

# First Imprisonment and the Age-Crime Curve

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### **ABSTRACT**

For decades criminologists have debated the connection between age and crime, along with the role criminal justice contacts such as imprisonment play for this connection. But even though a host of consequences of imprisonment have been analyzed thoroughly, we only know little about the effect of age at first imprisonment on criminal recidivism. In this paper, I address this issue and provide a causal estimate of the effect of age at first imprisonment on criminal re-convictions among young violent offenders in Denmark. I exploit a policy reform in Denmark in 1994 to obtain variation in age at first imprisonment that is plausibly exogenous to offender characteristics and criminal justice characteristics, and I use register data to follow these young offenders from age 15 to age 30. Results show that even though younger age at first imprisonment increases criminal convictions significantly in the short run, the overall shape of the Age-Crime Curve up to age 30 is similar, although not identical.

## INTRODUCTION

Does earlier first imprisonment impair young offenders' chances of achieving resocialization following release, or does it deter them from criminal recidivism? Criminologists have long debated the connection between age and crime, and the role criminal justice contacts such as imprisonment plays for this connection (e.g., Farrington, 1986; Gottfredson and Hirschi, 1990; Hirschi and Gottfredson, 1983; Hoffmann, 2010; Laub and Sampson, 2003; Liberman, Kirk, and Kim, 2014; Nieuwbeerta, Nagin, and Blokland, 2009; Warr, 1993). And, indeed, imprisonment is by now known to affect various life course outcomes and future criminal justice contacts (e.g., Western, 2006).

Largely missing from the debate, however, is knowledge on the effect of age at first imprisonment on the connection between age and crime. Existing studies compare arrestees to non-arrestees (Liberman, Kirk, and Kim, 2014), prisoners to non-prisoners (Nieuwbeerta, Nagin, and Blokland, 2009), detained juveniles to arrested but released juveniles (Aizer and Doyle, 2015), or court-processed juveniles to juveniles allocated to diversion programs (Petitclerc et al., 2013; Petrosino, Turpin-Petrosino, and Guckenburg, 2010). These studies provide important knowledge on the effects of experiencing first criminal justice contact compared to not having such an experience. But we do not know whether younger age at first imprisonment is better or worse for life course outcomes among people who all experience imprisonment. This gap in research is problematic because imprisonment is such a commonly used correctional tool (Walmsley, 2013), even for young people (Aizer and Doyle, 2015).

Providing the debaters with knowledge on the effect of age at first imprisonment on criminal recidivism is a challenge. One simply cannot compare outcomes among people who

were older at first imprisonment with people who were younger. This is because those who were younger at their first imprisonment are likely to differ in many ways from those who were older on a host of unobserved characteristics, which also matter for their risk of criminal recidivism. Offenders who are imprisoned earlier could, for example, have lower self-control and could have committed more serious offenses at a younger age.

This paper provides evidence on the effect of age at first imprisonment on criminal reconvictions through age 30 among young violent offenders in Denmark. To overcome the challenges of unobserved characteristics and obtain unbiased effect estimates, I compare young violent offenders who experienced their first imprisonment before and after a policy change in 1994 in Denmark. The policy change was aimed at promoting faster processing of criminal cases, which as a consequence provided variation in age at first imprisonment that was uncorrelated with offender characteristics and criminal justice characteristics. I rely on Danish register data, which allow me to follow young violent offenders who were charged before and after the policy change in 1994 from age 15 to age 30, and to analyze their criminal convictions by age.

Results show that younger age at first imprisonment increases the risk of criminal reconviction through age 30, even though the overall shape of the Age-Crime Curve among those who were older and younger at first imprisonment is similar (but not identical). The higher reconviction frequency among those who were younger at first imprisonment is only statistically significant at ages 20 and 21, suggesting a short term negative effect of age at first imprisonment on reconvictions. But the uniformity of results across ages 15 to 30, and the robustness in results across various checks, suggests that earlier imprisonment is, at the very least, not associated with fewer criminal convictions.

## AGE AT FIRST IMPRISONMENT AND THE AGE-CRIME CURVE

Few phenomena have received as much attention from criminologists as the connection between age and crime, and all criminologists are familiar with the Age-Crime Curve. Criminal propensities, especially among men, increase from early adolescence and up to late adolescence and peaks around the late teens, only to decrease with the coming of adulthood, a relationship that is one of the most consistent findings across studies even from different countries (Farrington, 1986; Hirschi and Gottfredson, 1983; Loeber and Farrington, 2014).

Despite the broadly acknowledged relationship between age and crime, the explanation of this relationship has been the topic of heated theoretical debate (e.g., Gottfredson and Hirschi, 1990; Hirschi and Gottfredson, 1983; Moffitt, 1993; Laub and Sampson, 2003; Warr, 1993). Gottfredson and Hirschi (1990) ascribes the shape of the Age-Crime Curve to the age distribution of self-control, as self-control improves with age due to hormonal development, socialization, and increased costs associated with losing self-control and committing crimes. Moffitt (1993) ascribes the shape of the Age-Crime Curve to the existence of two distinct types of offenders, life-course-persistent offenders—a small share of offenders, but accountable for a large share of crimes throughout their life course—and adolescence-limited offenders, the vast majority of offenders, accountable for comparatively little crime committed only during adolescence. Stacking the comparatively small number of crimes committed during adolescence by the large number of adolescence-limited offenders on top of the large number of crimes committed throughout the life course by the small number of life-course-persistent offenders, gives the Age-Crime Curve its shape. Laub and Sampson (2003) ascribes the shape of the Age-

Crime Curve to the age distribution of informal social control derived from social institutions, as for example the social institution of marriage causes desistance from crime but is also highly correlated with age. And Warr (1993) ascribes the shape of the Age-Crime Curve to the age distribution of peer influence.

Missing from the theoretical accounts of the connection between age and crime, however, are considerations on the consequences of age at first imprisonment for the Age-Crime Curve, which might seem surprising given that imprisonment is so common throughout the world (Walmsley, 2013). And as Hoffmann (2010) shows that younger people are more likely to respond to stressful events with delinquent or criminal behavior—an association which decreases among young adults—stressful interferences like imprisonment might be especially consequential for young people.

Imprisonment has been shown to have dual effects. On the one hand, research has documented damaging effects of imprisonment, using both U.S. data (e.g., Western, 2006) and data from other countries (e.g., Andersen and Andersen [2014] in Denmark, Nieuwebeerta, Nagin, and Blokland [2009] in the Netherlands, Di Tella and Schargrodsky [2013] in Argentina). Thus, people who have been imprisoned tend to fare worse on life course outcomes than people who have not been imprisoned, both because of characteristics of the imprisoned people (e.g., Western, Kling, and Weiman, 2001) and because of how the surrounding society responds to the imprisonment (e.g., Pager, 2003). One might therefore expect younger age at first imprisonment to elevate the level of the Age-Crime Curve. But on the other hand, using both U.S. data (e.g., Paternoster and Piquero, 1995) and data from other countries (e.g., Drago, Galbiati, and Vertova [2009] in Italy; Maurin and Ouss [2009] in France), imprisonment has been shown to have deterrence effects. Thus, imprisoned people should be deterred from committing future crimes,

and one might therefore expect younger age at first imprisonment to lower the level of the Age-Crime Curve.

Analyzing the consequences for the Age-Crime Curve of age at first imprisonment is, however, not an easy task. One simply cannot compare offenders who were imprisoned for the first time at one specific age with offenders who were imprisoned for the first time at another age. This is because any difference in the Age-Crime Curve between these offenders might be caused by other differences in their characteristics, which are not strictly attributable to their age at first imprisonment. For example, offenders who are imprisoned at a younger age could differ from those who are imprisoned at an older age on a host of unobserved characteristics, which might very well be the main reason why these offenders become imprisoned at a younger age in the first place.

Reviewing 29 experiments which allocated arrested youth either to court processing or to diversion programs, Petrosino, Turpin-Petrosino, and Guckenburg (2010) found evidence of higher recidivism rates among court processed youths. The findings in the review suggest that negative consequences of formal court processing, such as labelling effects, outweigh the specific deterrence effects of court processing. Similar results have been found in non-experimental studies (Farrington, 1977; McAra and McVie, 2007; Petitsclerc et al., 2013), even across different countries (Huizinga et al., 2004).

The same conclusion arises from the few existing studies which aim at analyzing the consequences of first imprisonment. These studies compare offenders who experience their first imprisonment with either a matched population of non-offenders, who do not experience imprisonment, or a group of comparable offenders who were not imprisoned at that age.

Nieuwbeerta, Nagin, and Blokland (2009) shows that first imprisonment in the Netherlands is associated with an increase in crimes during the first three years following release, even though another study has found a modest incapacitation effect of first imprisonment in the same country (Wermink, Apel, Nieuwbeerta, and Blokland, 2013). Aizer and Doyle (2015) uses randomly assigned judges' preferences for sending juvenile offenders to detention, to show that juvenile incarceration has a very strong impact on future education chances and on future convictions. And although not analyzing imprisonment or incarceration, Liberman, Kirk, and Kim (2014) shows that first arrest increases the likelihood of both subsequent offending and subsequent arrests. Thus, these studies point towards damaging effects of first imprisonment (or incarceration or arrest) which elevates the level of the Age-Crime Curve.

But whereas these existing studies provide important evidence on the consequences of court proceedings or of first imprisonment compared to not having this experience, they do not directly address the question of the timing of first imprisonment. Addressing this question requires an empirical setup that credibly provides variation in age at first imprisonment—among offenders who all become imprisoned for their crimes committed at the same age—that is exogenous to characteristics of those imprisoned offenders.

This paper presents such an empirical setup which is suitable for causal inference. Specifically, I compare the Age-Crime Curve of young violent offenders who experienced their first imprisonment before and after a policy change in 1994 in Denmark. The policy change promoted faster processing of criminal cases, which provided variation in age at first imprisonment that was uncorrelated with offender characteristics and criminal justice characteristics. In what follows, I compare imprisonment in Denmark to imprisonment in the

United States, and explain the 1994 policy change in detail, to describe the context from which my results come.

## IMPRISONMENT IN DENMARK

Imprisonment in Denmark differs from imprisonment in the United States on a number of parameters. First, and most important for this paper, a special feature of the Danish criminal justice system is that being charged with a crime or being sentenced to imprisonment in Denmark does not mean that the offender is immediately placed in custody, as is the case in the United States. In Denmark—unless circumstances of the case or the offender speak against it—the offender is released into the community without bail. The offender is then summoned to court when court proceedings begin, and is then again released into the community until receiving his or her call for admission (if sentenced to imprisonment).

This special feature of the Danish criminal justice system means that whereas some offenders wait a long time from when they face criminal charges to when they are imprisoned, others wait for shorter periods—cases have different processing times. A host of case, offender, and court features might influence the case processing time, as for example some cases are more complex than others, some offenders are more cooperative than others, and some lawyers are busier than others, all features that might prolong or shorten the case process.

The difference in how imprisonment is initiated between the countries means that offenders in Denmark have better opportunities for preparing themselves for their prison stay. They have more time to take practical as well as emotional and mental precautions before imprisonment compared to in the United States, where incarceration is often a sharp disruption

of everyday life. In this way offenders might be better prepared for being imprisoned in Denmark, and might therefore better resist the negative influences of incarceration. But one might also argue that awaiting imprisonment is just an additional part of punishment in Denmark. If so, punishment in Denmark could be made up of two distinct periods, one when the offender awaits imprisonment, and one when the offender is imprisoned. I return to this consideration in the Discussion section.

Second, Denmark and the United States differ in the sense that few developed democracies differ more in their imprisonment rates than these two countries. The United States imprisonment rate of 716 per 100,000 in 2013 is almost ten times the Danish rate of 73 per 100,000 (Walmsley, 2013). The low Danish imprisonment rate reflects on Danish sentences being very short: Around two out of three sentences are shorter than three months, and the shortest prison sentences in Denmark are only seven days (Danish Prison and Probation Service, 2014). This difference between the countries means that imprisonment in the United States is far more common and far lengthier than in Denmark, and the social stigma associated with imprisonment might be even bigger differ between the countries

Third, there is a fundamental difference between the punishment ideologies of the countries. Harsh prison conditions, which are viewed as part of the punishment in the United States, are avoided in Denmark (DeMichele, 2014). In Denmark, prison life is structured so as to resemble life outside the prison—for example, inmates are expected to work while incarcerated, they get paid a (low) wage for this work, they shop for groceries in a grocery store inside the prison, and they prepare their own foods in small kitchens at their ward. This difference between the countries could imply two things. On the one hand, the imprisonment experience in the United States might place harder burdens on offenders caused, for example, by anxiety

associated with the harsh prison conditions. In Denmark, where prison life to a greater extent resembles everyday life in the community, prison life might prepare the offender better for reintegration, simply because the contrast between imprisonment and everyday life is not so stark. But on the other hand, the deterrence effects of harsher prison conditions could produce lower recidivism rates in the United States than in Denmark, where imprisonment is not as punitive.

Fourth, in Denmark there is no distinction between jail incarceration and imprisonment, as there is in the United States where sentences of more than a year would typically send the offender to prison rather than jail. There are local arrest houses in Denmark, but these are primarily used for pre-trial incarceration. Danish prisons are divided into low security (open) prisons and high security (closed) prisons. In open prisons, doors are not locked, the prison is surrounded by a fence rather than a wall, the staff to inmate ratio is just below 1:1, and inmates are allowed to pursue pro-social activities, like taking up education or employment, outside the prison. In closed prisons, doors are locked, the prison is surrounded by walls, the staff to inmate ratio is just above 1:1, and inmates are not allowed to leave the prison premises unless a board strictly permits it. The inmate capacity in Danish prisons is 1,358 in open prisons and 909 in closed prisons (Danish Prison and Probation Service, 2014).

The offenders in the analyses for this paper are all imprisoned in open prisons, which is the prison type that differs most from imprisonment in the United States. This paper does not, however, analyze the consequences of length of imprisonment or of prison conditions, only the consequences of age at first imprisonment, and in this sense results from young violent offenders in Denmark might still be informative regarding young violent offenders in the United States.

As is clear from this comparison of imprisonment in the United States and Denmark, there are important differences between the countries. But as no study has yet estimated the effect of age at first imprisonment on the Age-Crime Curve, this caveat is perhaps not the most central one. The offenders in my sample are all sentenced the same way and imprisoned within the same context, and only their age at first imprisonment varies. If there are important differences between the Age-Crime Curve among the offenders in my sample from a criminal justice context that must be viewed as comparatively lenient, it might be all the more important to take age at first imprisonment into account when debating age and crime in other contexts.

#### THE 1994 POLICY CHANGE

The change in age at first imprisonment that I exploit for causal inference was a consequence of a new law aimed at cases of violence in Denmark, enacted on May 18 1994 (The Danish Administration of Justice Act, Law no. 366). The reform was aimed at increasing sentence lengths in cases of violence for repeat offenders and in cases of assault in which the victim was at elevated risk because of his or her job type—and it was aimed at decreasing case processing times in all cases of violence. But already on January 11 1994 the rules regarding case processing that were enacted in May were in effect, because on this date the Chief Public Prosecutor in Denmark set forth new objectives regarding the maximum processing time in cases of violence—these objectives were just not formalized by law until May 1994 (The Danish Administration of Justice Act, Law no. 366, Section 3.1.4). In what follows, 'policy change' refers to the Chief Public Prosecutor's new objectives regarding case processing time from January 11 1994, and not to the actual policy reform in May the same year.

The feature of the 1994 policy change that I use for causal inference is that sentence length in cases of common assault for first time offenders was not targeted by the change—unless their victims were at elevated risk because of their job, but as I show below, average sentence length did not change among first time violent offenders. But among these first time offenders, their case processing time was indeed targeted by the policy change, and those charged after the policy change should have faster processing than those charged before.

Faster case processing means younger age at imprisonment. Thus, having identical sentence lengths, one consequence of the policy change in January 1994 was that two comparable groups of first time violent offenders exist: A control group charged before the change who had slower case processing and who were older at first imprisonment. And a treatment group charged after the change who had faster case processing and therefore, on average, were younger at first imprisonment. And, indeed, the mean age at first imprisonment among the relevant offenders was around six months younger among those charged after the change, which I show in what follows.

Three objectives were stated by the Chief Public Prosecutor on January 11 1994. First, no more than 30 days should pass from charge date to the court proceedings. Second, court proceedings should not exceed seven days. And third, no more than 30 days should pass from when the convicted and sentenced offender received his call for imprisonment to when he was actually imprisoned.

Figure 1 shows the decrease in 1994 in the annual median time (days) from charge to imprisonment in common assault cases for male offenders in Denmark during 1991-2000. The



time offenders of common assault waited from charge to imprisonment were almost halved from 1993 to 1994: From 13 to 7 months.

[Insert Figure 1 about here]

The new objectives in 1994 were aimed at all crimes of violence (except for cases with additional charges, such as robbery), including for example aggravated assault. But Figure 1 only shows figures for common assault, as only these cases were effectively affected by the reform (results available upon request). Also, similar objectives were handed down by the Chief Public Prosecutor in 2004 for charges on rape and in 2008 for charges on weapon possession. But these later changes had little impact on actual case processing, and both changes targeted only few offenders (results available upon request), which is why these later reforms are not used in this paper.

The 1994 change in case processing was implemented by the Danish Prison and Probation Service, who established a total of 87 temporary prison cells in three of the then existing 13 low security (open) prisons during 1994 and 1995. These temporary cells were removed again at the end of 1995, and the total prisoner capacity of the Danish open prisons went up from 1,356 in 1993 to 1,410 and 1,470 in 1994 and 1995, and then back to 1,347 in 1996, including planned capacity adjustments. In Figure 1 the removal of the temporary cells in late 1995 might explain the small increase in case processing times during 1996. But the median case processing time never reached its high level from before 1994 again. This suggests that the entire criminal justice system became more effective in cases of common assault because of the policy change, and that the temporary prison cells merely made this adjustment easier.

Figure 1 also shows other characteristics of the Danish criminal justice system in cases of common assault during 1991-2000. These characteristics are the use of pre-trial incarceration, average sentence length, and the police clearance rate in these cases. Importantly, these other characteristics were remarkably stable over the period (pre-trial incarceration was used in around 5 percent of cases; the sentence length was around 30 days throughout the period; and the police clearance rate was close to 80 percent). This stability in other criminal justice system characteristics indicates that the change in processing time, which occurred during 1994 because of the policy change, is plausibly exogenous to other criminal justice system characteristics, and might thus be suitable for causal inference.

## METHOD AND DATA

I use the 1994 policy change to measure the effect of age at first imprisonment on criminal convictions by regressing each person's criminal conviction frequency on their treatment status (which means by whether they were charged before or after the policy change in January 1994) and on their individual control variables, by age. As I argue that the younger average age at first imprisonment is exogenous to offender characteristics and criminal justice characteristics, this approach measures the mean and unbiased effect of altering age at first imprisonment in the way it was done in Denmark in 1994, by age. All models rely on OLS estimation.

DATA

I use Danish register data available from Statistics Denmark. In Denmark, all residents have a unique personal identification number, similar to a Social Security Number in the United States, which is linkable across many registers and across years. In this way the data are a full population panel with precisely measured variables on many key aspects of Danish residents' lives, such as their marital status, contacts with the welfare system, tax forms, and all criminal justice contacts and incarcerations (see Lyngstad and Skardhamar, 2011).

These register data are well suited for the purpose of my study, as they provide access to an individual-level full population panel of all criminal justice contacts in Denmark. This feature of the data means that I may follow a sample of offenders from age 15 to age 30, and add to the sample their criminal convictions, by age, just as I may add dates of charges, convictions, imprisonments, and releases—and I may add a range of covariates, all gathered from administrative records.

Sample

From the register on criminal charges I obtain individual identifiers on all 15-20 year old people whose charge with common assault during two years before and after the policy change in early 1994 led to their first imprisonment, and who did not face any additional charges on the case in question (as those with additional charges were not targeted by the policy change). This gross sample consists of 694 offenders. I then add information on their cases—for example sentence length, charge date, conviction date, imprisonment date and release date—as well as on their prior and later criminal charges.

Additional steps prepare the sample for analysis, and the number of cases deleted during each step is seen from Table 1. I drop a few cases with extraordinarily long prison sentences, as these sentence lengths seem implausible and are likely to be caused by registration errors; I exclude cases with errors in one or more date variables, which primarily means missing release date; I trim the 1 percent cases per year that has the longest case processing times, as some cases seem to last for exceptionally long time (which, again, is most likely caused by registration errors); I keep only males because there are only very few females in the raw sample; and I keep only cases where I am able to identify the same person across all relevant registers. The final analytic sample has 511 persons.

[Insert Table 1 about here]

Variables

The 1994 policy change affected persons charged with common assault after January 11 1994 and the treatment variable is then defined as facing charges of common assault within the first two years following this date (1994 and 1995), which led to the person's first imprisonment. This happened to 262 persons. The control group consists of young violent offenders charged with common assault within two years before the change (1992 and 1993), and who were about to experience their first imprisonment, which happened to 249 persons. Although the treatment variable simply measures whether or not a person was charged before or after the policy change, individuals in the control group (who were charged before the change) could still experience their first imprisonment after the policy change, as the change was only aimed at charges from January 11 1994 onward.

To obtain background variables and enhance the credibility of the 1994 policy change as providing exogenous variation in age at first imprisonment I, as mentioned, link this sample of young violent offenders in 1992-1995 to a host of other registries. I add their date of birth, which I use to construct the age variables described above, and I add previous arrests, low income (defined as belonging to the lower income quartile among 15-20 year old men in Denmark, by year), years of education, ethnic minority background, and whether they have children or not, just as their release date provides their actual length of imprisonment as a supplement to sentence length. These control variables are all measured during the year before their common assault charge in 1992-1995.

As outcome variable I follow each person in the sample from age 15 to age 30 in the registry, and add their criminal convictions, by age. In this way I measure criminal convictions both prior to and following each person's first imprisonment. This allows me to analyze their crime level by age, for all ages 15 thru 30 years, and to see whether the crime level of those charged before the policy change differs from the crime level of those charged after.

**RESULTS**

**DESCRIPTIVE STATISTICS AND EXOGENEITY OF THE REFORM**

Figure 2 shows the criminal conviction frequency among those charged before and after the 1994 policy change, by age. The shape of the Age-Crime Curve in the sample is as expected, and the conviction frequencies increase up to around age 19, and then decreases.

[Insert Figure 2 about here]

But importantly, even though the criminal conviction frequencies of the treated (charged after the policy change) and controls (charged before the policy change) do not differ up until their average age at charge (18.6 years), these frequencies start to gap from age 19. The conviction frequency among those charged after the change is higher at all ages than among those charged before. The difference is especially strong at ages 19, 20, 21, and 22, which is just after these young offenders experienced their first imprisonment. This descriptive finding indicates that there could be a negative effect of the 1994 policy change, and that offenders who were younger at first imprisonment might have a higher risk of criminal conviction, at least during the first years following first imprisonment.

Table 2 shows descriptive statistics of the sample, by treatment status. The first row of the table shows the mean number of criminal convictions per person from age 15 to age 30. The average cumulative number of convictions among the treated (3.6) is higher than among the controls (3.0), yet the difference is only marginally significant ( $p = 0.053$ ).<sup>1</sup> Still, this marginally significant difference in convictions between the groups is substantial, and corresponds to around 149 additional convictions in the group charged after the policy change, simply because offenders in this group were imprisoned at a younger age.

Table 2 also breaks down the time period these offenders waited from their common assault charge to their first imprisonment, and shows the distribution of their age at first imprisonment. On average, the control group waited close to 13 months from their common assault charge to their first imprisonment, and the treatment group waited almost 7 months, which is a highly significant difference. The period may be broken into time from charge to conviction (7.5 months for controls, 4.2 for the treated), and from conviction to imprisonment

(5.4 months for controls, close to 2.7 months for the treated), and both of these differences are also statistically significant.

The consequence of this decrease in case processing time was, as already mentioned, that those charged after the 1994 policy change (treated), and who therefore waited for a shorter period, were on average 19.2 years old at their first imprisonment, whereas those charged before the policy change (controls) were on average 19.7 years old. Table 2 also shows the distribution of age at first imprisonment among treated and controls, and it is seen that higher shares of the treated than controls have experienced their first imprisonment at younger ages. For example, 43 percent of the treated were imprisoned before age 19, while the same number is 25 percent among the controls.

[Insert Table 2 about here]

Table 2 further shows that in addition to being plausibly exogenous to other characteristics of the Danish criminal justice system, which was indicated in Figure 1, the 1994 policy change might also be unrelated to background characteristics of the offenders in my sample. Out of nine control variables, only one differs at any conventional significance level between the treatment and control group. And as both the treatment and control group has around 250 observations, this lack of difference between the groups is little likely to be caused by sample size.

The one variable that does differ between the two groups is the average time offenders waited from their offense date to when they faced charges. The controls waited a little more than half a month, whereas the treated waited a little less than half a month. This difference is problematic because the 1994 policy change was only supposed to affect the period from charge

to imprisonment, not from crime to charge. The difference makes sense, however, because the obligation to process criminal cases faster, which was the aim of the 1994 policy change, might have caused a more general increase in police effectiveness among cases after the change—and as the 1994 policy change appears to be exogenous to all other covariates in Table 2, quicker charges might just have been yet another tool applied to adhere to the 1994 objectives.

The control variables show that on average the offenders were charged at age 18.6 years. They were on average sentenced to one month of imprisonment, and on average they also served one month of imprisonment. Very few had children, and few were of ethnic minority background. Their education length corresponded on average to elementary schooling, and around 10 percent of the sample had low earnings the year before they were charged. Last, around one in seven were previously arrested.

MAIN RESULTS

Figure 3 shows the main results, and plots the estimated difference in criminal convictions, by age, between those charged after the policy change (the treated who on average had younger age at first imprisonment) and those charged before the change (controls who on average had older age at first imprisonment). All models are estimated using OLS, and control for the covariates that were just described.

[Insert Figure 3 about here]

The estimated effects and their 95 percent confidence intervals confirm the descriptive findings. Specifically, there are no differences up until age 18, where the groups do not differ on

any parameters, and the sign of the treatment coefficient bounces around 0 at this young age. But from age 19 and up to age 30 the estimated effect is consistently positive (except for ages 23 and 24, where estimates are just below zero), suggesting that those who were charged after the policy change, and who were on average younger at first imprisonment, had a higher average conviction frequency, by age. The difference is only statistically significant at ages 20 and 21, which means in the relatively short run following first imprisonment, and we may thus only rest assure that the conviction risk among the treatment group is higher at these ages. The estimated differences between the groups at these ages are 0.11 and 0.09, and recalculated as number of convictions, these effects correspond to 29 additional crimes at age 20 and 24 at age 21 among those charged after the reform, a substantial increase in criminality.

## ROBOSTNESS CHECKS

The above mentioned results rely on the existence of a historical control group to measure the effect of age at first imprisonment on criminal convictions, as I compare criminal convictions among those charged after January 11 1994 to criminal convictions among those charged before this date. Even though in my specific setup, those from the historical control group waited longer for their first imprisonment and might therefore be imprisoned at the same time as the treatment group, the reliance on a historical control group is always accompanied by questions of whether this particular group is valid for comparison.

I investigate this question using three robustness checks. First, I investigate whether results are sensitive to different macro conditions surrounding the treated and controls. Second, I investigate whether the estimated policy change effect indeed corresponds to the timing of the

policy change, or whether there are signs of announcement effects which could contaminate results. And third, I investigate whether similar effects appear among samples of offenders who were charged during the same period, but who were not targeted by the policy change, because they were charged with other crimes than common assault.

## Macro Trends

I investigate whether different macro trends surrounding the imprisonment of the treated and controls could be driving my main results. I do this by normalizing the dependent variable (number of convictions per person, by age) using the mean number of convictions among 15-30 year old men who are not in the main sample, by year (results not shown but available on request). I find that this normalization does not alter my conclusions. Thus, different macro trends for treated and controls are not driving the main result that younger age at first imprisonment has damaging (short-term) effects.

## Reform Timing

I investigate whether the timing of the policy effect indeed corresponds to the timing of the policy change in January 1994. I do this by rerunning analyses for all samples of offenders corresponding to the analytic sample, with the treatment date artificially moved in one month steps from January 1993 to January 1995 (and I thus select monthly pseudo policy changes +/- 12 months from the true policy change date). This robustness check is to see whether there are

signs of announcement effects of the policy change, which would invalidate my claim of causal inference.

[Insert Figure 4 about here]

Figure 4 plots the p-values of the estimated pseudo effects, by pseudo policy change month. The obvious U-shape of the plotted p-values around the p-value of the true effect estimate indicates that the timing of the effect indeed corresponds to the timing of the reform. But one month before and after the true policy change in January 1994, however, the p-values deviate from the U-shape. This deviation from the U-shape could imply some sort of treatment migration within a narrow window around the true policy change date, which could problematize causal inference. For example, it is possible that some prosecutors postponed the pressing of charges in easier cases from late 1993 to early 1994 in order to get a head start in adhering to the Chief Public Prosecutor's objectives regarding faster case processing from January 1994.

One conservative approach to investigating the consequences of such treatment migration is to rerun the main analyses without any cases from the months just before and after the policy change. When excluding all cases from December 1993 to February 1994, I get results that are similar to the main results, although with a few exceptions (results not shown but available on request). First, the effect estimate at age 20 becomes larger ( $0.131, p < 0.01$ ). Second, the effect estimate at age 21 becomes smaller and loses statistical significance ( $0.077, p = 0.072$ ). And third, all standard errors become larger, which is not surprising given the lower number of observations. Yet despite these exceptions, the overall conclusion remains unaltered, and results indicate that younger age at first imprisonment has damaging effects, at least in the short run, although the overall shape of the Age-Crime Curve is similar among treated and controls. Thus,

announcement effects, or other effects associated with the timing of the policy change, do not seem to be driving the main results.

#### Other Crime Types

To enhance that the estimated effects do not co-occur with other structural breaks in the Danish criminal justice system, I re-run all analyses choosing other crime types than the one affected by the policy change (common assault). I specifically investigate whether there are pseudo effects on robbery and burglary, and I find no signs of reform effects for these crime types (results not shown but available on request). Thus, the main results do not appear to be driven by other structural breaks in the criminal justice system in Denmark.

### DISCUSSION

Younger age at first imprisonment does not alter the overall shape of the Age-Crime Curve up to age 30 among violent male offenders, yet it does promote statistically significant and substantially important damaging effects within the first few years following first imprisonment. This is the main conclusion from this study on the effect of age at first imprisonment on criminal convictions through age 30.

Even though the connection between age and crime has been so vividly debated among criminologists, theoretical accounts on the consequences of age at first imprisonment for this connection are absent, and empirical knowledge on the topic has been sparse. The few studies

that do direct attention to the consequences of first criminal justice contacts all compare those who experience criminal justice contact to a comparison group who does not.

This study is the first to compare the connection between age and crime among an offender group that were younger at first imprisonment with an offender group who were older, all of whom were eventually imprisoned for crimes committed at the same age. And, importantly, the credibility of the comparison is secured by exploiting a policy change in 1994 that shortened case processing in common assault cases in Denmark, which as a side-effect produced younger age at first imprisonment that was unrelated to offender characteristics and criminal justice characteristics. Hence, results have causal interpretation, and even though younger age at first imprisonment does not alter the overall shape of the Age-Crime Curve, it does increase criminal reconvictions in the short run.

One seemingly straight forward implication of these results is that if younger age at first imprisonment is damaging, at least in the short run, policy makers should try and postpone first imprisonment for violent offenders. But this implication soon becomes complicated, as questions of criminal justice quickly arise: For example, where should violent offenders go while they grow old enough to become imprisoned? And just how old should these violent offenders be before imprisonment is optimal? Would it perhaps be more beneficial to allow for noncustodial alternatives to imprisonment at an even younger age, rather than postponing imprisonment? Answering these questions is no easy task, and is not possible within the limits of this paper, yet they are important questions for figuring out the practical implications of the findings in this study.

Not supported by the results in this paper is the claim from deterrence theory that earlier imprisonment should deter offenders from future crimes. Results indeed point in the opposite direction, and lend more support to the paradigm of damaging effects of imprisonment. But in defense of deterrence theory, however, one could argue that the lengthy case processing in common assault cases in Denmark should be viewed as part of the punishment. In this light, in addition to causing younger age at first imprisonment, the 1994 policy change effectively decreased the total punishment for common assault in Denmark. If the policy change is interpreted in this way, results do lend support to deterrence theory for as long as one is willing to believe that the decrease in deterrence that comes from a decrease in total punishment outweighs the increased deterrence that comes from earlier punishment—a statement that does not seem unrealistic.

Imprisonment is consequential for the offender because experiences which occur during imprisonment put physical as well as emotional constraints on offenders. But results in this paper come from comparing offenders who were, on average, imprisoned for the same period and under identical prison conditions, and only the timing of first imprisonment differed between those charged before and after the policy change. The findings then imply that in addition to the physical and emotional consequences of imprisonment the period which offenders wait from facing criminal charges to when they are imprisoned has short-term criminological consequences. These consequences could be psychosocial, and the period from criminal charge to imprisonment could be characterized by elevated stress levels because the offender has to explain the situation to family members and arrange for a period of absence from education or employment. When offenders have less time for these preparations, stress levels might increase even more, which could explain the short-term increase in criminal convictions among those who



were younger at first imprisonment. This explanation is supported by research which shows that adolescents are worse than young adults at handling elevated stress levels (Hoffmann, 2010).

This paper has shown that younger age at first imprisonment does not alter the overall shape of the Age-Crime Curve, and the connection between age and crime continues to be a criminological puzzle that pertains to all countries. But in Denmark, younger age at first imprisonment does increase criminal convictions in the short run, as this paper has shown. And, indeed, knowing what are the effects of age at first imprisonment in one of the countries that together with the United States bookshelves the criminal justice systems of developed democracies should be of interest to scholars of criminal justice as well as to scholars of mass imprisonment in general—especially since no previous study has estimated the effect of age at first imprisonment on the Age-Crime Curve.

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STATUE CITED

The Danish Administration of Justice Act, Law no. 366. Lov om ændring af straffeloven, retsplejeloven og offererstatningsloven (Bekæmpelse af vold) [Law on changes to the Penal Law, the Procedure Code, and the Law on Victim Compensation (Controlling Violence)]. Enacted on May 18 1994.

Table 1. Sample Selection

Data Step	<i>N</i>
Raw Sample	694
Drop long sentences	-4
Drop date errors	-129
Trim 1 percent	-4
Drop females	-12
Drop missing in linked registers	-34
Analytic Sample	511

Table 2. Descriptive Statistics

	Before Change		After Change		
	M	(SD)	M	(SD)	
Criminal convictions, ages 15-30	3.024	(2.818)	3.561	(3.405)	
Months charge—imprisonment	12.908	(5.696)	6.889	(3.772)	***
Months charge—conviction	7.508	(4.407)	4.171	(2.798)	***
Months conviction—imprisonment	5.400	(3.757)	2.718	(2.801)	***
Age at first imprisonment	19.701	(0.945)	19.189	(0.921)	***
—share younger than 16	0.000	(0.000)	0.000	(0.000)	
—share younger than 17	0.004	(0.063)	0.008	(0.087)	
—share younger than 18	0.024	(0.154)	0.088	(0.284)	**
—share younger than 19	0.249	(0.433)	0.431	(0.496)	***
—share younger than 20	0.643	(0.480)	0.760	(0.428)	**
—share younger than 21	0.908	(0.290)	0.989	(0.107)	***
—share younger than 22	0.992	(0.089)	1.000	(0.000)	
—share younger than 23	1.000	(0.000)	1.000	(0.000)	
Months offense—charge	0.582	(0.964)	0.399	(0.783)	*
Age at charge	18.640	(0.851)	18.623	(0.910)	
Sentence length (days)	31.181	(14.278)	29.893	(13.407)	
Imprisonment length (days)	31.779	(23.316)	30.202	(22.588)	
Parent	0.016	(0.126)	0.011	(0.107)	
Ethnic minority background	0.056	(0.231)	0.073	(0.260)	
Years of education	9.313	(0.694)	9.317	(0.724)	
Low prior income <sup>a</sup>	0.096	(0.296)	0.111	(0.314)	
Previously arrested	0.145	(0.352)	0.141	(0.349)	
N	249		262		

<sup>a</sup>Low prior income is defined as income below the lower income quartile in the full population of 15-20 year old men the year before the common assault charge.

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

Figure 1

Median Number of Days from Charge to Imprisonment in Common Assault Cases in Denmark, 1990-2000

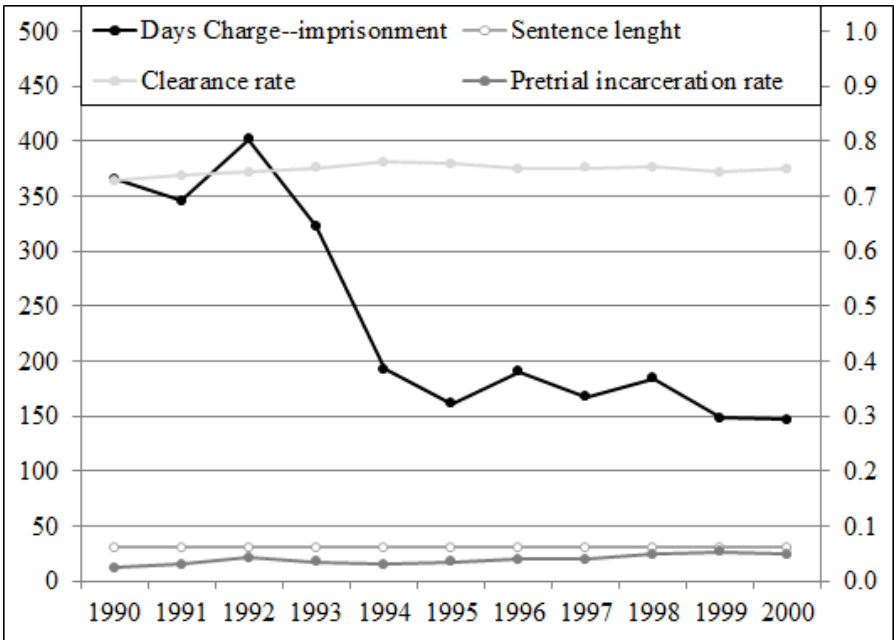
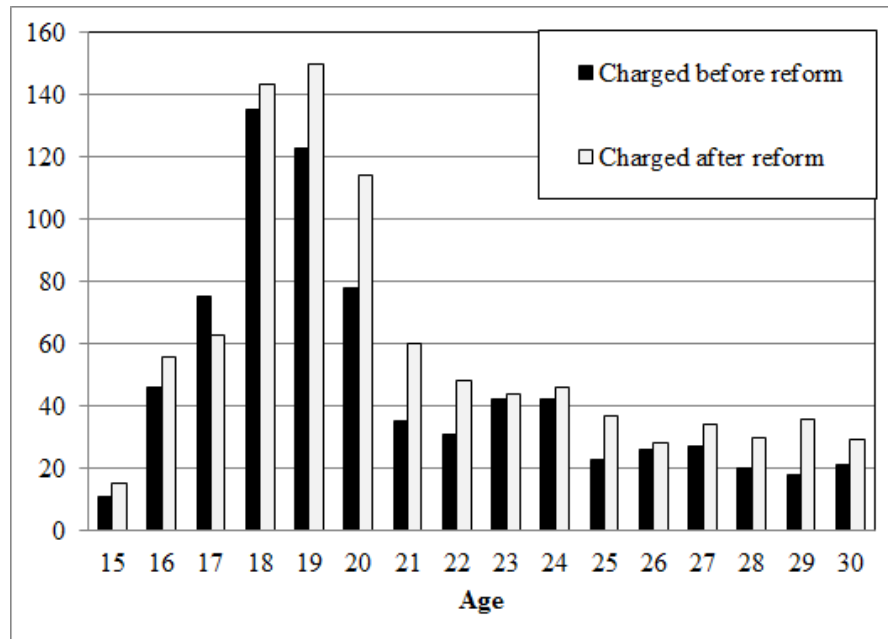


Figure 2

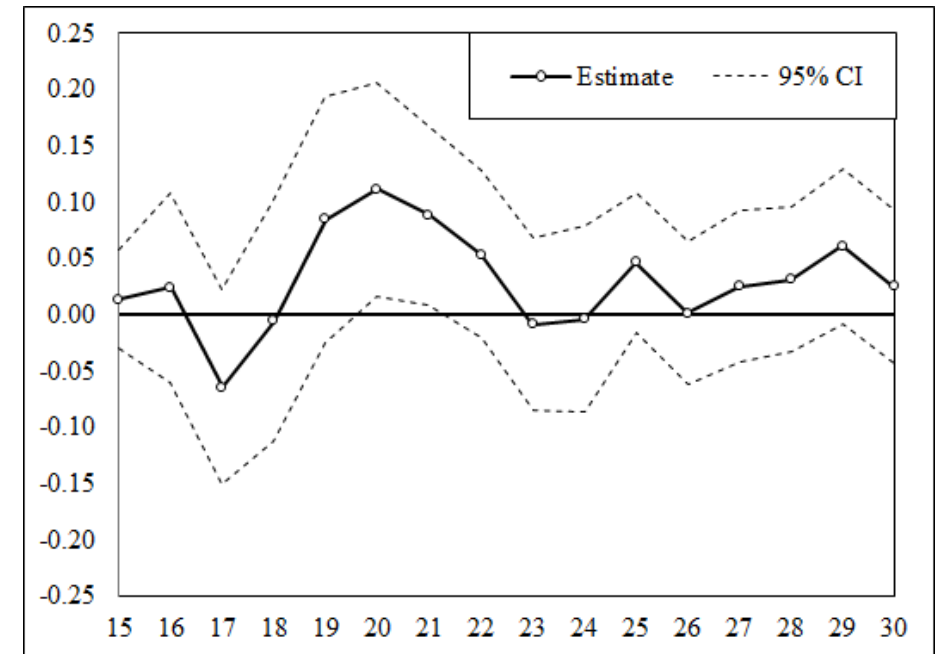
Criminal Conviction Frequency by Age among Common Assault Offenders Charged Before and After the 1994 Policy Change



Note: All persons in the sample were 18.6 years when facing the common assault charge that selects them into the sample. But the average age at first imprisonment among those charged before and after the policy change differs: 19.7 and 19.2 years.

Figure 3

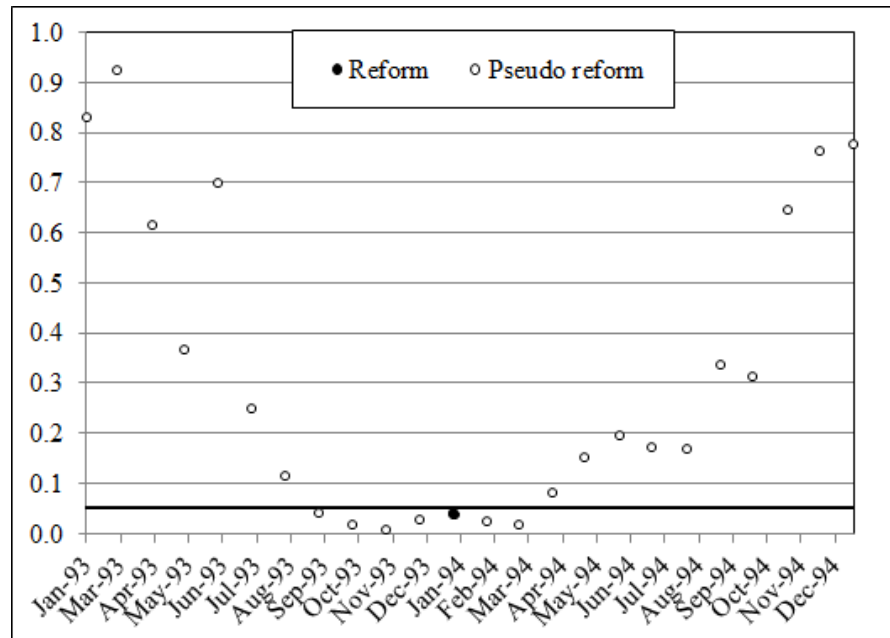
Policy Effect Estimates and 95 percent Confidence Intervals, by Age



Note: Effects are estimated using the standard OLS model, by age, and controlled for age at charge, days from offense to charge, sentence length, length of imprisonment, whether the person has children, ethnic minority background, years of education, low earnings, and previous arrest.

Figure 4

P-values for Monthly Pseudo Policy Effect Estimates at Age 20, 1993-1995



Note: Pseudo effects are estimated using the standard OLS model at age 20, among samples corresponding to the main sample, but selected around the pseudo policy dates. The vertical line marks the p-value of the actual policy change effect in January of 1994. Estimates are controlled for age at charge, days from offense to charge, sentence length, length of imprisonment, whether the person has children, ethnic minority background, years of education, low earnings, and previous arrest.

## NOTES

<sup>1</sup> Regressing the total number of criminal convictions per person on age dummies, by treatment status, and testing the joint significance of all age dummies for the treatment group—using the F-test statistic—shows no significant difference between the treated and controls on the average number of convictions throughout the entire age window.

