6 months to 10 months across reform implementation, implying that reform-induced court delays may have increased the cumulative rate of new offenses (that led to conviction) after 5 years by on average around  $(10 - 6) \times 0.02 = 0.08$  convictions per person (as the data

consist of about 160,000 persons over four years, delaying all cases by three months would thus imply, on average,  $0.08 \times (160,000 / 4) = 3,200$  additional crimes among people charged within one year).

### ***Summary of Results from Additional Analyses***

Almost all sensitivity and robustness checks which I outlined in the Methods section arrive at the same overall conclusion as the main results for each definition of the outcome variable (Figures A2 and A3 summarize the sensitivity and robustness results for the risk of new charges and number of new charges, respectively). The magnitude of estimates differs across subsamples, and estimated confidence intervals vary in width because of the different sample sizes used in each estimation. But the most important point is the overall structure of the results: Increased time to adjudication lowers criminality in the short run but in the longer run it increases the average number of criminal reconvictions although not the share reconvicted. Results show that this holds also when splitting the data by criminal history, by how fast after the offense a person was arrested, by whether the person was detained pretrial or not, by sentence length, and by age, and by how extremely slow cases are handled in the data (at which percentile time to adjudication is top coded). In addition, Table A3 in appendix shows results from the pre / post analysis of the reform's effect on reconviction, as it was laid out in under Methods. Results are, again, very similar to the main results, and thus provide very little evidence that the main results might be driven by methodological choices rather than expressing substantive associations.

Results that normalize the outcomes by the trend in unemployment and the conviction rate in Sweden show the same overall pattern as the main results, with the one exception that when using normalized outcomes, there are statistically detectable effects also on the rate of

criminal recidivism after five years. As such, it should be noted that the “null finding” for the binary outcome could to some degree be caused by general time trends in the data. The last set of analyses, which focus on offense types in Denmark that happened to *not* be delayed by the reform, consistently return estimates close to zero, again implying that general trends in the data are unlikely to drive the main results (Figures A4 and A5).

## **Discussion**

Delays at court seem consequential for criminal recidivism and hence represent a feature of the criminal justice system that calls for special and continuous attention. This is the overall conclusion that can be drawn from this study. To arrive at this conclusion, I relied on data from before, during, and after a policy reform in 2007 which restructured the Danish national police and the Danish courts and as a side-effect produced massive delays in the courts. Average time to adjudication increasing from around 6 months just before the reform to around 10 months after it (and which did not retrieve its pre-reform level until 2011/12). Exploiting the accurate linkages in Danish register data, I focused on people charged with criminal offenses before, during, and after the policy reform and tracked their rates of new convictions up to 5 years from their initial charge.

Results show that increasing time to adjudication suppresses new offenses (for which a person is later convicted) during the period from the original charge and until adjudication. This finding is theoretically meaningful if we think about it in terms of when deterrence effects of an ongoing case are likely to prevail versus when we might expect the damaging consequences of criminal conviction to materialize. But reconviction rates then catch up with rates for those who had shorter time to adjudication, and after 5 years there is no traceable impact of time to adjudication on criminal recidivism. This result only holds for the

reconviction *rate*, i.e., the share who relapse into crime within the first five years. Looking at the average *frequency* of new convictions, however, a more concerning result emerges.

Although increasing time to adjudication also decreases the average frequency of recidivism in the short run, the average number of criminal reconstructions after 5 years was higher among those who were hit by court delays.

The theoretical implications of this study are that the link between celerity of punishment and new crimes should perhaps not be grasped with one and the same etiology for all points in time following the index crime. Rather, results are more in line with the existence of two independent time periods, each with their own link to new crimes. The first period, from index crime to adjudication, is likely one of uncertainty and stress and may in many ways resemble a pre-sentence sentence (cf. Laursen, Mjåland, and Crewe 2020). During this period, people may also be more under the criminal justice system's surveillance, either mechanically from pretrial detainment or more broadly from just being on the criminal justice system's radar. These observations and the finding that the risk of new crimes is lower during this period implies that strong deterrence effects may be at play. This interpretation is also consistent with the findings in Jaynes and Wilson (2022) that people prefer immediate sanctions over delayed ones, perhaps in realization that the potential punishment and risk of getting caught are higher when having a criminal case pending at court. This same logic may then explain why the amount of recidivism increases after adjudication, the second distinct period of the link between celerity and new crimes. Here, the additional surveillance, sanction threat, and certainty of punishment of new crimes while having a pending case no longer apply – which in turn may unleash the criminogenic consequences of longer time to adjudication, such as the stressful effects of being under more intensive surveillance for a longer period.

Importantly, although both the probability of new crimes and the number of new crimes decrease during the first distinct period – implying that the crime suppressing effects during this period of additional surveillance are general – longer time to adjudication only substantively increases the average *number* of new crimes within the first five years, not the share of people who are convicted of new crimes. This implies that the unleashing of damaging effects that built up while having a case pending for longer only applies to those who would have committed new crimes anyway. This is nonetheless still an important finding. For perspective, consider that the average number of reconvictions after 5 years increases by around 0.02 new crimes for each (exogenously given) month of additional time to adjudication. When a group of 10 persons must wait an additional five months for adjudication, one person in that group will be convicted of 1 additional crime within 5 years, on average, although only half the group is reconvicted within 5 years. Court delays thus push people who persist with criminal offenses even deeper into the criminal enterprise. Adding this damaging consequence to the broader range of consequences of court delays, such as administrative, justice, and individual ones, implies that only few if any good things can be said about court delays.

With the COVID-19 pandemic, courts throughout the world experienced substantial delays and case backlogs. Many state courts as well as federal courts in the US halted judicial processes while the virus spread, and by late April 2020, for example, 39 US states had statewide orders suspending in-person proceedings, according to the National Center for State Courts ([nsc.org](https://nsc.org), visited on April 24, 2020). In England and Wales, the backlog of criminal court cases passed 37,000 in late March 2020 and more than half of court buildings were closed. Denmark, the context of this study, was one of the first countries to lock down to fight the spread of the virus (with a nation-wide lockdown in effect from March 11, 2020). The lockdown hit ‘non-essential’ court proceedings too, and although the country was gradually

reopening by late April 2020 due to low rates of new COVID-19 infections, courts had de facto been closed for weeks and most criminal cases were postponed indefinitely, again leading to massive case backlogs (time to adjudication has doubled in 10 out of 24 courts from 2016 to 2021, Rigsrevisionen 2022). As per the results presented in this paper, we should thus expect an additional collateral consequence of the pandemic to manifest in the years to come: increasing criminal recidivism among recidivists. There are, of course, several differences between policy-induced court delays in Denmark in 2007 and global court delays stemming from the pandemic, and it is unclear just how informative results from this paper are for the current challenge. But representing the only available evidence of the effect of court delays on recidivism, results presented in this paper should at least cause concern over the aftermath of the pandemic.

There are two core limitations to this study. First, how well do results from Denmark transfer to other countries? There is no straightforward answer to this question. This is because, on the one hand, Denmark differs from other contexts along many margins, such as along the nature of the social security net, the incarceration rate, etc., as was also discussed earlier in this paper. On the other hand, results presented in this paper are based on a natural experimental setup that (as it is argued but see below) focuses on variation on just one margin of the criminal justice system – time to adjudication. And although the rate of new criminal convictions may be context dependent, this would mostly seem to concern the *level* of response in the outcome, not the *structure* of the response, and I view the latter as the most important result presented here: Criminality decreased in the short run but then the rate of recidivism was unaffected after 5 years, whereas the intensity of criminality among the recidivists increased (and the same structure is at work across various subgroups in the data, as shown in the sensitivity and robustness analyses – but it should be noted that results for the criminal recidivism rate could to some degree be biased downward by general time trends as

expressed from the unemployment rate in Denmark and the conviction rate in the neighboring country, Sweden).

The second limitation concerns research design and challenges the assumption that results in this study only reflect changes in time to adjudication and not in other relevant factors that could have been affected by the reform. As was discussed, the reform was large and national and focused on increasing the education level of police and of public prosecutors and increasing efficiency by streamlining registration practices and centralizing key administrative tasks. Some of these goals may have been met, making the post reform criminal justice regime in Denmark of higher quality and efficiency than the one prior to the reform, which would obviously challenge the research design. Official evaluations of the reform implementation, however and as mentioned, found that only very few of these goals had been met by 2009 and that the main response to the reform was court delays. My way of confirming that the potential threat of other changes was low was to plot the trends in several key dimensions of criminal justice system performance over the relevant period; I found very few anomalies in these processes. I also performed a long line of sensitivity analyses – including analysis of offense types that remained unaffected by the reform induced court delays, again implying few-to-no anomalies. But other changes to the broader policy and economic climate in Denmark over the data period could of course also condition the relationship between when a criminal charge was filed and eventual criminal recidivism, which could lead to bias in the estimates presented in this paper.

[Insert Table 3 about here]

To give some indication of the extent of the threat, Table 3 summarizes a selection of relevant changes that occurred during the data period and the potential bias they may have caused. The table indicates that there are some changes to macrostructural conditions that

could potentially introduce bias, which is why I caution against strong causal interpretation of the results presented in this paper. Rather, results should more rightly be seen as hopefully highly informative associations. To bolster the informative nature of the results, Figure A6 shows results comparable to the main results but while including dummy variables for whether each month of the follow up period for each person in the data was before or after each of the reforms listed in Table 3 (the potential impact of decreasing trend in convictions over the data period was already analyzed under sensitivity analyses). Results naturally become noisier (because the included dummy variables inevitably correlate with the instrumental variable, calendar time), but the overall conclusions nonetheless persist. I of course invite future studies to challenge the research design and to exploit designs that rely on changes in time to adjudication that are more straightforwardly independent of other features of criminal justice systems and individual offenders.

### **Acknowledgments**

The author thanks the working group on Nordic register-based studies in criminology, workshop participants at the Netherlands Institute for the Study of Crime and Law Enforcement (NSCR), Valerio Bacak, Jason Schnittker, and Peter Fallesen for comments to previous versions of the manuscript, and thanks Martin L. Trinhammer for research assistance. Any errors are, of course, the author's responsibility.

### **Declaration of Interests**

The author reports there are no competing interests to declare.

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**Table 1** Descriptive Statistics, Full Sample and by Time to Adjudication. Denmark, 2005-2008.

	Full data M / (SD)	By time to adjudication		
		Shorter than lower quartile M / (SD)	Between lower and upper quartiles M / (SD)	Longer than upper quartile M / (SD)
Days to adjudication	248.674 (275.896)	40.149 (16.830)	164.836 (68.430)	625.052 (309.737)
Year of criminal charge				
2005	0.272 (0.445)	0.326 (0.469)	0.291 (0.454)	0.181 (0.385)
2006	0.255 (0.436)	0.283 (0.450)	0.255 (0.436)	0.228 (0.420)
2007	0.239 (0.427)	0.208 (0.406)	0.233 (0.423)	0.283 (0.450)
2008	0.233 (0.423)	0.184 (0.387)	0.221 (0.415)	0.307 (0.461)
Demographic background				
Female	0.178 (0.383)	0.241 (0.428)	0.163 (0.370)	0.145 (0.353)
Age	28.511 (11.344)	29.840 (12.186)	27.931 (11.152)	28.348 (10.726)
Ethnic minority background	0.222 (0.416)	0.224 (0.417)	0.218 (0.413)	0.228 (0.419)
Has children	0.688 (1.132)	0.719 (1.144)	0.680 (1.129)	0.674 (1.123)
Married	0.135 (0.342)	0.167 (0.373)	0.117 (0.321)	0.139 (0.346)
Police jurisdiction <sup>a</sup>				
– 1	0.087 (0.281)	0.110 (0.313)	0.088 (0.283)	0.061 (0.239)
– 2	0.103 (0.304)	0.113 (0.317)	0.100 (0.300)	0.100 (0.300)
– 3	0.079 (0.270)	0.088 (0.283)	0.079 (0.270)	0.071 (0.257)
– 4	0.073 (0.260)	0.076 (0.265)	0.075 (0.264)	0.064 (0.245)
– 5	0.079 (0.269)	0.081 (0.272)	0.079 (0.269)	0.076 (0.266)
– 6	0.097 (0.296)	0.109 (0.312)	0.100 (0.300)	0.077 (0.266)
– 7	0.068 (0.251)	0.064 (0.245)	0.065 (0.247)	0.076 (0.265)
– 8	0.068 (0.252)	0.054 (0.226)	0.069 (0.254)	0.079 (0.269)
– 9	0.078 (0.268)	0.079 (0.270)	0.077 (0.266)	0.080 (0.271)
– 10	0.089	0.078	0.083	0.113

	(0.285)	(0.268)	(0.276)	(0.317)
– 11	0.172	0.136	0.177	0.199
	(0.378)	(0.342)	(0.382)	(0.400)
– 12	0.007	0.011	0.007	0.004
	(0.085)	(0.105)	(0.084)	(0.064)
Educational attainment				
Elementary school	0.775	0.740	0.795	0.771
	(0.417)	(0.439)	(0.404)	(0.420)
Upper sec. education	0.038	0.041	0.035	0.041
	(0.191)	(0.198)	(0.184)	(0.199)
Vocational training	0.148	0.166	0.138	0.149
	(0.355)	(0.372)	(0.345)	(0.356)
Tertiary education	0.039	0.053	0.032	0.038
	(0.194)	(0.224)	(0.177)	(0.192)
Years of education	8.551	8.691	8.415	8.685
	(4.118)	(4.262)	(4.091)	(4.017)
Missing years of education	0.151	0.154	0.156	0.140
	(0.358)	(0.361)	(0.363)	(0.347)
Prior labor market status				
Employed	0.416	0.423	0.400	0.443
	(0.493)	(0.494)	(0.490)	(0.497)
Unemployed	0.067	0.063	0.067	0.070
	(0.250)	(0.243)	(0.250)	(0.256)
Outside labor force	0.517	0.514	0.533	0.487
	(0.500)	(0.500)	(0.499)	(0.500)
Post-tax income				
Less than lower quartile	0.227	0.215	0.234	0.222
	(0.419)	(0.411)	(0.424)	(0.416)
Between lower and upper quartiles	0.521	0.498	0.535	0.518
	(0.500)	(0.500)	(0.499)	(0.500)
Above upper quartile	0.252	0.287	0.231	0.260
	(0.434)	(0.452)	(0.422)	(0.439)
Contact w/ mental healthcare system				
Within last year	0.060	0.065	0.062	0.051
	(0.238)	(0.247)	(0.242)	(0.220)
Any prior contact	0.174	0.177	0.179	0.160
	(0.379)	(0.382)	(0.384)	(0.367)
Criminal history				
Age at first conviction	22.570	24.167	21.946	22.226
	(9.450)	(10.746)	(8.843)	(9.056)
No prior convictions	0.297	0.353	0.280	0.276
	(0.457)	(0.478)	(0.449)	(0.447)
– 1	0.150	0.148	0.151	0.149
	(0.357)	(0.355)	(0.358)	(0.356)
– 2	0.097	0.093	0.098	0.101
	(0.297)	(0.290)	(0.297)	(0.302)
– 3	0.069	0.061	0.071	0.072
	(0.253)	(0.240)	(0.256)	(0.259)

- 4	0.052 (0.222)	0.046 (0.209)	0.053 (0.225)	0.055 (0.227)
- 5+	0.335 (0.472)	0.299 (0.458)	0.347 (0.476)	0.347 (0.476)
No prior incarceration	0.631 (0.483)	0.671 (0.470)	0.621 (0.485)	0.610 (0.488)
- 1	0.151 (0.358)	0.139 (0.346)	0.158 (0.365)	0.150 (0.357)
- 2	0.065 (0.246)	0.056 (0.230)	0.066 (0.249)	0.070 (0.255)
- 3+	0.154 (0.361)	0.134 (0.340)	0.155 (0.362)	0.171 (0.376)
Current charge				
Charged w/ violent crime	0.265 (0.441)	0.378 (0.485)	0.267 (0.442)	0.147 (0.355)
Charged w/ property crime	0.621 (0.485)	0.562 (0.496)	0.626 (0.484)	0.672 (0.470)
Detained pretrial	0.072 (0.259)	0.117 (0.322)	0.062 (0.242)	0.047 (0.212)
Case acquitted	0.296 (0.456)	0.313 (0.464)	0.271 (0.444)	0.329 (0.470)
Fined	0.702 (0.457)	0.768 (0.422)	0.716 (0.451)	0.607 (0.488)
Probation	0.167 (0.373)	0.094 (0.292)	0.160 (0.367)	0.255 (0.436)
Imprisonment	0.170 (0.375)	0.175 (0.380)	0.167 (0.373)	0.171 (0.376)
Sentence length <sup>b</sup>	7.143 (13.968)	5.239 (5.858)	6.604 (11.189)	10.148 (21.988)
N	160,710	40,081	80,493	40,136

Note: Table shows means (M) and standard deviations (SD, in parentheses) of the background characteristics of the full sample and by quartiles of time to adjudication. <sup>a</sup> Police jurisdictions refer to the jurisdictions after the reform. <sup>b</sup> Number of observations with imprisonment (and hence valid sentence length) are 27,279 (column 1), 7,022 (column 2), 13,406 (column 3), and 6,851 (column 4).

**Table 2** Results from First Stage Regression of Time to Adjudication on Quarter and Year of Charge. Denmark 2005-2008.

Model	1	2	3	4
Quarter by year	-0.201*** (0.042)	-0.192*** (0.042)	-0.198*** (0.041)	-0.173*** (0.039)
- squared	0.093*** (0.007)	0.093*** (0.007)	0.095*** (0.007)	0.092*** (0.006)
- cubic	-0.004*** (0.000)	-0.004*** (0.000)	-0.005*** (0.000)	-0.005*** (0.000)
Police jurisdiction fixed effects		Yes	Yes	Yes
Standard control variables			Yes	Yes
Criminal case control variables				Yes
Model's explanatory power (R <sup>2</sup> )	0.020	0.031	0.042	0.149
F-test of excluded instruments	1028.679	1058.118	1084.869	1157.244
N	160,710	160,710	160,710	160,710

Note: Table reports parameter estimates and standard errors (in parentheses). Standard errors are clustered at the individual level. Dependent variable is time to adjudication measured in months.

Police jurisdictions refer to the jurisdictions after the reform.

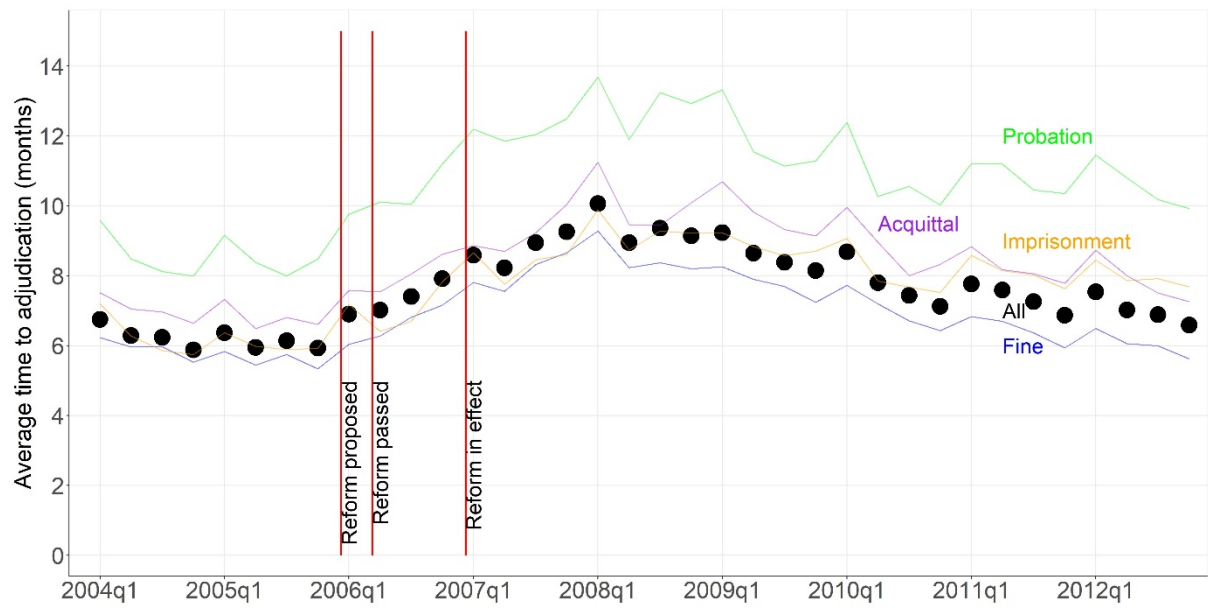
\* p < 0.05; \*\* p < 0.01; \*\*\* p < 0.001.

**Table 3** A selection of changes to policies and macrostructural constraints in Denmark during the data period (2005-2013) and the direction of likely biases that they could produce.

Area	Changes	Likely impact	Likely bias
Crime	Decrease throughout	Drive down recidivism rates, whereby people charged later in period (when time to adjudication was higher) have lower rates	Upward bias between charge and adjudication, downward bias after
Detection	May 24, 2005 expansion of DNA registration of charged persons	Higher detection rates	Negligible; in effect during 8.5 out of 9 years of the data period
Sentence length	No large reforms during 2005-2013	None	None
Sanction type	Expansions to noncustodial alternatives in 2005, 2006, 2008, 2010, and 2013	Alternatives shown to be less harmful than imprisonment, whereby recidivism rates may be lower	Upward bias between charge and adjudication, downward bias after
Probation and parole	No large reforms during 2005-2013.	None	None
Fine levels	No large changes during 2005-2013	None	None
Social welfare	No large reforms during 2005-2013	None	None
Public administration and healthcare	Structural reform in 2007	Municipal and healthcare administrative boundaries redrawn. May have impacted recidivism if e.g., healthcare delivery or service access got delayed or lost quality, or the opposite (which was the aim of the reform).	Unclear, depends on the actual impact of the reform
Economic	Global recession 2008 and on.	More hardship in later period when time to adjudication was high.	Upward bias between charge and adjudication, downward bias after

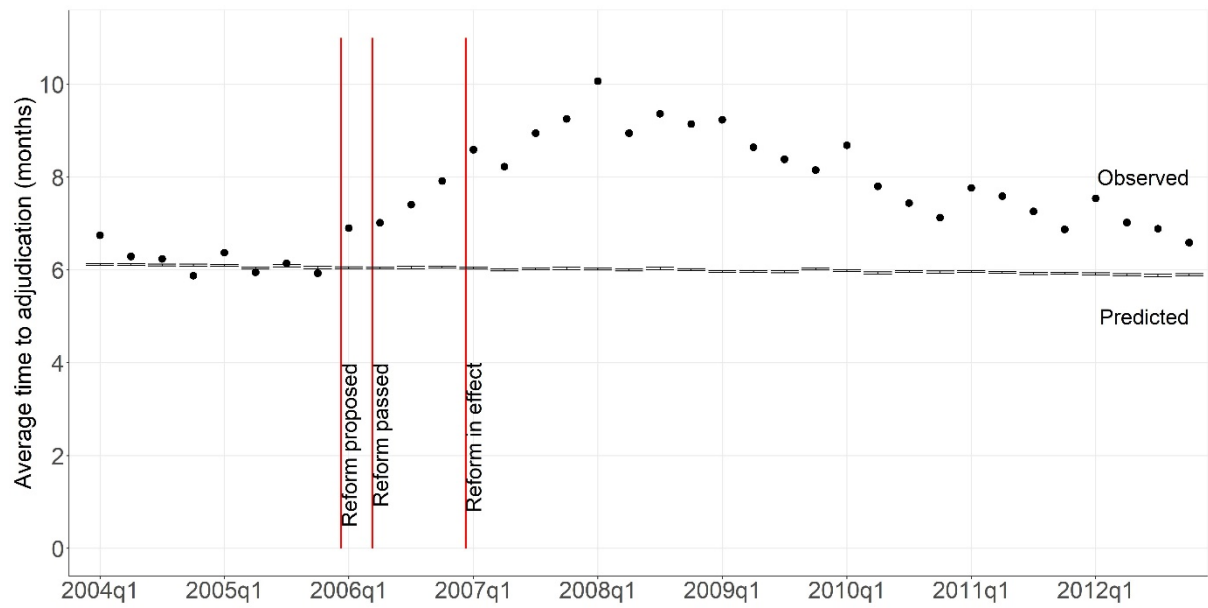
*Note:* Table summarizes major changes to relevant areas of policy and macrostructural constraints in Denmark over the data period and evaluates the potential threat of bias. It is important to note that cases with fast as well as slow time to adjudication exist throughout the entire data period whereby the risks of bias from the time dependent features described in the table do not uniformly correlate with time to adjudication.

**Figure 1** Average Time to Adjudication (Months), All Cases and by Case Outcome. Denmark 2004-2012.



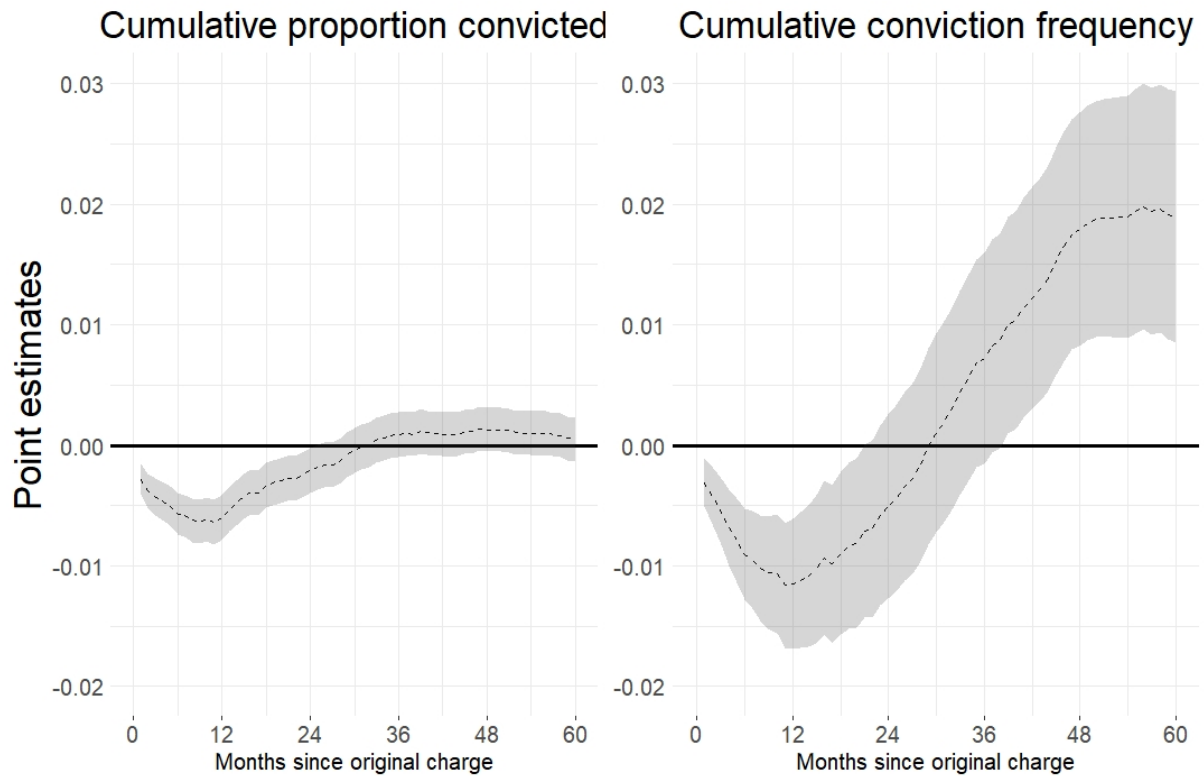
Note: Figure shows average time from criminal charge to adjudication in months by quarter of year, for all criminal cases and by case outcome. Vertical lines denote the timing of the 2007 reform's proposal, passing, and enactment.

**Figure 2** Average Time to Adjudication (Months) and Average Time to Adjudication Predicted from Background Characteristics Alone. Denmark, 2004-2012.



Note: Figure shows average time from criminal charge to adjudication in months by quarter of year. Vertical lines denote the timing of the 2007 reform's proposal, passing, and enactment.

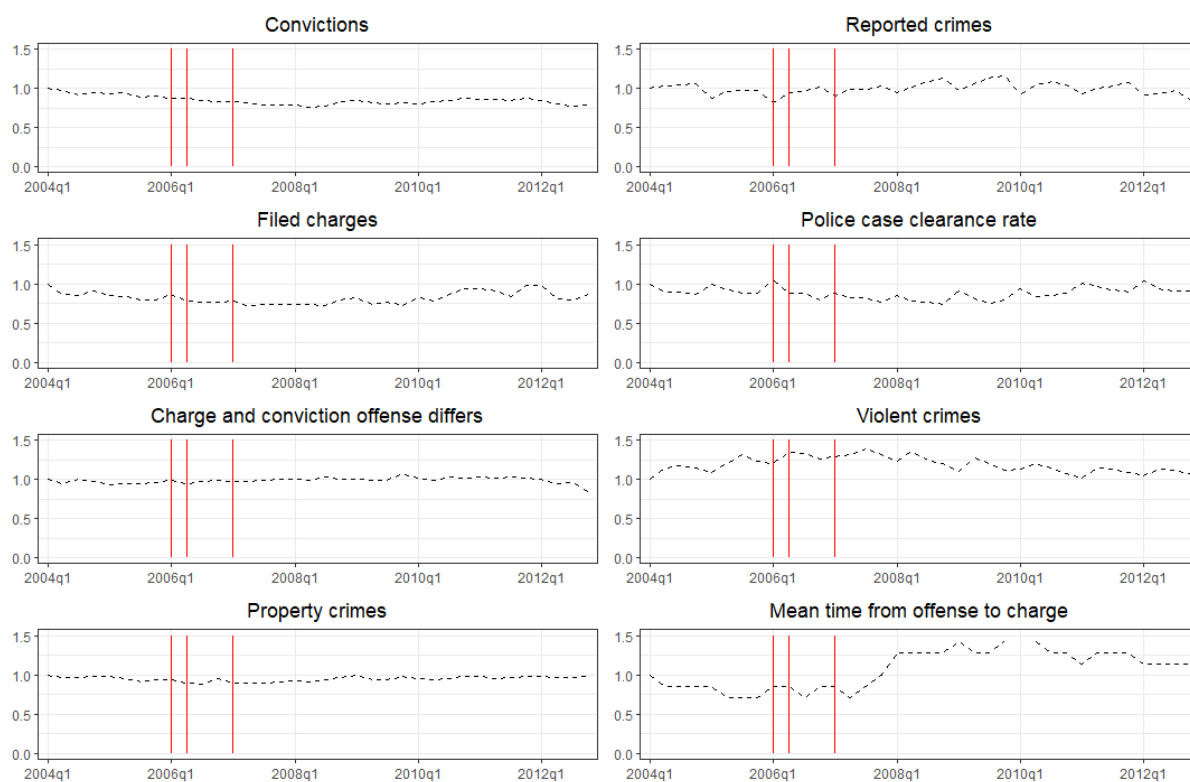
**Figure 3** Point Estimates of the Effect of Time to Adjudication (Months) on New Criminal Charges (Cumulative Proportion Convicted (left) and Cumulative Conviction Frequency (right)). Denmark, up to 60 Months Following Charge in 2005-2008.



Note: Figure shows the results from the second stage regressions of time to adjudication (in months) on new criminal charges for each of the 60 months of the follow up period. Left figure shows results for the cumulative binary outcome and right figure shows results for the cumulative number of new charges. Gray shadings represent 95% confidence intervals with standard errors clustered at the individual level. N = 160,710. Supplementary results (available from the author on request) using BKY sharpened p-values as developed by Benjamini, Krieger, and Yekutieli (2006) and implemented by Anderson (2008) shows that reported p-values are insensitive to multiple hypothesis testing.

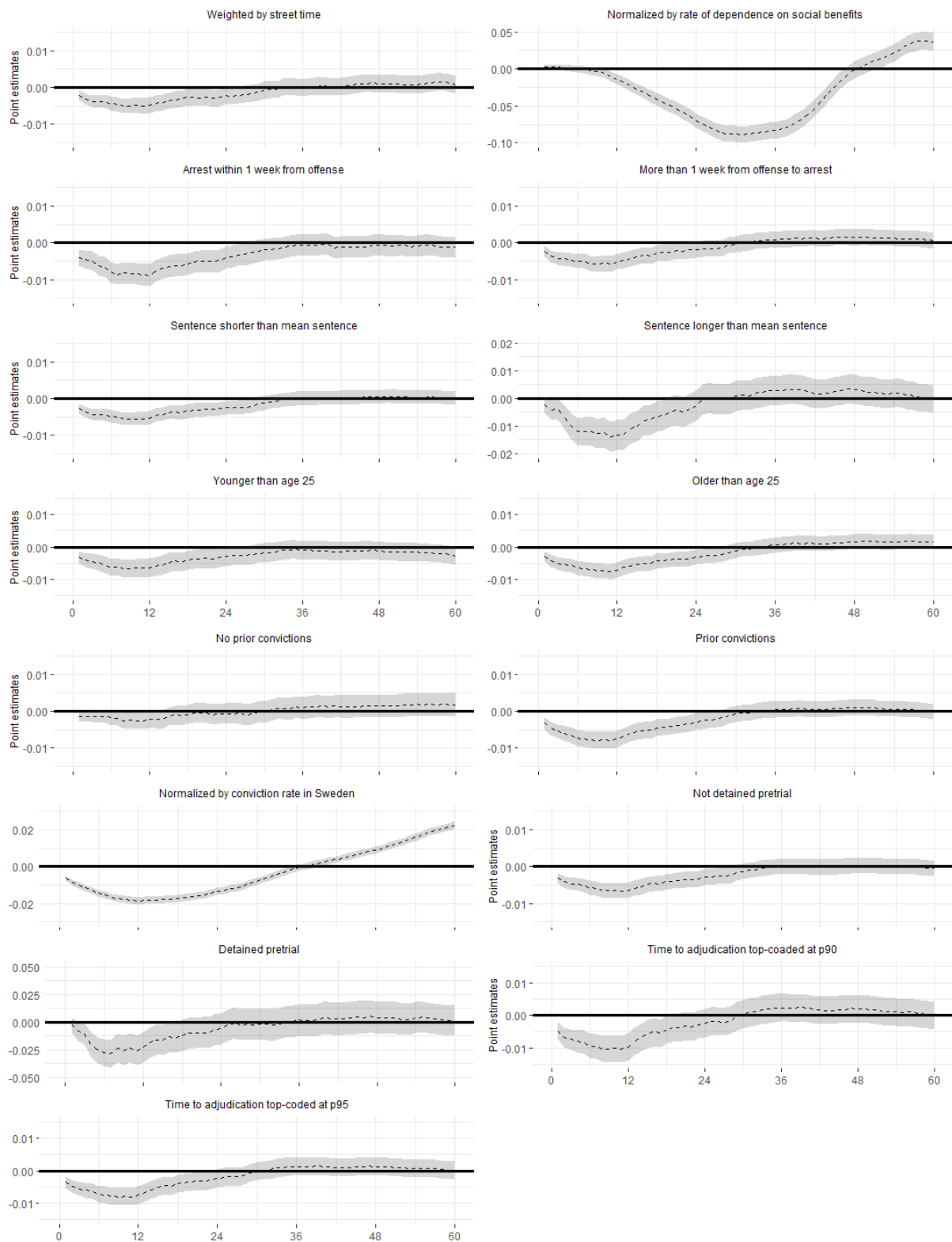
## Supplementary Material

**Figure A1** Trends in Various Margins of the Criminal Justice System. Denmark 2004-2012.



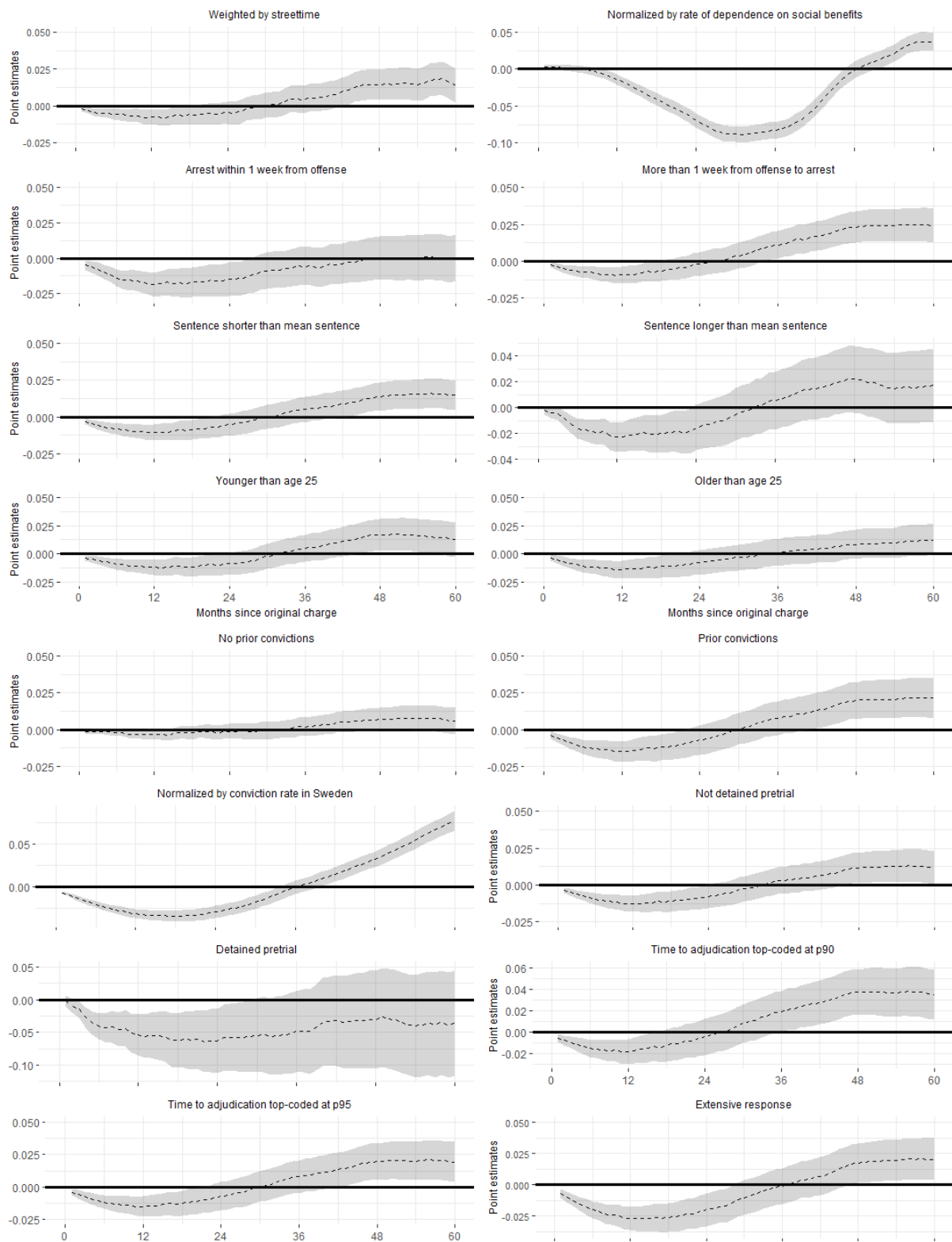
Note: Figure shows the trends (relative to the level in first quarter of 2004) of different margins of the Danish criminal justice system. Vertical lines denote the timing of the 2007 reform's proposal, passing, and enactment.

**Figure A2** Summary of Results from Sensitivity and Robustness Analyses of Cumulative Risk of New Charges. Denmark 2005-2008.



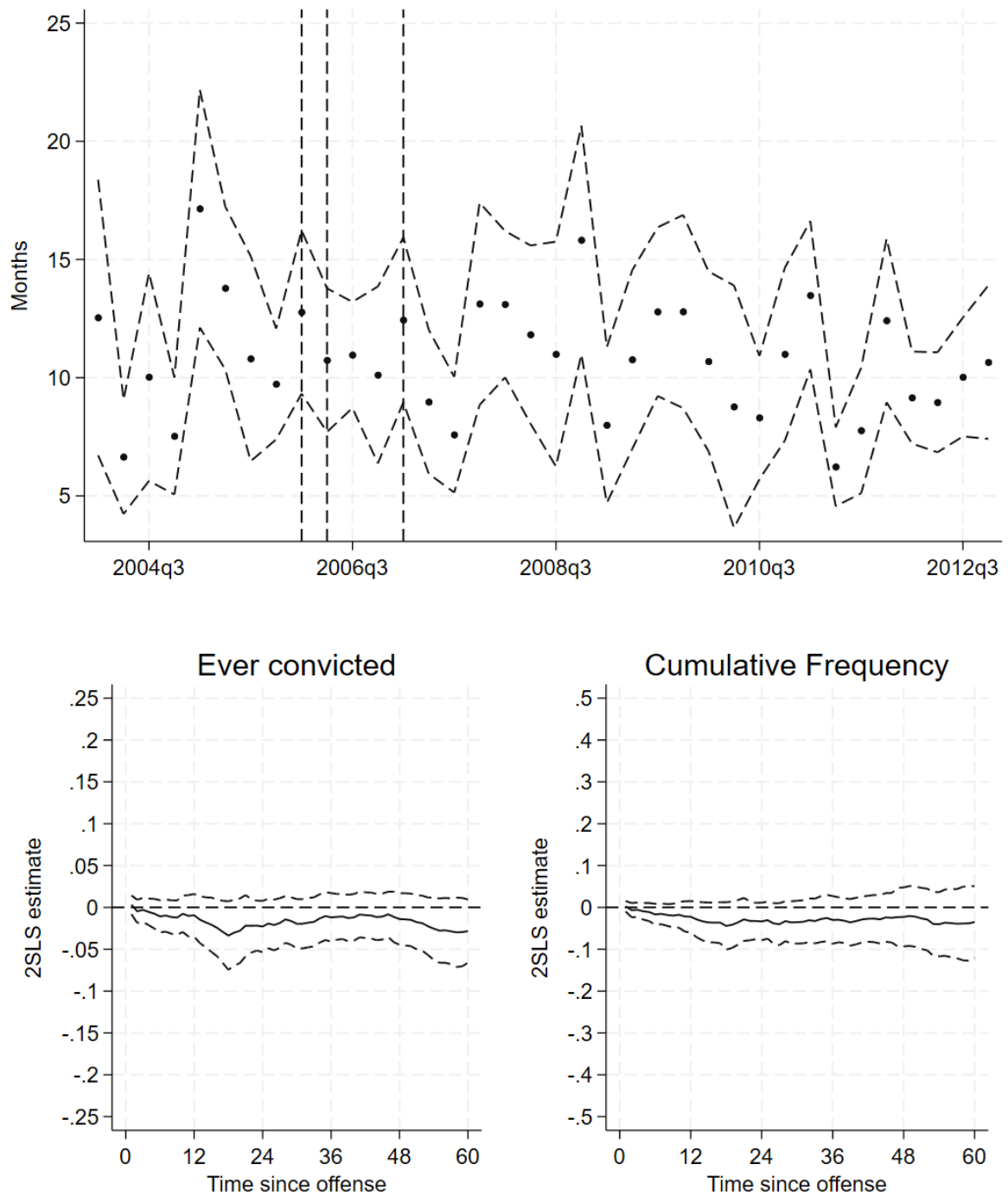
Note: Figure shows results for subgroups of the data. Gray shading denotes 95% confidence intervals with standard errors clustered at the individual level.

**Figure A3** Summary of Results from Sensitivity and Robustness Analyses of Cumulative Number of New Charges. Denmark 2005-2008.



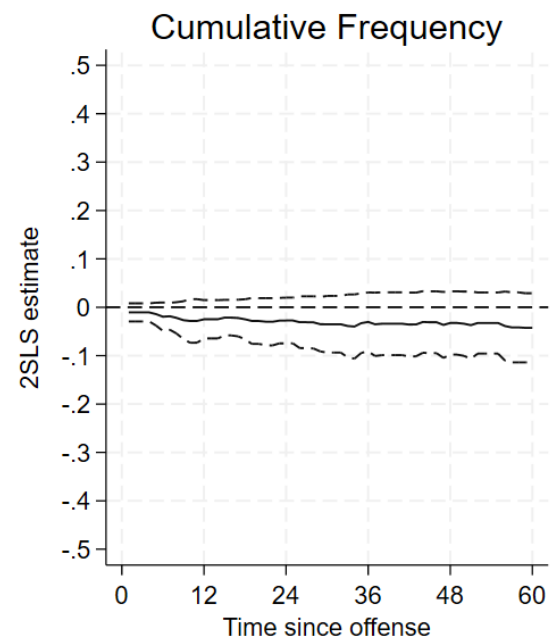
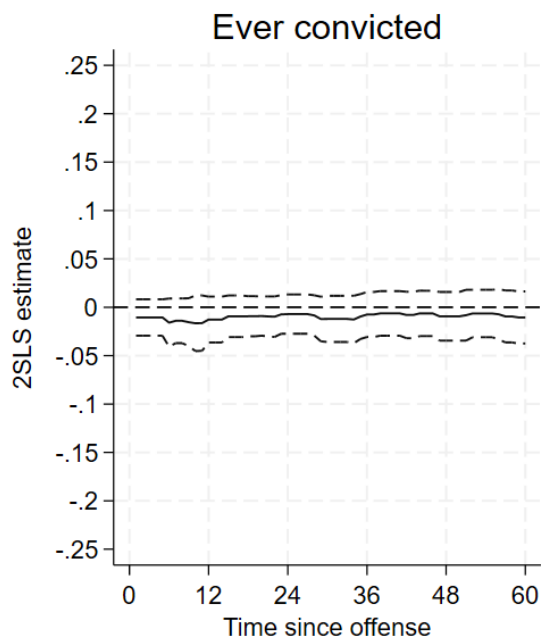
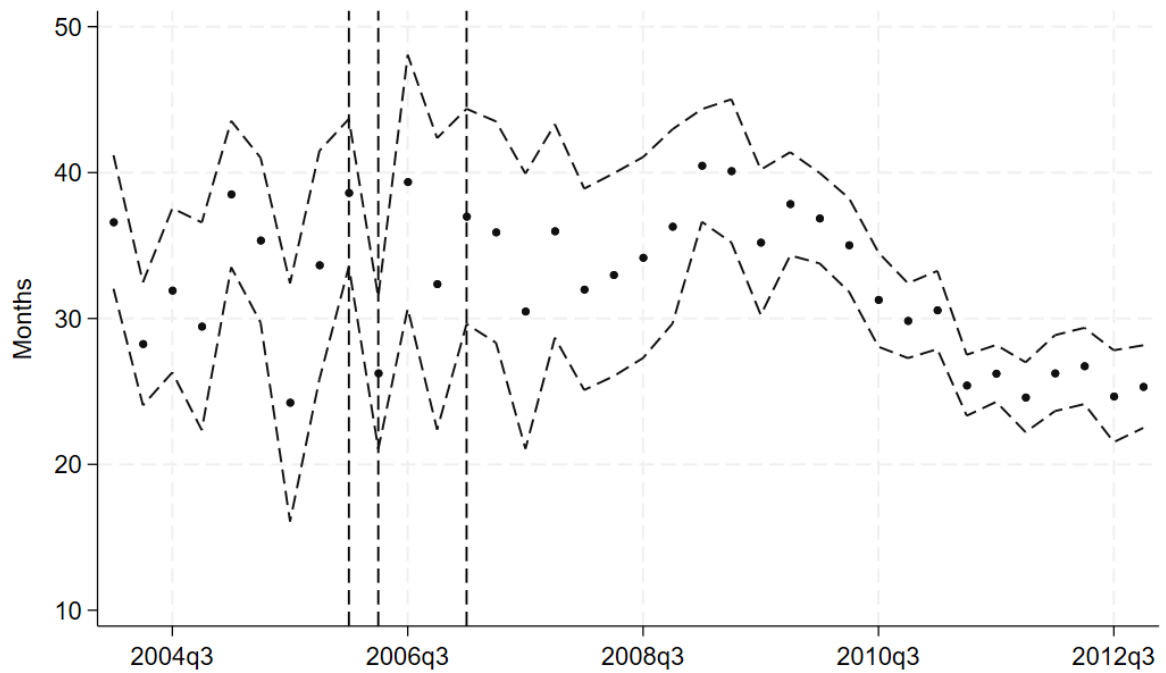
Note: Figure shows results for subgroups of the data. Gray shading denotes 95% confidence intervals with standard errors clustered at the individual level.

**Figure A4 Results for Trafficking of Drugs**



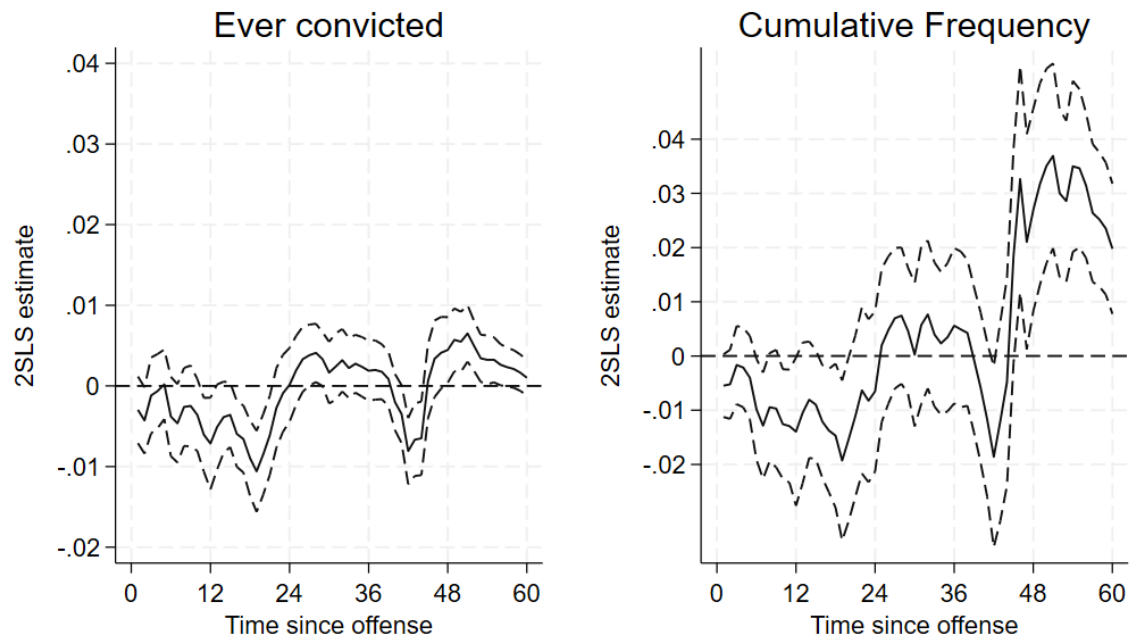
Note: Upper panel shows average time to adjudication in cases of trafficking of drugs during 2004-2012 (N = 1,233), and lower panels shows results corresponding to the main results in these cases (charges during 2005-2008 as in the main data, N = 620). Vertical lines in upper panel refer to the reform’s proposal, approval, and enactment (from left to right).

**Figure A5** Results for Fraud against Unemployment Insurance Fund



Note: Upper panel shows average time to adjudication in cases of fraud against unemployment insurance fund during 2004-2012 (N = 1,351), and lower panels shows results corresponding to the main results in these cases (charges during 2005-2008 as in the main data, N = 262). Vertical lines in upper panel refer to the reform's proposal, approval, and enactment (from left to right).

**Figure A6** Results Controlling for Other Policy Reforms During Follow Up Period.



Note: Results replicating main results but including dummy variables for whether each month of follow up was before or after each of the policy reforms summarized in Table 3 (coded at individual level).

**Table A1** Parameter Estimates from Separate Regressions of each of the Background and Case Characteristics on the Instrumental Variables. Denmark 2005-2008.

Variable	Time to adjudication				R <sup>2</sup>	F-test
	Intercept	Main effect	Squared	Cubic		
Female	0.185*** (0.003)	-0.008*** (0.002)	0.001*** (0.000)	-0.000*** (0.000)	<0.001	8.349
Age	0.509*** (0.095)	-0.139* (0.055)	0.004 (0.009)	0.000 (0.000)	<0.001	18.850
Ethnic minority background	0.232*** (0.004)	-0.010*** (0.002)	0.001*** (0.000)	-0.000*** (0.000)	<0.001	13.065
Has children	0.667*** (0.010)	0.008 (0.006)	-0.001 (0.001)	0.000 (0.000)	<0.001	2.210
Married	0.155*** (0.003)	-0.007*** (0.002)	0.001** (0.000)	-0.000* (0.000)	0.001	21.771
Police jurisdiction 1	0.086*** (0.002)	-0.002 (0.001)	0.000 (0.000)	-0.000 (0.000)	<0.001	2.752
- 2	0.100*** (0.003)	-0.000 (0.001)	0.000 (0.000)	-0.000 (0.000)	<0.001	4.896
- 3	0.076*** (0.002)	0.002 (0.001)	-0.000 (0.000)	0.000* (0.000)	<0.001	1.866
- 4	0.069*** (0.002)	0.001 (0.001)	0.000 (0.000)	-0.000 (0.000)	<0.001	3.080
- 5	0.080*** (0.002)	0.001 (0.001)	-0.000 (0.000)	0.000* (0.000)	<0.001	3.330
- 6	0.091*** (0.003)	0.003* (0.001)	-0.000 (0.000)	0.000 (0.000)	<0.001	2.617
- 7	0.067*** (0.002)	0.000 (0.001)	-0.000 (0.000)	0.000 (0.000)	<0.001	2.556
- 8	0.067*** (0.002)	0.001 (0.001)	-0.000 (0.000)	0.000* (0.000)	<0.001	3.505
- 9	0.079*** (0.002)	-0.002 (0.001)	0.000 (0.000)	-0.000 (0.000)	<0.001	1.224
- 10	0.093*** (0.002)	-0.002 (0.001)	0.000 (0.000)	-0.000 (0.000)	<0.001	1.152
- 11	0.186*** (0.003)	-0.003 (0.002)	0.000 (0.000)	-0.000 (0.000)	<0.001	13.824
- 12	0.007*** (0.001)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	<0.001	2.218
Elementary school	0.767*** (0.004)	0.002 (0.002)	-0.000 (0.000)	0.000 (0.000)	<0.001	5.779
Upper sec. education	0.037*** (0.002)	0.000 (0.001)	0.000 (0.000)	-0.000 (0.000)	<0.001	3.725
Vocational training	0.153*** (0.003)	0.000 (0.002)	-0.000 (0.000)	0.000 (0.000)	<0.001	4.760
Tertiary education	0.043*** (0.002)	-0.002* (0.001)	0.000 (0.000)	-0.000 (0.000)	<0.001	2.338
Years of education	0.124***	-0.059**	0.007*	-0.000	<0.001	5.041

	(0.036)	(0.020)	(0.003)	(0.000)		
Missing years of education	0.142***	0.005**	-0.001	0.000	<0.001	4.470
	(0.003)	(0.002)	(0.000)	(0.000)		
Employed	0.366***	0.005*	0.001	-0.000*	0.004	158.003
	(0.004)	(0.002)	(0.000)	(0.000)		
Unemployed	0.081***	-0.002	-0.000	0.000	0.001	38.338
	(0.002)	(0.001)	(0.000)	(0.000)		
Outside labor force	0.554***	-0.003	-0.001	0.000*	0.002	77.433
	(0.004)	(0.002)	(0.000)	(0.000)		
Post-tax income below lower quartile	0.226***	0.001	-0.000	0.000	<0.001	0.062
	(0.004)	(0.002)	(0.000)	(0.000)		
Post-tax income between lower and upper quartiles	0.522***	0.000	-0.000	-0.000	<0.001	0.811
	(0.004)	(0.003)	(0.000)	(0.000)		
Post-tax income above upper quartile	0.252***	-0.001	0.000	-0.000	<0.001	1.252
	(0.004)	(0.002)	(0.000)	(0.000)		
In contact w. mental healthcare system within last year	0.054***	0.001	0.000	-0.000	<0.001	9.540
	(0.002)	(0.001)	(0.000)	(0.000)		
Any prior contact w. mental healthcare system	0.147***	0.006**	-0.000	0.000	0.001	56.907
	(0.003)	(0.002)	(0.000)	(0.000)		
Age at first conviction	0.386***	-0.101*	0.006	-0.000	<0.001	15.763
	(0.080)	(0.046)	(0.007)	(0.000)		
No prior convictions	0.304***	-0.008***	0.002***	-0.000***	<0.001	11.614
	(0.004)	(0.002)	(0.000)	(0.000)		
- 1	0.151***	-0.001	0.000	-0.000	<0.001	1.384
	(0.003)	(0.002)	(0.000)	(0.000)		
- 2	0.092***	0.002	-0.000	0.000	<0.001	2.319
	(0.002)	(0.001)	(0.000)	(0.000)		
- 3	0.065***	0.002	-0.000	0.000	<0.001	1.421
	(0.002)	(0.001)	(0.000)	(0.000)		
- 4	0.053***	-0.001	0.000	-0.000	<0.001	0.740
	(0.002)	(0.001)	(0.000)	(0.000)		
- 5+	0.334***	0.006**	-0.002***	0.000***	<0.001	13.552
	(0.004)	(0.002)	(0.000)	(0.000)		
No prior incarceration	0.631***	-0.007**	0.002***	-0.000***	<0.001	13.771
	(0.004)	(0.002)	(0.000)	(0.000)		
- 1	0.156***	0.002	-0.001*	0.000*	<0.001	8.170
	(0.003)	(0.002)	(0.000)	(0.000)		
- 2	0.062***	0.001	-0.000	0.000	<0.001	1.349
	(0.002)	(0.001)	(0.000)	(0.000)		
- 3+	0.150***	0.003	-0.001**	0.000***	<0.001	9.665
	(0.003)	(0.002)	(0.000)	(0.000)		
Violent crime	0.239***	0.009***	-0.001	0.000	0.001	31.581
	(0.003)	(0.002)	(0.000)	(0.000)		
Property crime	0.657***	-0.014***	0.001**	-0.000	0.001	54.699
	(0.004)	(0.002)	(0.000)	(0.000)		
Detained pretrial	0.074***	-0.001	0.000	-0.000	<0.001	0.287
	(0.002)	(0.001)	(0.000)	(0.000)		
Case acquitted	0.285***	0.002	0.000	-0.000	<0.001	9.573

	(0.003)	(0.002)	(0.000)	(0.000)		
Fined	0.745***	-0.001	-0.001***	0.000***	0.004	225.535
	(0.003)	(0.002)	(0.000)	(0.000)		
Probation	0.154***	0.002	0.000	-0.000	0.001	30.898
	(0.003)	(0.002)	(0.000)	(0.000)		
Imprisonment	0.174***	-0.001	-0.000	0.000	<0.001	4.401
	(0.003)	(0.002)	(0.000)	(0.000)		
Sentence length	0.017	-0.032	0.002	0.000	<0.001	8.685
	(0.051)	(0.030)	(0.005)	(0.000)		

Note: Table reports parameter estimates from 49 separate regressions of the listed variables on quarter of year (main, squared, and cubic effects) of the criminal charge. Standard errors (in parentheses) are clustered at the individual level. Continuous variables are demeaned. N = 160,710.

\* p < 0.05; \*\* p < 0.01; \*\*\* p < 0.001.

**Table A2** Estimation Results from First Stage Regression of Time to Adjudication on Quarter and Year of Charge. Denmark 2005-2008.

Model	1	2	3	4
Quarter of year	-0.201***	-0.192***	-0.198***	-0.173***
	-0.042	-0.042	-0.041	-0.039
- squared	0.093***	0.093***	0.095***	0.092***
	-0.007	-0.007	-0.007	-0.006
- cubic	-0.004***	-0.004***	-0.005***	-0.005***
	0.000	0.000	0.000	0.000
Police jurisdiction 2 (reference: Police jurisdiction 1)		1.450***	1.506***	1.482***
- 3		-0.108	-0.107	-0.099
		1.047***	1.006***	0.996***
- 4		-0.111	-0.110	-0.102
		1.141***	1.099***	1.041***
- 5		-0.116	-0.115	-0.107
		1.553***	1.571***	1.455***
- 6		-0.113	-0.113	-0.104
		0.581***	0.610***	0.677***
- 7		-0.104	-0.103	-0.095
		2.299***	2.281***	2.378***
- 8		-0.124	-0.123	-0.114
		2.709***	2.710***	2.608***
- 9		-0.125	-0.124	-0.115
		1.785***	1.828***	1.866***
- 10		-0.115	-0.115	-0.105
		3.253***	3.253***	3.341***
- 11		-0.118	-0.118	-0.109
		2.643***	2.709***	2.760***
- 12		-0.096	-0.097	-0.090
		-0.838***	-0.740**	-0.699**
		-0.244	-0.246	-0.231
Female			-1.274***	-1.178***
			-0.067	-0.064
Age (centered)			0.047***	0.049***
- squared			-0.006	-0.005
			-0.005***	-0.005***
			0.000	0.000
Ethnic minority background			-0.365***	-0.173**
			-0.066	-0.062
Has children			0.060*	0.040
			-0.025	-0.024
Married			0.490***	0.403***
			-0.095	-0.087
Upper secondary education			0.122	0.127
			-0.172	-0.158
Vocational training			-0.160	-0.187
			-0.155	-0.143
Tertiary education			-0.169	-0.200
			-0.262	-0.242
Years of education			-0.003	0.006
			-0.032	-0.030
Missing years of education			-0.333	-0.110
			-0.289	-0.268
Employed			0.496***	0.423***

Unemployed			-0.061	-0.057
			0.702***	0.532***
Post-tax income below lower quartile			-0.104	-0.097
			0.652***	0.507***
Post-tax income above higher quartile			-0.079	-0.074
			0.134	0.147*
Contact w/ mental healthcare during last year			-0.074	-0.069
			-0.539***	-0.172
Ever contact w/ mental healthcare system			-0.116	-0.109
			-0.275***	-0.175*
Age at criminal onset (centered)			-0.079	-0.074
			0.025***	0.025***
1 prior conviction (reference: 0 prior convictions)			-0.006	-0.005
- 2			0.235**	0.161*
			-0.074	-0.070
- 3			0.435***	0.355***
			-0.091	-0.086
- 4			0.455***	0.393***
			-0.107	-0.101
- 5+			0.436***	0.438***
			-0.124	-0.118
1 previous incarceration (reference: 0 previous incarcerations)			-0.045	-0.035
- 2			-0.105	-0.100
			0.007	0.256***
- 3+			-0.082	-0.077
			0.458***	0.658***
Violent crime			-0.118	-0.112
			0.407***	0.617***
Property crime			-0.105	-0.099
				-6.755***
Detained pretrial				-0.086
				-1.633***
Acquittal				-0.085
				-5.538***
Probation				-0.103
				3.154***
Imprisonment				-0.062
				6.216***
Sentence length (days, centered)				-0.075
				3.235***
Intercept	6.204***	4.458***	4.818***	5.342***
	-0.065	-0.096	-0.129	-0.143
F-test of excluded instruments	1028.679	1058.118	1084.869	1157.244
R <sup>2</sup>	0.020	0.031	0.042	0.149
N	160,710	160,710	160,710	160,710

Note: Table reports parameter estimates and standard errors (in parentheses). Standard errors are clustered at the individual level. Dependent variable is time to adjudication measured in months. Continuous control variables are demeaned. F-test refers to test of excluding the three variables for *quarter of year*.

\* p < 0.05; \*\* p < 0.01; \*\*\* p < 0.001.

**Table A3** Summary of Estimation Results from Pre / Post Reform Analysis. Denmark 2005 and 2008.

	Months following initial charge				
	12	24	36	48	60
<b>Binary outcome</b>					
Post reform	-0.016*** (0.003)	-0.004 (0.003)	0.004 (0.003)	0.004 (0.003)	0.003 (0.003)
R <sup>2</sup>	0.166	0.219	0.244	0.259	0.266
N	80,287	79,211	78,297	77,294	76,422
<b>Count outcome</b>					
Post reform	-0.024* (0.010)	-0.005 (0.014)	0.041** (0.016)	0.069*** (0.017)	0.074*** (0.019)
R <sup>2</sup>	0.153	0.201	0.232	0.255	0.273
N	80,287	79,211	78,297	77,294	76,422

Note: Table reports parameter estimates (and standard errors in parentheses) from regressing the outcomes on a post reform dummy variable. All models control for post reform police jurisdiction fixed effects, the full set of control variables, and criminal case control variables.

\*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ .