Nordic Journal of Criminology | Scandinavian University Press

RESEARCH PUBLICATION

Volume 26, No. 1, pp. 1–21
ISSN online: 2578-9821
DOI: https://doi.org/10.18261/njc.26.1.6
Received: 1 July 2024
Accepted: 31 October 2024

Thirty years of prison alternatives in Denmark: Policy efficiency and inequality before the law

Lars Højsgaard Andersen
Research Professor, ROCKWOOL Foundation Research Unit
lha@rff.dk
https://orcid.org/0000-0003-2357-1896

Abstract

The use of non-custodial alternatives to imprisonment has grown dramatically over the past 30 years in the Nordic region. But few studies have analyzed the implications of this development. This paper focuses on the case of community service and home confinement under electronic monitoring in Denmark, programs that have been expanded 12 times since 1992. Results from population registry data analyses show that the risk of serving the equivalent of a prison sentence of up to a year in prison is currently below 50% because of these alternatives. Results further indicate persistent inequality in access to non-custodial alternatives along income, education, and ethnic background gradients. However, they also show that differences in recidivism rates across these gradients largely exceed differences that arise from the endogenous selection of people to alternatives within these gradients. Both in general and across the mentioned gradients, reform estimates suggest little to no causal impact of non-custodial alternatives on rates of criminal recidivism. Results thus portray the challenge of striking a balance between the principle of equality before the law and policy efficiency when implementing "smart sentences".

Keywords
non-custodial alternatives, imprisonment, policy evaluation, recidivism, smart sentencing

Introduction

The Nordic countries have long been proponents of the use of non-custodial alternatives to imprisonment (e.g., Lähteenmäki, 2022). These alternatives have been implemented to ease pressure on prisons and improve resocialization by better balancing punitive and rehabilitative features of punishment (Andersen & Andersen, 2014; Øster & Rokkan, 2018). Expectations seem to have been met. In the context of this study, some convicted persons in Denmark are allowed to serve their sentence at home under electronic monitoring (EM) or by contributing to the local community through community service (CS). These non-custodial alternatives to imprisonment produce better outcomes for sentenced persons (e.g., Larsen, 2017) and their families (e.g., Anker, 2023). Some studies even indicate recidivism reducing benefits in Denmark (e.g., Clausen, 2007), in Sweden (Marklund & Holmberg, 2009), to some degree in Finland (Muiluvuori, 2001), and outside the Nordic region (e.g., Di Tella & Schargrodsky, 2013), although the results from several systematic reviews are more uncertain about the effectiveness of EM (e.g., Renzema & Mayo-Wilson, 2005).

When deciding whether a convicted person should serve a prison sentence or an alternative sentence, courts or the relevant prison services focus on *risk reduction*, favoring those convicted people with a higher chance of completing a non-custodial alternative without breach of the conditions. This approach makes sense, as the legitimacy of knowingly removing the incapacitation effect of incarceration (by serving a sentence in the community) could lose public support if, for example, convicted people committed new crimes while serving sentences in the community. And it makes sense as taking the life circumstances of the person into account in sentencing is explicitly stated in the Danish Penal Code (§ 80).[1]

From a public policy perspective, the risk reduction strategy seems successful. Rates of criminal recidivism are lower among those who serve alternative sentences than among those released from prison (Danish Prison and Probation Service, 2016). In addition, the costs of running non-custodial alternatives are generally lower than the costs of running prisons, and the private costs are lower when measured from important domains of life such as the labor market and the family (e.g., Andersen & Andersen, 2014). Indeed, the successful matching of sentencing alternatives to those convicted persons least likely to breach their conditions is known as "smart sentencing" in the research literature (e.g., Byrne et al., 1992).

Few studies have focused on the broader implications for criminal justice and for the (Nordic) criminal justice systems that now rely heavily on non-custodial alternatives, however (but see Lähteenmäki, 2022). This oversight is important, as there may well be an inverse relationship between policy efficiency ("smartness") and social equality, owing to the non-random matching of programs to people that may systematically favor certain groups in society. This point is not a new one. The inherent potential for social discrimination was indeed one of the arguments for abandoning individualized sentencing in the 1970s and 1980s, and the issue was debated again when introducing alternative sanctions in the 1990s (Lappi-Seppälä, 1993). But after 30 years of policy expansions, it is timely to study their compound implications for these important principles.

In this study, I use population data from Denmark from 1990 to 2020 to show the growth in non-custodial alternatives to imprisonment over 30 years, and I analyze the implications of this growth from efficiency and inequality perspectives. Regarding policy efficiency, I focus on the implications of expanding the use of non-custodial alternatives for the overall criminal recidivism to which society is exposed. I do this descriptively and by relying on data from before and after several policy expansions (to allow for causal inference). Regarding inequality, I focus on how the non-random selection of people into non-custodial alternatives has favored specific social groups over others, as defined by prior income, education level, and ethnic background.

The Nordic countries have a tradition of both social equality and policy pragmatism (Lappi-Seppälä, 2007). Results from this study show that simultaneously meeting efficiency and equality goals may not always be possible, at least in Denmark. Weighing highest risk reduction against equality before the law thus becomes an ideological and political trade-off of which policy makers ought to be aware.

Background

The costs of imprisonment

Prisons are costly to run. For example, Denmark and Norway spend about DKK 3.5 billion and NOK 5 billion, respectively, per year on prisons.

Extensive research also shows that imprisonment can be costly for those experiencing it. A key mechanism behind such costs relates to the sociological concept of stigma (Goffman, 1963), whereby the surrounding society infers personal features and characteristics from the information that a person has been imprisoned, essentially making a moral character judgment. Such stigma processes associated with imprisonment have been shown to lead to lower post-release employment chances (Braman, 2004). Imprisoned persons may also lose human capital and skills while in prison, effectively decreasing their value in the labor market (e.g., Western et al., 2001). Two further mechanisms focus on the impact of the confinement experience. The first relates to the conditions under which a prisoner serves their sentence, as often-harsh conditions may directly harm the individual during imprisonment, such as when placement in restrictive housing impacts post-release employment chances and criminal recidivism (Wildeman & Andersen, 2020). The second relates to the fact that incarceration almost inevitably ties a person to other criminals who may exert negative influences (Damm & Gorinas, 2020). Many of these same mechanisms extend to family members, with the separation experienced by families during the period of imprisonment having the potential to lead to additional strain (e.g., Eddy & Poehlmann-Tynan, 2019).

Other studies have found more mixed and even positive effects of imprisonment. Bhuller et al. (2020), for example, found that, in Norway, while imprisonment has negative effects on the labor market outcomes of people who had had labor market attachment prior to imprisonment, it improves labor market outcomes for those with little to no labor market attachment prior to imprisonment. InDenmark, Landersø (2015) found that increasing sentence lengths for young violent offenders led to better labor market outcomes post-release.

Non-custodial alternatives to imprisonment and their use in Denmark

The "facility costs" of non-custodial alternatives are lower than those of prisons. In Denmark, for example, the daily cost of keeping a convicted person in EM is about half that of keeping a person in open prison (DKK 546 in EM vs. DKK 1,103 in open prison in 2012 [the The Judicial Committee to the Danish Parliament, 2012]). The use of non-custodial alternatives to imprisonment thus results in substantial cost savings.

The focus of this paper is CS and EM in Denmark. These alternative sanctions are implemented differentially in the Nordic countries, which could invite a comparative study of the impact of contextual factors and political priorities for the logics laid out (see Lappi-Seppälä, 2019). However, laying out the logics and analyzing them empirically in one context is an important first step in this direction.

CS consists of a specified number of hours of unpaid work that a convicted person must undertake to benefit the community. This might be in a private firm, a public institution, or in an NGO, and may include tasks such as service at a local library. CS is carried out during the person's out-of-work hours, thereby allowing the person to retain normal employment while serving their sentence. There is no fixed translation between the number of hours of a CS sentence and the length of the prison alternative; however, guidelines in Denmark are that 30 hours of CS worked within four months functions as an alternative to 7–14 days imprisonment, 40 hours of CS within four months as alternative to 20–30 days imprisonment, and so on.[2] CS always includes probationary supervision throughout the CS period.

Denmark's CS scheme has gradually expanded over the past 30 years through policy reforms, as summarized in the first column of Table 1. Following a trial period, CS

was formally implemented as a sentence in 1992 as a non-custodial alternative to prison sentences of up to 12 months in length. The 1992 policy stressed, however, that CS should not be used extensively for crime types that could challenge the legitimacy of the penal system, with crime types such as violence, robbery, sex crimes, and drugs crimes generally not considered for CS. Today, the scheme includes many more crime types and CS functions as an alternative to longer sentences. Symptomatic of the earlier discussion about how to balance the use of non-custodial alternatives with the public's sense of justice, the most recent CS reform (2020) limits the use of CS for people convicted of more serious or repeat violent crimes, thus countering the general trend of expanding the use of this sanction alternative.

Table 1. Overview of reforms to the CS and EM schemes in Denmark, 1990–2020

Date	Community service (CS)	Home confinement under electronic monitoring (EM)
April 1 1992	After a trial period, CS is formally approved as sanction alternative for prison sentences up to 12 months. Limited use in cases of violence, robbery, sex crimes, and drug crimes.	
May 1 1997	Suspended sentences with CS may now also include prison sentences of max. 3 months.	
July 1 2000	Expansion to include more severe traffic offenders and eligibility evaluation a requirement for property offenders. Minimum hours of CS increased from 30 to 40 hours.	
March 1 2005[a]		Introduces EM for traffic offenses. Sentence lengths max. 3 months.
September 1 2005	Max. blood alcohol content level for drunk drivers allowed in CS increases to 2.00.	
April 21 2006		Expands to include all crime types[b] but only for people younger than 25 years. Sentence lengths max. 3 months.
July 1 2008		Removes age criterion.
July 1 2010		Sentence length requirement changes to max. 5 months.
July 1 2013		Sentence length requirement changes to max. 6 months. Also abolishes the employment criterion for sentences shorter than 30 days. Parole under EM also possible now.
2012[c]	Max. available hours of CS increase from 240 to 300 hours, making CS alternative to prison sentences of length 1.5 to 2 years too.	
May 1 2015	Increases the use of CS for people convicted of violence. Increases sentence lengths for new crimes committed during the CS period.	
January 1 2020	Limits the use of CS in cases of violence (esp. repeat cases and cases with more	

(*Continued*)

Table 1. (*Continued*)

Date	Community service (CS)	Home confinement under electronic monitoring (EM)
	serious bodily or psychological harm, and in aggravated assault cases).	

[a]The law was formally effective from 1 July 2005 but extended to sentences handed down after March 1 2005.
[b]In general, people sentenced to less than two weeks of imprisonment for the unlawful possession of weapons or explosives cannot be considered for EM.
[c]The 2012 CS reform is irrelevant to the analyses presented in this paper as the focus of the paper is on sentences of max. one year.

In relation to the CS scheme in Denmark, and especially the reform in 2000, it is important to also mention the abolishment of very short-term prison sentences ("*hæfte*") in 2001 (law passed in May 2000). *Hæfte* was a prison sentence served under milder conditions of confinement and shielded from the general prison community, applicable only to sentences shorter than six months. Following the abolishment, some people who would earlier have been sentenced to *hæfte* would likely be sentenced to "standard" imprisonment, but a sizeable portion of the group was, after the policy change, more likely to be sentenced to alternatives, either fines or – because of the 2000 reform to CS – to CS.

EM in Denmark is an alternative way to serve a prison sentence. It consists of an electronic device (fitted around the ankle) that sends the sentenced person's location to the Prison and Probation Service. EM comes with a daily schedule that specifies the person's allowed location at certain hours – such as at a designated workplace during office hours. If the person is not where they are supposed to be, the prison service may revoke the EM and send the person to prison to serve the remaining sentence. EM includes restrictions on alcohol and drug use, with random spot tests for such substances, and the requirement to complete a crime reduction course at the Prison and Probation Service. Additional requirements include having a permanent address (e.g., not be living in a shelter for the homeless) and residing where the electronic monitoring is practically possible. The person must also be employed, participate in some form of active labor market program, or be enrolled in an education program.[3] The person's employer and adult members of their household must formally accept the EM.

Denmark's EM scheme has gradually expanded since it was first introduced in May 2005 (see the second column of Table 1). As with CS, the EM scheme was cautiously introduced. At first, it only included traffic offenders but then expanded to young people and only for comparatively short sentences. It has since expanded to include several crime types and sentences of up to six months. Today, as in Norway, Sweden, and Finland, people can also be released from prison early on parole under EM (in 2022,–745 of 2,320 early releases on parole in Denmark were through this type of "backdoor" EM [Danish Prison and Probation Service, 2023]).

Studies from Denmark that rely on policy-induced sentencing variation to overcome the non-random selection of convicted persons into non-custodial alternatives document positive or null effects of CS and EM. Andersen (2015) found higher earnings and lower dependence on social assistance among people serving in CS compared to those who served prison sentences. And lower rates of criminal reconviction among people originally convicted of violent crimes, some traffic offences, and misdemeanors, although the study did not find overall effects of CS on reconvictions. Andersen and Andersen (2014) found lower dependence on social assistance among young people allowed to serve an EM sentence with the 2006 reform, but no effect on people older than 25 years affected by the

2008 reform to EM. In addition, Larsen (2017) found improved education rates among young people affected by the 2006 reform. Last, Jørgensen (2011) compared all convicted persons who received information about their potential eligibility for EM during the first year since the 2006 policy expansion to a pre-reform group with similar sentence length, age, and crime type. Results implied lower criminal reconviction rates among young people sentenced to EM because of the reform.[4]

Other studies exist for Denmark. Klement (2015) compared the reconviction rates of people sentenced to imprisonment or CS in a sample that were all found formally eligible for CS. Results showed lower reconviction rates among those released from CS compared to those released from prison. Klement (2015) had access to all variables on which CS eligibility was formally evaluated, which is a core strength of that study. But the conclusion in the study still hinges on one of two strong assumptions: 1) that judges are on average randomly sentencing to CS or imprisonment conditional on eligibility, or 2) that judges differ in their tendency to sentence to CS and that cases are randomly assigned to judges. Clausen (2007) applied regression models (survival models and logistic regression models) to compare people sentenced to CS for violence or driving under influence in 2000/2001 (i.e., after the 2000 reform) to pre-reform comparison groups. Results imply slower and lower reconviction rates among specific subgroups of the CS groups, such as the young and the unemployed, but no overall reconviction-reducing effects. But even if effects had been stronger, the applied regression models only identify the effect of CS if *all* selection into CS occurs on variables that are observed in the data and controlled for in the models; unobserved differences between persons in the treatment and control groups are not accounted for.

The positive effects extend beyond the sanctioned person to their family members in Denmark. Relative to imprisonment, CS lowers rates of out-of-home placement (Andersen & Wildeman, 2014), lowers youth crime conviction risks (sons only, Wildeman & Andersen, 2017), and improves educational outcomes (Anker, 2023) for children. EM has also been shown to lead to lower risk of romantic partnership dissolution (Fallesen & Andersen, 2017).

Policy efficiency vs. equality before the law

Authorities decide who to allow to serve a sentence in the community and tend to favor people with higher likelihood of being able to meet the requirements of these sentence types (in the case of Denmark, Norway, and Sweden, this is the prison service; in Finland, it is the presiding judge [Lähteenmäki, 2022]). But because risk profiles are related to socioeconomic variables, such focus on policy efficiency may unintendedly clash with another fundamental aim of Nordic societies: social equality and the principle of equality before the law. According to the United Nations' Declaration of Human Rights,[5] people convicted of similar crimes should face similar sanctions from the state. As laws dictate that factors other than the punishable act, such as attitudes towards the court and victim, for example, should also weigh in on setting the punishment (e.g., Danish Penal Code § 80), this declaration only pertains to fully comparable cases, persons, and circumstances, however. It is thus not a violation of human rights if two people sentenced for similar crimes differ in their imposed sanction because they differ in other aspects, such as attitudes and efforts to obtain resocialization. But the non-random selection of convicted persons for non-custodial alternatives still may, consequently, promote inequality. In the case of EM, a judge in Denmark may sentence two people to imprisonment for an identical length of time for committing identical crimes. However, because the prison

and probation service administratively decide the sanction, that sanction may differ. One person may avoid going to prison and another may be forced to go to prison because they are unemployed or cannot afford the type of housing in which electronic monitoring is possible – or because they differ in attitudes or the like. The choice is both pragmatic (EM must be technically possible and it would seem unreasonable to allow EM for someone with a high expected risk of breaching the EM conditions) and in compliance with the law (emphasizing personal circumstances), and one therefore might debate whether tensions relate as much to criminal policy expediency, crime prevention, and other values in sentencing on the one hand, as to proportionality on the other. Indeed, there are likely many interpretations of the tension, but in the context of this study, a core consequence of the choice may be increased inequality and the state actively providing better life chances for people with more resources available to them, something which seems incompatible with the Nordic societies' focus on social equality.

Labor market attachment and living arrangements are just two of the many factors that discriminate with regard to CS and EM eligibility. Other features of social inequality correlate with the factors that impact the form of sanction administered, implying that the risk reduction paradigm may promote social inequality before the law. In this paper, I focus on prior income, education level, and ethnic minority background as examples of such features.

Data and methods

Danish registry data

This study uses registry data from Denmark. The data, collected from several record-keeping agencies and gathered and hosted by Statistics Denmark, are made available for research in pseudo-anonymized form under strict data security precautions as defined in the Law of Statistics Denmark[6] and in compliance with the EU General Data Protection Regulation (GDPR)[7] . Unique individual identifiers allow for the merging of individual-level information across several registries and across time, making registry data a uniquely strong and flexible data source for research (Lyngstad & Skardhamar, 2011; for Denmark specifically, Andersen, 2018).

Criminal justice agencies, such as the national police and the Prison and Probation Service, provide data on all contacts with the criminal justice system. From these data, I selected all cases for the period 1990–2020 that resulted in either a prison sentence shorter than one year or CS. This choice is based on convictions, i.e., imprisonment or CS sentences passed down by a judge. For prison sentences, the Prison and Probation Service then evaluated eligibility for EM (see below).

As the outcome variable, I focused on criminal recidivism, defined as any new violation of the penal code that leads to conviction. I followed individuals in the data for three years from the conviction that selected them into the sample and recorded the date of their first recidivism crime. Starting the recidivism "clock" at the original conviction date ensured that potentially differential processing times for prison, CS, and EM sentences could not affect the follow up period. For recidivism crimes, I focused on the dates that crimes were committed rather than conviction dates, as the lag between the two could be considerable and vary over time. I constructed a binary variable that took the value of 1 if a person violated the penal code (for which the person was later convicted) within three years of the original conviction, and 0 otherwise.

Background variables included gender, age, and non-Western ethnic minority background from the population registry. Gender was recorded biological gender at birth. Age measured age at the conviction that selected a person into the data. I limited the data to only focus on those aged 18–60 at the time of conviction (over the 30-year data period, 11,240 observations fell outside this window, corresponding to 3% of the data). Non-Western ethnic minority background measured whether one or both parents had immigrated to Denmark from a non-Western country. From the education registry, I marked whether a person had mandatory schooling (10 years) as their highest educational attainment. From the income registry, I added prior income, which relies on all third-party reported income to the tax authorities and thus covers all pre-tax income from legal work and from public assistance (such as social assistance). In the analyses, I divided individuals in the data into groups defined by an income level above or below the median income during the year before the conviction that selected them into the data. Regarding the conviction that selected the person into the data, I added sentence length and crime type (violent crime, property crime, traffic offence). I also added information on criminal history, namely number of convictions for violating the penal code within the previous five years and whether the person had experienced incarceration prior to the case in question.

Methods

As the first step in the analysis, I provided an overview of the trend in the relevant sentences and ways of serving these sentences over the 30-year data period. I plotted the counts of new CS sentences and prison sentences that led either to prison admission or to EM. I also showed the proportion of the total number of relevant sentences that served in CS or EM.

In the second step, I focused on criminal recidivism. Here, I showed the cumulative recidivism rate for each of the comparison groups, i.e., people who served their sentence in prison, CS, or EM, accumulated over the first three years post-conviction. These results represent a combination of any effect of sentence type and the consequence of judges and the prison service endogenously selecting people to serve in CS or EM. To address endogeneity, I explored the process of policy evaluation, exploiting changes in the likelihood of a convicted person serving in CS or EM arising from the timing of their conviction relative to policy shocks. If a policy shock was statistically effective, it would produce a pre-reform universe and a post-reform universe that differed only regarding policy change. For example, prior to the reform on 1 July 2008, only people younger than 25 years of age could be considered for EM. The reform removed the age criterion, meaning that convicted persons of any age could be considered for EM (contingent on meeting other requirements which did not change with the reform). Following the 2008 reform, those older than 25 years of age thus experienced an abrupt change in their chance of serving an EM sentence rather than a prison sentence, unrelated to the crimes they committed or personal factors. The endeavor of analyzing the effects of the reform on criminal recidivism is straightforward: the exogeneity of the reform allows for a simple comparison of rates of criminal recidivism among people in the pre- and post-reform groups. Any difference in their average outcomes can be attributable to the difference in the use of CS or EM in these groups.

Because there can be noticeable time trends in crime and other societal factors, however, comparing people further away from the reform may be susceptible to bias. I therefore evaluated reform effects using a regression discontinuity design that effectively

compared persons sentenced immediately prior to and immediately after the policy shocks while taking the trend in criminal recidivism into account (e.g., Thistlethwaite & Campbell, 1960).[8] The estimation model is:

$$y_{ic} = \alpha + \beta * Reform_{ic} + f(t_{ic})'\delta + X'_{ic}\gamma + \varepsilon_{ic} \quad (1)$$

where y is the dummy variable for individual i's criminal reconviction within three years from conviction c, the dependent variable. $f(t)$ expresses the functional form of the association between the running variable (time of conviction c relative to the relevant reform) and the outcome, which has different linear slopes on either side of the reforms. The inclusion of $f(t)$ implies that the general trends in criminal recidivism among those sentenced before and after the reforms are taken into account, effectively ensuring that time trends in the outcome are captured and the effect of the reforms is measured by comparing only those sentenced *just* before and *just* after the reform implementations. X holds control variables; α is the average recidivism rate of persons convicted *just* prior to the reforms and β is the average change for similar people convicted *just* after the reforms (the parameter of special interest, i.e., the average effect of the combined reforms); ε is the model's error term, assumed to be i.i.d; and standard errors are clustered by the running variable, t_{ic}.

The advantage of the model in Equation 1 is that it effectively identifies and measures any change in criminal recidivism rates that occur because the timing of the policy reforms allowed a higher proportion of otherwise comparable people sentenced right after the reforms to serve in the alternative than among those sentenced just before the reforms. By focusing narrowly on the shifts – or discontinuities – that arose because of the timing of the reforms, this strategy is strong for this subgroup. The strategy is, however, *not* informative about general trends in criminal recidivism over the 30-year period in the data, as there are way too many endogenous trends happening simultaneously over such a period to meaningfully identify the effects. Put directly, whereas Equation 1 effectively measures the criminal recidivism effects of the reforms *upon their implementations* (i.e., providing discreet jumps in the share serving in non-custodial alternatives), it says nothing about the criminal recidivism effects of *having* non-custodial alternatives to imprisonment.

In practice, and to avoid showing ten separate reform results, I pooled data from prior to and after the reforms to obtain one pre-reform and one post-reform universe. I did so because the focus here is not to show whether each separate reform affected outcomes but rather the overall implications of expanding the availability of alternatives the way it has been done. The latter is the most interesting parameter from a public policy perspective as it represents a weighted average of the compound effects of expanding the use of alternatives to imprisonment.

In the third step of the analysis, I focused on the distribution of CS and EM and their potential effects across the key background variables of prior income, education, and non-Western ethnic minority background. I first showed the proportion of these groups that are allowed to serve CS or EM sentences by repeating the descriptive analysis in the first analytical step using the stratifying variables. Unlike the first analytical step, these results were controlled for crime type, sentence length, and criminal history to account for the likelihood that these key variables would be unevenly distributed across persons defined by prior income, educational attainment, and ethnic background. Failure to control for these key variables would make it unclear whether differences in sanction

types across groups arose from the uneven distribution of crime or from inequality before the law.

I next turned to the criminal recidivism outcome and repeated both the descriptive and the policy evaluation exercises from the second analytical step, split by the stratifying variables. This exercise served two purposes: First, it showed the unequal impact of the reforms. Second, it explored whether the policy-induced variation in the likelihood of serving a sentence in CS or EM translated differentially into criminal recidivism across the groups.[9]

Results

Trends in the use of CS and EM in Denmark, 1990–2020

Figure 1 shows trends in the use of imprisonment, CS, and EM in Denmark over the 1990–2020 period. Panel (a) reports frequencies and shows that the total annual sentences of maximum one year in length (or comparable, for CS), decreased over the period, from about 14,000 in 1990 to below 8,000 in 2020. The number of sentences served in prison decreased even more. Around 2000, the decrease likely reflects the abolishment of *hæfte*, as previously discussed, but the general decrease over the period also comes from generally increasing sentence lengths; the number of sentences longer than a year *increased* from around 700 in the early 1990s to around 1,000 in the late 2010s (own calculations not shown).

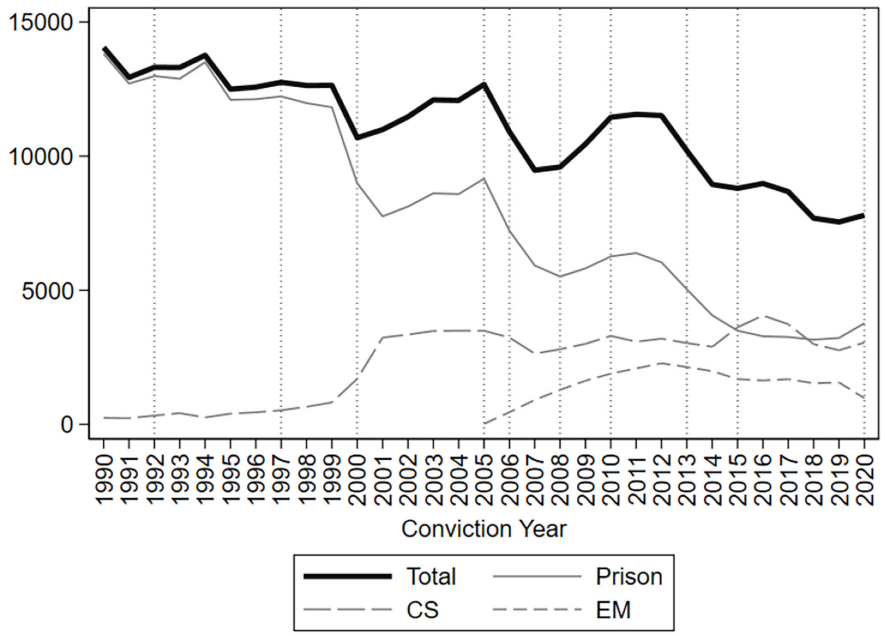
In the early 1990s, almost all sentences were served in prison. However, toward the end of the data period this number had dropped to approximately 3,800. The opposite trend was observed for CS. EM became a sanction option only in 2005 but quickly expanded to reach almost 2,300 sentences served in 2012. Since then, the number has declined slightly, with the final drop in 2020, likely owing to the limited use of EM during the COVID-19 pandemic.[10]

Panel (b) shows the proportion of combined total sentences (of maximum length one year, corresponding to the solid black line in panel (a)) each year served in either CS or EM ("non-custodial alternative"). The vertical dashed lines in the figure mark years with the policy reforms listed in Table 1. Results indicate a strong upward trend in the use of non-custodial alternatives over the period, with notable increases following some reforms that increased the use of non-custodial alternatives. At the start of the period, almost all sentences were served in prisons. Toward the end of the period, the chance of not going to prison to serve what could have been up to a full year exceeded 50%, and in 2016/2017 it neared two-thirds of all sentences.

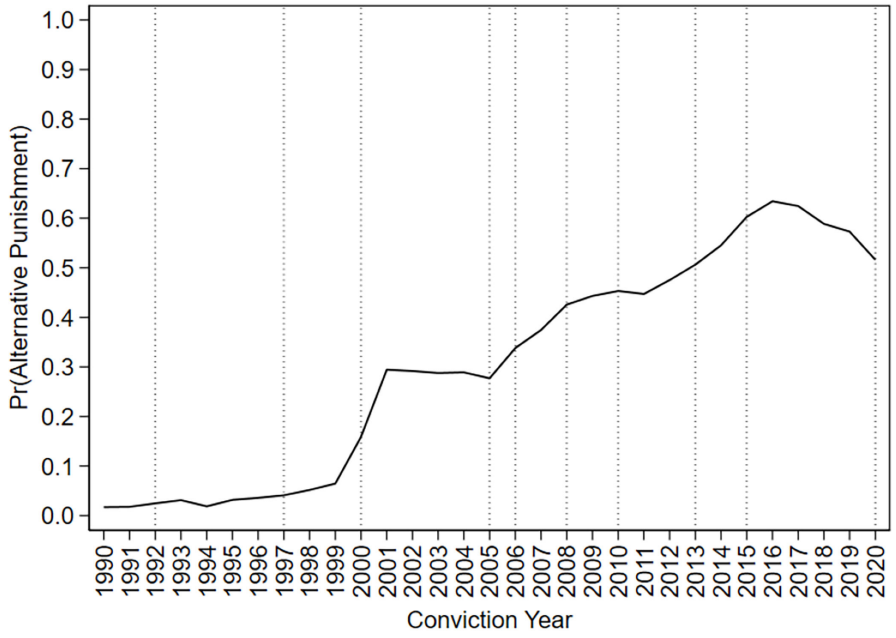
Criminal recidivism

Figure 2 shows the cumulative proportion of people with at least one new crime that led to conviction, split by whether the person served their original sentence in prison, in CS, or in EM. Just over half (51.8%) of those who served a prison sentence were re-convicted for crimes committed within three years. For those who served alternative sentences, the cumulative rates were 27.4% for EM and 21.5% for CS.

These large differences in recidivism rates across sanction types are not indicative of the causal effects of sanction type on behavior. To explore causality in more detail, Figure 3 focuses on the pooled data from prior to and after the reforms over the 1990–2020 period. Panel (a) shows the proportion who served non-custodial alternative sentences (CS or EM). Prior to reforms, the rate was 25.6% (Table A5); after reforms, the average rate increased



(a) Number of sentences



(b) Proportion of sentences

Figure 1. Development in sentences corresponding to maximum one year's imprisonment, total number and split by imprisonment and non-custodial alternatives (CS and EM), and as yearly proportions

Notes. Dashed vertical lines denote years with policy reforms as listed in Table 1. "EM" reflects prison sentences where the person was allowed to serve at home under EM, and "Prison" excludes these sentences.

discontinuously by 8.2 percentage points (totaling 33.8%). This jump in the proportion of non-custodial alternatives can be used to measure the effect of these alternatives on criminal recidivism without being confounded by (in the data unobserved) mechanisms that select

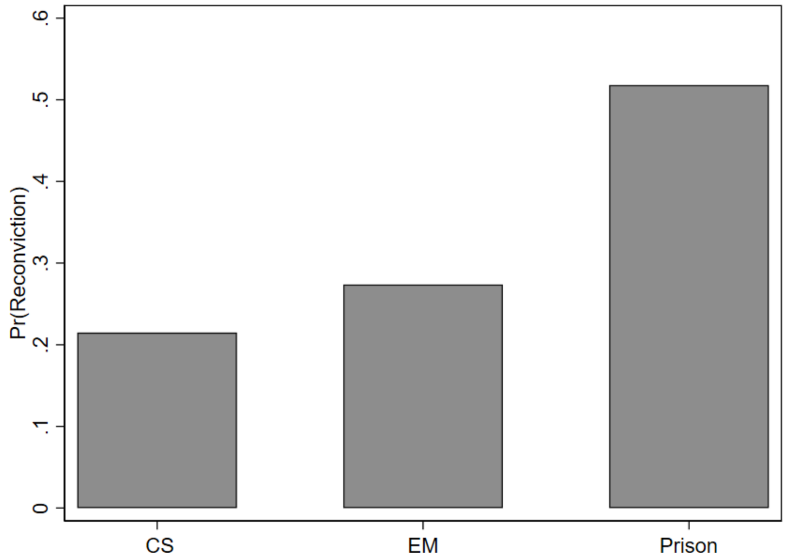
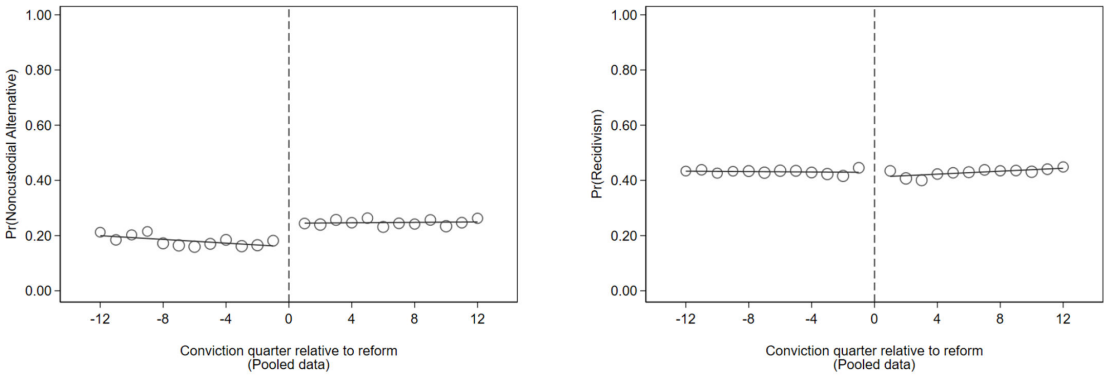


Figure 2. Cumulative proportion with recidivism up to three years after original conviction, by sentence type

Notes: Data pooled across all sentences (shorter than one year or equivalent for CS), 1990–2020.



(a) Proportion of sentences served in non-custodial alternative (b) Proportion convicted for new crime committed within three years

Figure 3. Proportion of relevant sentences served in non-custodial alternative (CS or EM) and three-year recidivism rate by conviction quarter before and after policy reforms

Notes. Data pooled across all reforms listed in Table 1 (except the reform in 2012 as this affected only sentences longer than 1.5 years).

people into the alternatives. Panel (b) shows the criminal recidivism rates of the same people. If the reforms credibly (and collectively) altered rates of criminal recidivism upon their implementation, this should be visible as a discontinuous jump in recidivism rates at 0. In addition, the size of the jump in recidivism (panel (b)) relative to the size of the jump in sanctioning alternatives (panel (a)) would represent the causal effect on those allowed to serve alternative sentences (a local average treatment effect [LATE] estimate). However, the results in panel (b) show no jump at 0, suggesting no joint effect of the reforms on criminal recidivism. Estimates from Equation 1, shown in Table A5, confirm this null finding. Importantly, all estimates are negative, implying at the very least that the introduction and expansion of alternatives to imprisonment over the 30-year period has not led to increased

rates of criminal recidivism; in fact, the results imply recidivism-reducing but imprecisely estimated effects.

Inequality before the law

Figure 4 shows the proportion of relevant non-custodial alternative sentences with the data split by key stratifying variables (prior income, educational attainment, and non-Western ethnic minority background) and controlled for crime type, sentence length, and criminal history.[11] It is evident that people with higher prior income, with higher educational attainment, and from majority ethnic backgrounds accumulated higher chances of not serving a prison sentence for (on average) similar crime types, sentence lengths, and criminal histories.

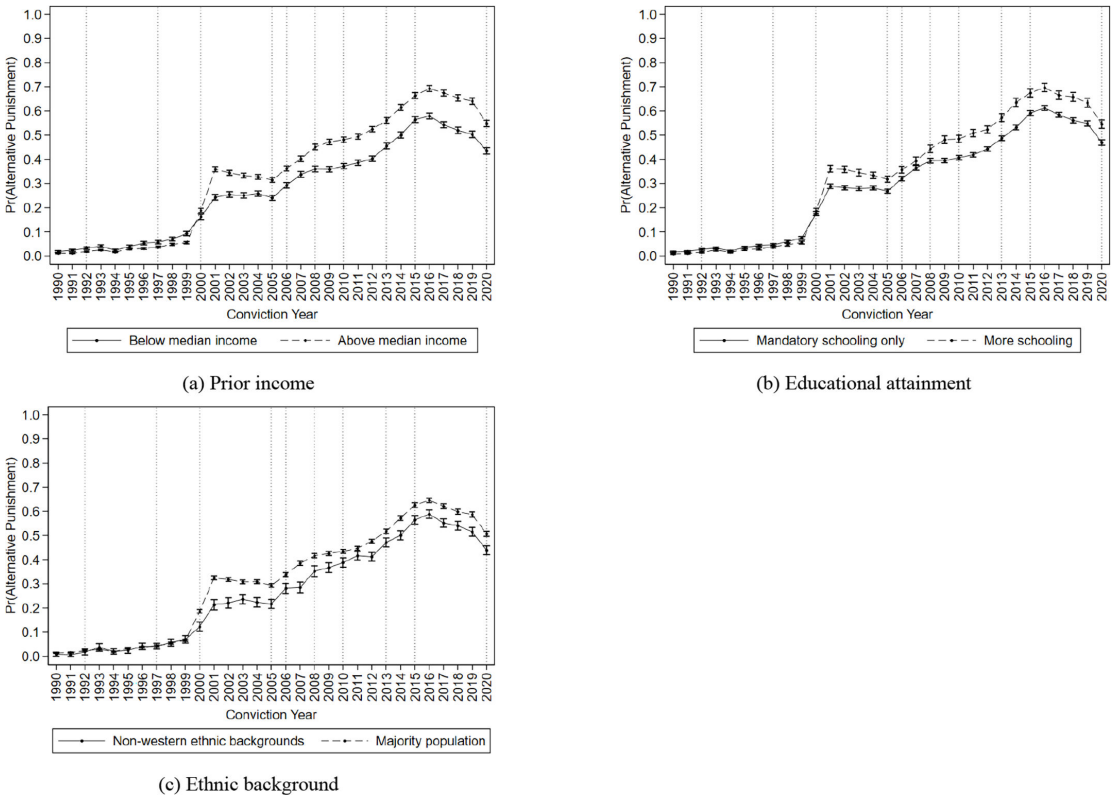
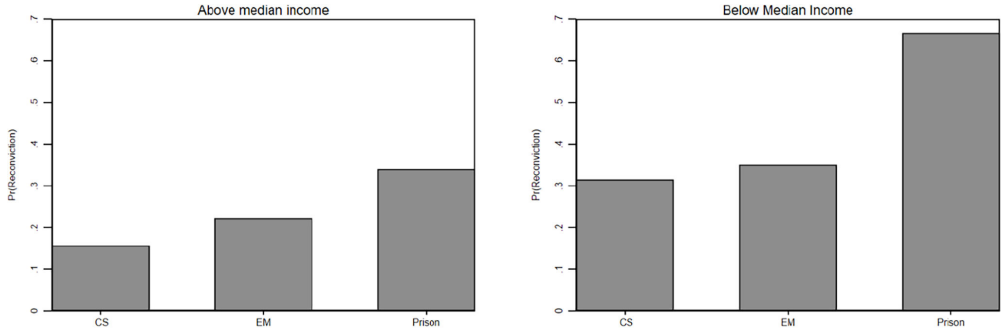


Figure 4. Proportion of sentences corresponding to maximum one year's imprisonment that was served in CS or EM, by stratifying variables

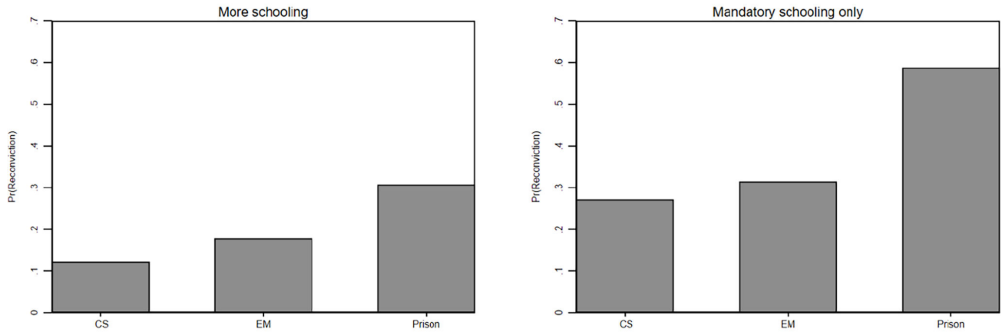
Notes. Dashed vertical lines denote years with reforms as listed in Table 1. Results controlled for crime type, sentence length, and criminal history.

The Danish Prison and Probation Service (deciding on EM) and Danish judges (deciding on CS) thus favor those individuals with greater availability of resources in the allocation of non-custodial alternatives. From a public policy perspective, is this endogenous selection reasonable? Results presented in Figure 5 suggest that, with some nuance, it is. Figure 5 shows the 3-year cumulative criminal recidivism risk by sanction type and across the stratifying variables. The plots on the left show results for those with greater resources (measured from higher prior income, higher educational attainment, and majority ethnic background), while the plots on the right show results for those with fewer resources. Splitting the data by prior income and schooling shows that those with more resources

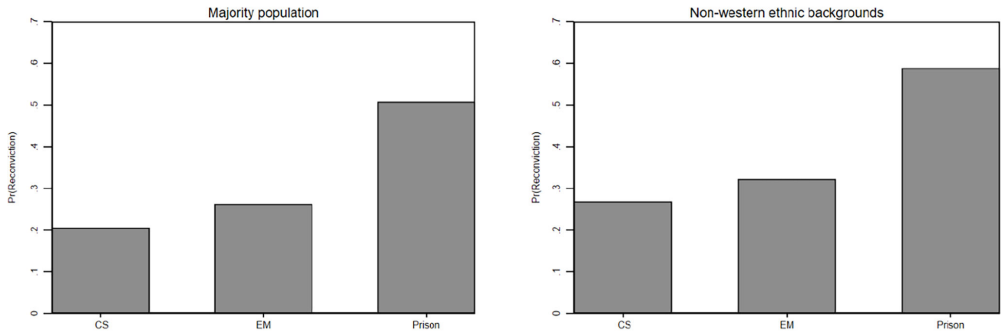
have lower rates of criminal recidivism per sanction type. After three years, the recidivism rate of those with higher prior income and higher educational attainment who served their sentence in prison was almost identical to that among those with lower income and lower educational attainment who served non-custodial alternatives. The recidivism rate of the most negatively selected persons in the group with more resources thus aligns with the rate among the most positively selected persons in the group with fewer resources. In addition, recidivism rates among those with fewer resources who served in prisons are extremely high, about or exceeding 60%.



(a) Prior income



(b) Educational attainment



(c) Ethnic background

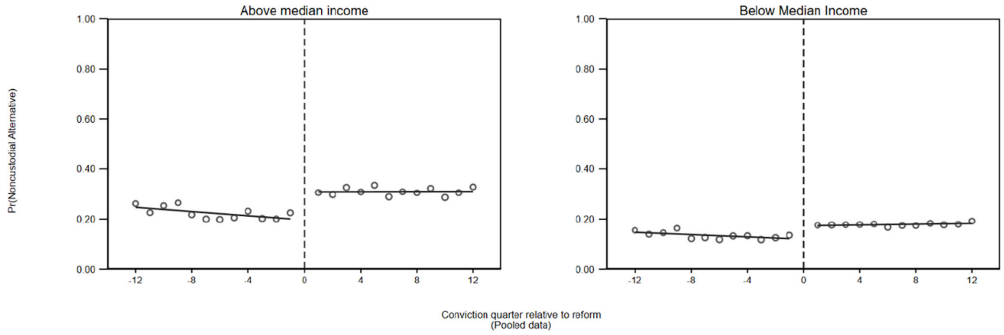
Figure 5. Cumulative proportion with recidivism three years after original conviction, by sentence type and by stratifying variables

Notes: Data pooled across all sentences (shorter than one year or equivalent for CS), 1990–2020.

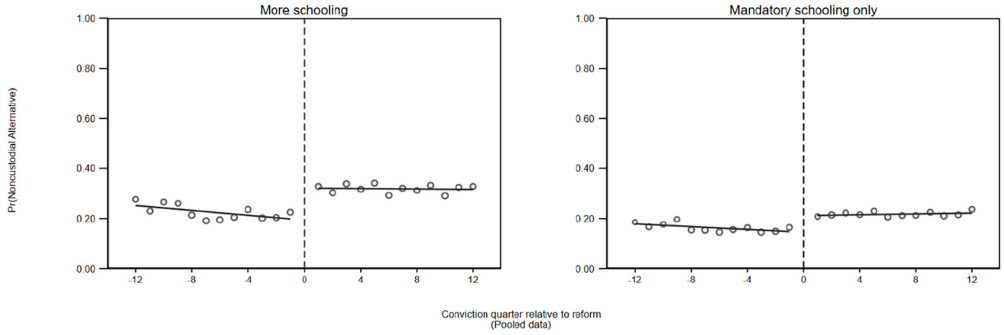
The lower panel of Figure 5 shows the results for ethnic background. Rates of criminal recidivism per sanction type are almost identical for people of ethnic majority and

non-Western ethnic minority backgrounds. Persons who served custodial sentences still have the highest recidivism rates, followed by EM and CS. However, the ethnic gap in recidivism rates across sanction types does not appear to be as large as that for prior income and education.

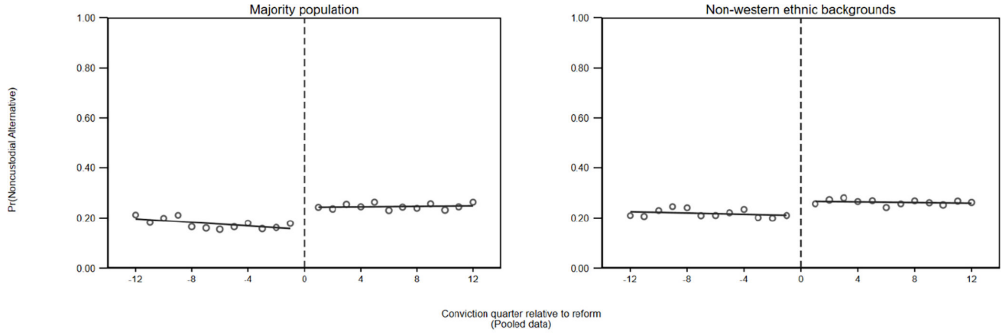
Figures 6 and 7 repeat the policy evaluation approach to obtain unbiased estimates of the effects of sanction type on criminal recidivism on the data pooled across all reforms but split by the stratifying variables. Figure 6 shows that the policy reforms jointly benefited groups with more resources to a greater degree than groups with fewer resources. The discontinuous jumps in the proportion allowed to serve non-custodial alternative sentences are larger for persons with above median prior income, with higher educational attainment, and with majority ethnic backgrounds. Estimates reported in Table A5 suggest



(a) Prior income



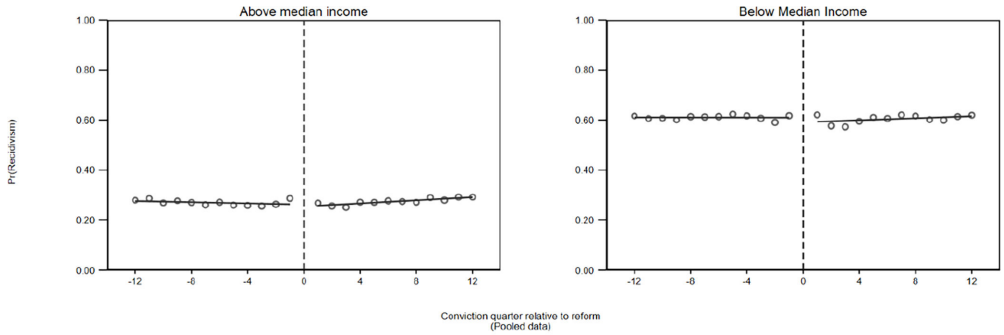
(b) Educational attainment



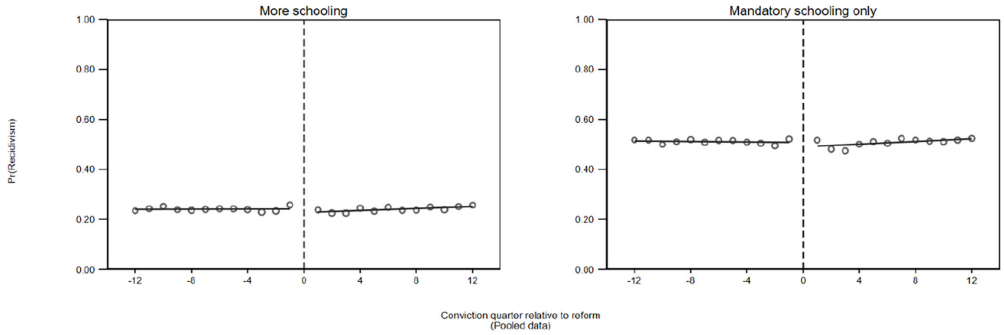
(c) Ethnic background

Figure 6. Proportion of relevant sentences served in non-custodial alternative (CS or EM) by conviction quarter before and after policy reforms, and by stratifying variables

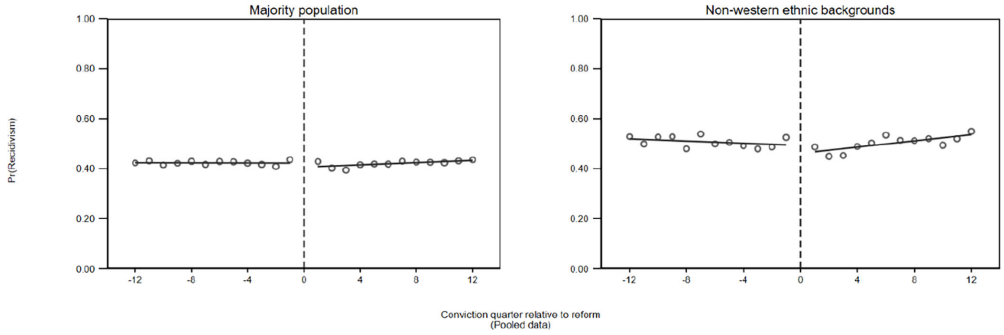
Notes. Data pooled across all reforms listed in Table 1 (except the reform in 2012 as this affected only sentences longer than 1.5 years).



(a) Prior income



(b) Educational attainment



(c) Ethnic background

Figure 7. Proportion convicted of new crime committed within three years, by conviction quarter before and after policy reforms and by stratifying variables

Notes. Data pooled across all reforms listed in Table 1 (except the reform in 2012 as this affected only sentences longer than 1.5 years).

this is approximately twice as high. However, as in the general data, Figure 7 shows that none of the reform-induced discontinuities in the chance of not going to prison led to any discernable response in the criminal recidivism rate (see Table A5 for estimates from Equation 1 per group; all estimates are negative which excludes harmful within-group effects of the reform).

Discussion

The use of non-custodial alternatives to imprisonment has grown dramatically over the past 30 years in Denmark. CS and EM, the focus of this paper, were barely in use in the early 1990s; however, by the late 2010s, more than half of sentences that could cost up

to one year in prison were served outside of prison. Non-custodial alternatives to imprisonment have thus become an integral part of the criminal justice system in Denmark, as in other Nordic countries (Lähteenmäki, 2022). This paper focused on this trend and explored its implications for public policy and for the fundamental principle of equality before the law. The main conclusions from the study are as follows.

First, people who serve a non-custodial alternative sentence have much lower rates of criminal recidivism than those who serve prison sentences. Given the endogenous nature of who is selected for (or "allowed into") non-custodial alternatives, this should be no surprise. Selection for non-custodial alternatives generally favors risk reduction, with priority given to those with the lowest risk of failure. In combination with the empirical findings of this paper that sanction type seems causally inconsequential for recidivism, the implications of this observation are important: The use of non-custodial alternatives to imprisonment thus does not cause significant public harm (if anything, they may have resulted in slightly decreased recidivism).

Second, although other studies have found that non-custodial alternatives have positive effects on individual and family outcomes, the results presented here corroborate the findings of previous systematic reviews (e.g., Renzema & Mayo-Wilson, 2005), namely that the effects of non-custodial alternatives on criminal recidivism are unclear.

Third, the "null findings" presented in this paper can inform public policy. While policy planners might hope to see positive outcomes from non-custodial alternatives, these alternatives are also a success as they can reach the same outcomes at substantially lower costs than imprisonment and with less intrusion into people's lives. Also, studies of other domains of life have documented direct benefits of the alternatives on private costs (better labor market attachment and lower risk of relationship dissolution, for example), which should not be overlooked. From this perspective, the true "smartness" of smart sentencing may lie in a much more efficient allocation of costs and reduced harm imposed on convicted people and their families.

Fourth, "smartness" may promote social inequality. Because recidivism risks generally correlate with social background, risk reduction means selecting for alternative sentences those convicted people who have more socioeconomic resources. Indeed, over the 30-year period covered in this paper, the chances of not serving a sentence in prison have grown differentially across prior income, educational attainment, and ethnic background for people convicted and sentenced for similar crimes. Per Danish law, the selection of people to sentences should indeed be endogenous, considering personal circumstances, which implies that inequality was even to some degree politically intended (and that there may be several interpretations of the efficiency/equality tradeoff that I discuss in this paper). Data related to reforms to expand CS and EM over the data period show no signs that recidivism rates are differentially impacted by serving non-custodial alternatives across background characteristics.

Last, while a lower proportion of convicted people of non-Western ethnic minority backgrounds are sentenced to non-custodial alternatives (relative to the ethnic majority group), recidivism rates are almost identical across ethnic background within sanction types. This finding could be interpretated in two ways. On one hand, the prison service may not consider ethnic background when selecting people for non-custodial alternatives; is it simply the case that fewer non-Western ethnic minority individuals qualify for these alternatives. On the other hand, it might be the case that people of non-Western ethnic backgrounds pose a higher recidivism risk overall and, for this reason, the prison service is less likely to favor this group for alternative sentencing. If the prison service were to

not consider ethnic background, recidivism rates among ethnic minority people in CS and EM would then be higher than for people from ethnic majority backgrounds. As it is not possible to test between these two interpretations with the data at hand, future research should explore this further.

A broader implication of the results presented in this paper concerns the potential long-term consequences of the current weighing of policy efficiency and equality before the law. Denmark appears to weight the former higher than the latter, reflecting the pragmatic aim of reducing public risk. However, because studies have found non-custodial alternatives to be beneficial to several social outcomes, the inequality in sentencing people to these alternatives may reinforce social inequalities with regard to the resocialization of people convicted of a crime. According to this logic, the prison service in Denmark may unintentionally shape future social problems through their current pragmatic methods of selection to non-custodial alternatives. The results presented in this paper do not suggest any causal crime-reducing effects of non-custodial alternative sentences on individual offenders on recidivism. But other prosocial effects may compound recidivism effects in the longer term, just as lower individual and family costs (e.g., to employment and romantic relationships) indeed make alternative sanctions more attractive also regarding their consequences. Given Wildeman and Andersen (2017) finding of the intergenerational protective effects of CS on youth crime among sons, this logic extends to the next generation too. From a public policy perspective, greater understanding of the types of "looping" processes that may arise from the current sanctioning regime is essential, and I encourage future research to explore this further.

Study limitations

There are several limitations to this study. First, CS and EM are merely two of a much broader palette of non-custodial alternatives to imprisonment. Various types of probationary supervision, treatment programs, and programs designed for youth all contribute to the sanctioning regime at different points in time. The focus on CS and EM in this paper was pragmatic; there have been several reforms hereof over the 30-year period under review. In addition, and perhaps most importantly, both CS and EM are explicitly stated in policy documents as direct alternatives to imprisonment. I invite other studies to focus on the full palette of sanction alternatives.

Second, this paper focused on two parameters, namely, selection for non-custodial alternatives and reconviction for new crimes. From a criminological perspective, this outcome set is limited, as many other margins of punishment and recidivism are likely relevant to fully understanding the depth of responses to sentencing trends over the 30-year period.

Third, the Nordic countries are, in many ways, forerunners in the use of non-custodial alternatives to imprisonment. But the structures and implementations of these alternatives differ in many ways across these countries (Lappi-Seppälä, 2019), just like the countries differ in their interest in and approach to minimizing social inequalities. And, importantly, these aspects may have trended differentially across the Nordic region over the long period that I focus on in this study. Therefore, it would have been ideal to also have accessed data from some or all of the other Nordic countries to perform a full comparative analysis of the developments identified in this paper for Denmark. Unfortunately, I did not have access to such data to perform a comparative study. I hope to access such data in the future.

Conclusion

The use of non-custodial alternatives to imprisonment represents an example of some of the challenges of designing public policy when fundamental principles and pragmatism collide. This paper has demonstrated this issue by focusing on 30 years of policy expansions to Denmark's CS and EM schemes. Specifically, favoritism for people most likely to succeed in non-custodial alternatives has led to people with lower recidivism risks serving sentences in the community (which seems pragmatically preferable) while effectively challenging fundamental principles of equality before the law.

The supplementary material is available via a link (online supplementary file 1).

Competing interests

The author declares no competing interests.

Data availability

Analyses are based on restricted-access Danish administrative registers. Researchers interested in obtaining access to these data must file a formal application to, and comply with the rules stated by, Statistics Denmark. Data use is subject to the GDPR. Code used for generating results presented in this paper is available under the Open Science Framework: https://osf.io/jexq8/?view_only=f628f7bc238f42b29309b70704528604.

Funding

This research was funded by the ROCKWOOL Foundation, grant number 1198. The funder had no influence on the research question, data production, analyses, or results interpretation.

Notes

1. https://www.retsinformation.dk/eli/lta/2024/434 (retrieved on June 19, 2024)
2. For a table of the full alignment of CS hours with prison sentence length, see p. 14 of the CIR1H nr 9006 of 01/01/2020 circulate by the Danish Director of Public Prosecutions (*Rigsadvokaten*), available at https://www.retsinformation.dk/eli/retsinfo/2020/9006 (visited February 29, 2024).
3. Unemployed offenders can fulfill the employment criterion by working at institutions appointed by the Prison and Probation Service.
4. Jørgensen (2011) did not apply the same sampling strategy in the post (only those receiving EM information letters) and pre (those formally supposed to receive EM information letters, had EM been possible) reform periods, which may challenge interpretation of results as strictly causal. This is because there could be unobserved differences between those formally covered by the law and those receiving the EM information letters.
5. https://www.un.org/en/about-us/universal-declaration-of-human-rights (retrieved on April 30, 2024).
6. https://www.dst.dk/en/OmDS/strategi-og-kvalitet/kvalitet-for-statistikproduktion/lovgivning (visited on February 27, 2024) and https://www.dst.dk/en/TilSalg/Forskningsservice/Dataadgang (visited on February 27, 2024).
7. https://gdpr-info.eu/ (visited on February 27, 2024).
8. The policy evaluation exercise rests on several other key statistical assumptions, which are tested in Tables A1-A4 and in Figure A1. Except for imbalance to the observation density around the policy cutoff (Table A2), none of the tests imply assumption breaches.
9. Again, for each subsample, the same identifying assumptions applied as in the pooled data, tests for which are reported in Figure A2 and Tables A1–A4.
10. Eligibility evaluation for EM included a qualitative interview; this type of face-to-face interaction was restricted during the pandemic.

11.See Figure A3 for results that are not controlled for crime type, sentence length, and criminal history. Differences across the stratifying variables are approximately twice those shown in Figure 3.

References

Andersen, L. H. & Andersen, S. H. (2014). Effect of Electronic Monitoring on Social Welfare Dependence. *Criminology & Public Policy*, *13*(3), 349–379. https://doi.org/10.1111/1745-9133.12087https://onlinelibrary.wiley.com/toc/17459133/13/3

Andersen, S. H. & Wildeman, C. (2014). The Effect of Paternal Incarceration on Children's Risk of Foster Care Placement. *Social Forces*, *93*(1), 269–298. https://doi.org/10.1093/sf/sou027

Andersen, S. H. (2015). Serving Time or Serving the Community? Exploiting a Policy Reform to Assess the Causal Effects of Community Service on Income, Social Benefit Dependency and Recidivism. *Journal of Quantitative Criminology*, *31*(4), 537–563. https://doi.org/10.1007/s10940-014-9237-2

Andersen, L. H. (2018). Danish register data: flexible administrative data and their relevance for studies of intergenerational transmission. In V. I. Eichelsheim & S. G. A. van de Weijer (Eds.), *Intergenerational continuity of criminal and antisocial behavior: an international overview of studies*. Routledge.

Anker, A. S. T. (2023). Educational Consequences of Paternal Incarceration: Evidence from a Danish Policy Reform. *Journal of Quantitative Criminology*, *39*(1), 125–160. https://doi.org/10.1007/s10940-021-09531-8

Bhuller, M., Dahl, G. B., Løken, K. V. & Mogstad, M. (2020). Incarceration, Recidivism, and Employment. *Journal of Political Economy*, *128*(4), 1269–1324. https://doi.org/10.1086/705330

Braman, D. (2004). *Doing Time on the Outside: Incarceration and Family Life in Urban America*. University of Michigan Press. https://doi.org/10.3998/mpub.17629

Byrne, J. M., Lurigio, A. J. & Petersilia, J. (1992). *Smart Sentencing. The Emergence of Intermediate Sanctions*. Sage.

Clausen, S. (2007). *Samfundstjeneste – virker det?*. Jurist- og Økonomforbundets Forlag.

Damm, A. P. & Gorinas, C. (2020). Prison as a Criminal School: Peer Effects and Criminal Learning behind Bars. *The Journal of Law and Economics*, *63*(1), 149–180. https://doi.org/10.1086/706820

Danish Prison and Probation Service. (2016). *Kriminalforsorgens recidivstatistik 2015*. Retrieved on 6. March 2024. Report available at https://kriminalforsorgen.dk/wp-content/uploads/2018/12/recidiv-2015.pdf

Danish Prison and Probation Service. (2023). *Statistik 2022*. Retrieved on 6. March 2024. Report available at https://kriminalforsorgen.dk/wp-content/uploads/2024/01/kriminalforsorgens-statistik-2022-a.pdf

Di Tella, R. & Schargrodsky, E. (2013). Criminal Recidivism after Prison and Electronic Monitoring. *Journal of Political Economy*, *121*(1), 28–73. https://doi.org/10.1086/669786

EddyJ. M. & Poehlmann-TynanJ. (2019). *Handbook on Children with Incarcerated Parents: Research, Policy, and Practice*. Springer. https://doi.org/10.1007/978-3-030-16707-3

Fallesen, P. & Andersen, L. H. (2017). Explaining the Consequences of Imprisonment for Union Formation and Dissolution in Denmark. *Journal of Policy Analysis and Management*, *36*(1), 154–177. https://doi.org/10.1002/pam.21933

Goffman, E. (1963). *Stigma: Notes on the Management of Spoiled Identity*. Prentice-Hall.

Jørgensen, T. T. (2011). *Afsoning i hjemmet. En effektevaluering af fodlænkeordningen*. Danish Ministry of Justice. Retrieved from 19. June 2024 https://www.justitsministeriet.dk/sites/default/files/media/Arbejdsomraader/Forskning/Forskningsrapporter/2011/Afsoning_i_hjemmet_-_En_effektevaluering_af_fodlaenkeordningen.pdf

Klement, C. (2015). Comparing the Effects of Community Service and Imprisonment on Reconviction: Results from a Quasi-Experimental Danish Study. *Journal of Experimental Criminology*, *11*(2), 237–261. https://doi.org/10.1007/s11292-015-9231-1

Landersø, R. (2015). Does Incarceration Length Affect Labor Market Outcomes? *The Journal of Law and Economics*, *58*(1), 205–234. https://doi.org/10.1086/682911

Lappi-Seppälä, T. (1993). Community Service and the Neo-Classical Framework of Penal Sanctions. *Rapport fra Kontaktseminar om samfunnstjeneste, Reykjavik, Island 1992. Nordisk Samarbejdsråd for kriminologi.*

Lappi-Seppälä, T. (2007). Penal Policy in Scandinavia. *Crime and Justice*, *36*(1), 217–295. https://doi.org/10.1086/592812

Lappi-Seppälä, T. (2019). Community Sanctions as Substitutes to Imprisonment in the Nordic Countries. *Law and Contemporary Problems*, *82*(1), 17–50. https://scholarship.law.duke.edu/lcp/vol82/iss1/3

Larsen, B. Ø. (2017). Educational Outcomes After Serving with Electronic Monitoring: Results from a Natural Experiment. *Journal of Quantitative Criminology*, *33*(1), 157–178. https://doi.org/10.1007/s10940-016-9287-8

Lyngstad, T. H & Skardhamar, T. (2011). Nordic Register Data and their Untapped Potential for Criminological Knowledge. *Crime and Justice*, *40*(1), 613–645. https://doi.org/10.1086/658881

Lähteenmäki, N. (2022). Electronic Monitoring as a Non-Custodial Sanction: Implementation, Experiences, and Nordic Criminal Policy. *Nordisk Tidsskrift for Kriminalvidenskab*, *109*(1), 37–44. https://doi.org/10.7146/ntfk.v109i1.130286

Marklund, F. & Holmberg, S. (2009). Effects of Early Release from Prison Using Electronic Tagging in Sweden. *Journal of Experimental Criminology*, *5*(1), 41–61. https://doi.org/10.1007/s11292-008-9064-2

Muiluvuori, M.-L. (2001). Recidivism Among People Sentenced to Community Service in Finland. *Journal of Scandinavian Studies in Criminology and Crime Prevention*, *2*(1), 72–82. https://doi.org/10.1080/140438501317205556

Øster, M. K. & Rokkan, T. (2018). The Norwegian Approach to Electronic Monitoring: Changing the System and Making a Difference. *Federal Sentencing Reporter*, *31*(1), 75–84. https://doi.org/10.1525/fsr.2018.31.1.75

Renzema, M. & Mayo-Wilson, E. (2005). Can Electronic Monitoring Reduce Crime for Moderate to High-Risk Offenders? *Journal of Experimental Criminology*, *1*(2), 215–237. https://doi.org/10.1007/s11292-005-1615-1

The Judicial Committee to the Danish Parliament. (2012). *Fodlænker mindsker tilbagefald til kriminalitet*. Press release, REU alm. del bilag 54. Retrieved from 26. April 2024 fra https://www.ft.dk/samling/20111/almdel/REU/bilag/54/1034713.pdf

Thistlethwaite, D. L. & Campbell, D. T. (1960). Regression-Discontinuity Analysis: An Alternative to the Ex Post Facto Experiment. *Journal of Educational Psychology*, *51*(6), 309–317. https://doi.org/10.1037/h0044319

Western, B., Kling, J. R. & Weiman, D. F. (2001). The Labor Market Consequences of Incarceration. *Crime & Delinquency*, *47*(3), 410–427. https://doi.org/10.1177/0011128701047003007

Wildeman, C. & Andersen, S. H. (2017). Paternal Incarceration and Children's Risk of Being Charged by Early Adulthood: Evidence from a Danish Policy Shock. *Criminology*, *55*(1), 32–58. https://doi.org/10.1111/1745-9125.12124

Wildeman, C. & Andersen, L. H. (2020). Long-Term Consequences of Being Placed in Disciplinary Segregation. *Criminology*, *58*(3), 423–453. https://doi.org/10.1111/1745-9125.12241