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30 YEARS OF PRISON ALTERNATIVES IN DENMARK: POLICY EFFICIENCY AND INEQUALITY BEFORE THE LAW

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ABSTRACT

The use of non-custodial alternatives to imprisonment has grown dramatically in the Nordic region for several decades, and non-custodial alternatives are by many viewed as “smart” sentences because they reach several of the same punitive aims as imprisonment but are less intrusive and run at much lower costs. But only few studies have taken a step back and analyzed the broader implications of expanding the use of non-custodial alternatives to imprisonment the way it has been done in the Nordic region. This paper focuses on the case of community service and home confinement under electronic monitoring in Denmark. These programs have been expanded 12 times over the past 30 years. Results from analyses on population register data show that today, the risk of serving the equivalent of a prison sentence of up to a year inside the prison walls falls below 50 percent because of these alternatives. The paper then analyzes this development in terms of policy efficiency and the implications for the principle of equality before the law that these policies’ endogenous selection of people to sanction alternatives have. Results indicate persistent inequality in access to non-custodial alternatives along income, education, and ethnic background gradients – but also show that differences in recidivism rates across these gradients (except for ethnic backgrounds) largely exceed differences that arise from the endogenous selection of people to alternatives within the gradients. Results thus portrait the dilemmas of striking the right balance between the principle of equality before the law and policy efficiency.

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 (i.e., overcrowding, or growing prison queues) and in expectation that they could improve resocialization by providing a better balance between the punitive and rehabilitative features of punishment (Andersen and Andersen 2014; Øster and Rokkan 2018). Expectations seem to have been met, at least in Denmark, the context of this study: Either by allowing convicted persons to serve their sentence at home under electronic monitoring (EM) or by contributing to the local community through community service (CS), non-custodial alternatives to imprisonment have been shown to lead to better outcomes for sentenced persons (e.g., Andersen 2015; Andersen and Andersen 2014; Larsen 2017) and their families (Andersen and Wildeman 2014; Anker 2023; Fallesen and Andersen 2017; Wildeman and Andersen 2017). Similar positive effects have been found on criminal recidivism in Sweden (Marklund and Holmberg 2009) and outside the Nordic region (e.g., Di Tella and Schargrofsky 2013), although results from several systematic reviews are much less convinced about the effectiveness of EM (e.g., Renzema and Mayo-Wilson 2005).¹

When deciding whether a convicted person should serve a sentence in prison or in an alternative, courts or the relevant prison services naturally focus on *risk reduction*; the favoritism of convicted people with higher chances of completing the non-custodial alternative without breaching the conditions of it, such as committing new crimes. This favoritism makes sense, as the legitimacy of knowingly bracketing the incapacitation effect of incarceration (by serving a sentence in the

¹ A critical study from Norway (Andersen, Hyatt, and Telle 2020) also showed that the mere availability of EM as a non-custodial alternative has led judges there to sentence marginal cases harder – i.e., handing down short prison sentences rather than non-prison alternatives in expectation that such marginal persons will then be considered for EM. Effectively, per the study, this change in sentencing behavior led to an (depending on model choice) estimated range of 0.6–5.5 percent of post-EM-implementation prison sentences to go to people that most likely would not have received a prison sentence for their crime if EM had not been available.

community) is a delicate matter that could easily lose public support if, for example, convicted people committed new and hideous crimes while serving a sentence in the community.

From a public policy perspective, the risk reduction strategy seems successful. Rates of criminal recidivism rates are lower among those who serve a sentence in an alternative than among persons released from prison (for CS and EM, recidivism rates are about half the rate of persons released from prison, Danish Prison and Probation Service 2016).² And the costs of running non-custodial alternatives are generally far lower than the costs of running prisons. For the same reasons, the successful matching of sentencing alternatives to convicted persons most likely to benefit from (or at least not breach the conditions of) them is known as “smart sentencing” in the research literature (e.g., Byrne, Lurigio, and Petersilia 1992).

But despite (or, perhaps because) of the smartness of sentencing alternatives, few studies to date have taken a step back and focused on the broader implications for criminal justice and the (Nordic) criminal justice systems that now rely heavily on the use of non-custodial alternatives to imprisonment (see, however, Lähteenmäki 2022). This oversight is important, as there may well be an inverse relationship between policy efficiency (“smartness”) and social inequality, exactly because the non-random matching of programs to people may systematically favor or disfavor certain groups in society.

In this study, I use granular population register data from Denmark over the 1990-2020 period to show the growth in non-custodial alternatives to imprisonment over a 30-year period, and I analyze the implications of this growth from both efficiency and inequality perspectives. In terms of

² The direct comparison of recidivism rates among people released from different sentence alternatives is likely to be misleading, however, as the association between sentence type and recidivism could well be confounded by other features that determine both sentence type and recidivism risk (i.e., a selection effect). Results in Di Tella and Schargrodsky (2013) use the random assignment of cases to judges who differ in their tendency to use electronic monitoring instead of imprisonment in Argentina to overcome the selection issue. They find recidivism-reducing benefits of home confinement relative to imprisonment. In my analytical strategy, I aim to overcome selection issues by relying on policy-induced variation in access to CS and EM, to which I return.

policy efficiency, I focus on the implications of expanding the use of non-custodial alternatives for the overall criminal recidivism that the surrounding society is exposed to. I do this both descriptively and while relying on data before and after several policy expansions (which allows for causal inference). In terms of inequality, I focus on how the non-random selection of people into non-custodial alternatives have favored or disfavored specific groups, as defined from prior income, education level, and ethnic minority backgrounds.

The Nordic countries have a strong tradition for social equality. But we also have a strong tradition for pragmatism in policy (Tonry 2007). Results from this study show that it may not always be possible to reach both efficiency and equality ends simultaneously. Whether to weigh highest risk reduction or equality before the law thus becomes an ideological and political decision, a trade-off which policy makers ought to be aware of.

BACKGROUND

The purpose and costs of imprisonment

From a philosophy of punishment point of view, imprisonment serves several purposes. First, being the most severe sanction type in most modern democracies, prisons serve as the main *general deterrent* of crime. People simply refrain from committing crimes because of the risk of imprisonment, the argument goes. Second, known as *specific* or *individual deterrence*, once a person has experienced the pains of imprisonment, that person will be less likely to break the law again because the person now knows just how miserable life in prison is. Third, also associated with the pains of imprisonment, imprisonment *retaliates* the wrongdoing that an imprisoned person has exposed a victim to, effectively satisfying the victim's and the population's sense of justice. Fourth, imprisonment *incapacitates* a person, mechanically disabling the person from committing new

crimes in society throughout the sentence. And fifth, imprisonment provides the chance for engaging a person in *resocializing and rehabilitative* programs with the ambition of transforming the person into a law-abiding citizen upon release.

Prisons are costly to run. In Denmark³ and Norway⁴, prisons use around 3.5 and 2.6 out of every 1,000 of total public spending, for example. These amounts correspond to more than DKK 3.5 billion and NOK 5 billion per year (corresponding to about USD ½ billion per year).

But as documented by extensive research, imprisonment can also be costly for persons experiencing it. A key mechanism behind such costs relates to the sociological concept of stigma (Goffman 1963), whereby the surrounding society (whether correct or not) infers personal features and characteristics from the information that a person was imprisoned, essentially making moral judgment of character. Stigma processes associated with imprisonment have been shown to lead to lower post-release employment chances, for example (Braman 2004). Another mechanism is that one may lose human capital and skills while being in prison, effectively decreasing one's value on the labor market (e.g., Western, Kling, and Weiman 2001). Not focusing on time spend away from the surrounding society but instead on the influences one may experience during confinement, two last mechanisms are relevant too. The first relates to the conditions one serves a sentence under, as these often-harsh conditions may directly harm people during imprisonment, such as when placement in restrictive housing harms post-release employment chances and criminal recidivism (Wildeman and Andersen 2020). The other relates to incarceration almost inevitably tying a person to other known criminals, which may exert negative influences (Damm and Gorinas 2020). Much the same mechanisms as were just described extent to family members too, but here, the separation

³ <https://www.dst.dk/da/Statistik/nyheder-analyser-publ/bagtal/2020/2020-08-31-hvordan-bruger-det-offentlige-tusind-kroner> (retrieved on February 23, 2024).

⁴ <https://www.ssb.no/statbank/table/10725/tableViewLayout1/> (retrieved on February 23, 2024).

families experience during the imprisonment of one family member may additionally strain families (see e.g., Eddy and Poehlmann-Tynan 2019).

Other studies find more mixed and even positive effects of imprisonment on imprisoned persons. Bhuller, Dahl, Løken, and Mogstad (2020), for example, found that in Norway, imprisonment indeed has negative effects on the labor market outcomes of people who had labor market attachment prior to imprisonment. But interestingly, they also find that imprisonment improves labor market outcomes for imprisoned persons with little-to-no labor market attachment prior to imprisonment. In Denmark, Landersø (2015) shows that increasing sentence lengths for young violent offenders leads to better labor market outcomes post-release, a finding that may have arisen from the policy reform under study increasing sentences across the threshold for when prison personnel are required by law to formally engage with resocializing measures, such as the development of individual action plans.

In all, prisons serve several key societal functions, which perhaps is why we are willing to spend so much money on them. Research documents that the promise of prisons may not be fully fulfilled, however, as prisons have mixed and often even damaging effects that extend well beyond those intended functions.

The promise of non-custodial alternatives to imprisonment and their use in Denmark

The fundamental promise of non-custodial alternatives to imprisonment is that they may reach some of the same societal purposes as prisons but at lower costs. The “facility costs” of non-custodial alternatives tend to dwarf those of prisons. In Denmark, for example, the daily cost of keeping a convicted person in EM is about half that of keeping the person in open prison (DKK 546 in EM vs.

DKK 1,103 in open prison in 2012, the Judicial Committee to the Danish Parliament 2012). The use of non-custodial alternatives to imprisonment thus accumulate to substantial cost savings.

Non-custodial alternatives to imprisonment cover all sanction types that are direct alternatives to imprisonment but without the confinement of a person to prison buildings. In principle, this definition includes all non-custodial sentences, ranging from fines to home confinement. In practice, however, fines and even standard probation supervision are not seen as direct alternatives to imprisonment, wherefore it makes more sense to think of non-custodial alternatives to imprisonment as “intermediate sanctions”; sanctions that carry elements of imprisonment’s fundamental logic and are (by law) direct alternatives to imprisonment, but where the sentence is not served within the confines of a prison (Byrne 2016).

The focus of this paper is on CS and EM. CS consists of a specified number of hours a convicted person must undertake unpaid work to the benefit of the community (in either a private firm, a public institution, or in an NGO). Such work may include an array of tasks, including service at the local library or a museum. CS is carried out in the person’s leisure time, whereby the person serving in CS may retain normal employment while serving the sentence. There is no fixed translation between the number of hours of CS a person is sentenced to and the length of the prison alternative, but guidelines in Denmark, for example, state that 30 hours of CS to be worked within 4 months functions as alternative to imprisonment for 7-14 days; 40 hours CS within 4 months is alternative to 20-30 days of imprisonment; as so forth.⁵ CS always includes probationary supervision throughout the period during which the CS hours must be carried out.

The CS scheme in Danmark has been gradually expanded over the past 30 years via policy reforms, and the first columns of Table 1 summarizes the reforms. Following a trial period, CS was

⁵ For the full table that aligns hours of CS with prison sentence length, see table on page 14 in the CIR1H nr 9006 of 01/01/2020 circulate from the Danish Director of Public Prosecutions (*Rigsadvokaten*), available on <https://www.retsinformation.dk/eli/retsinfo/2020/9006> (visited February 29, 2024).

formally implemented as a sentence in 1992 as a non-custodial alternative to prison sentences 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 penal systems legitimacy in the population, wherefore crime types such as violence, robbery, sex crimes, and drugs crimes were generally not considered for CS. By the end of the period, the scheme includes far more crime types and CS now 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 last CS reform (2020) limits the use of CS for people convicted of more serious or repeat cases of violence, and thus counters the general trend of expanding the use of this sanction alternative.

[Insert Table 1 about here]

EM in Denmark is a way of serving a prison sentence. EM consists of an electronic device (fitted around the ankle) that in real time sends the sentenced person's location to the relevant overseeing authority, the Prison and Probation Service. EM comes with a daily schedule that specifies where one is allowed to be located at certain hours – such as at a designated workplace or labor market training facility 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 also comes with restrictions on alcohol and drug use (and random spot tests for such substances), and people serving a prison sentence at home under EM must complete a crime reduction course at the Prison and Probation Service. Additional requirements include having a permanent address (e.g., not be living in an acute housing unit) and either be employed, participate in some form of active labor market program, or be enrolled in an education program.⁶ The

⁶ Unemployed offenders can fulfill this employment criterion by working at institutions appointed by the Prison and Probation Service.

employer must formally accept the EM, as do adult members of the electronically monitored person's household.

The EM scheme in Denmark has been gradually expanded since it was first introduced in May 2005, and the second column in Table 1 summarizes the relevant policy reforms. As with CS, the EM scheme seems to have been cautiously introduced – first to include only traffic offenders and then expanded to young people and only for comparatively short sentences. But then expanded to include several crime types and sentences up to six months. Today, just as in both Norway and Sweden, people can also be released from prison early on parole under EM (in 2022, 745 out of 2,320 early releases on parole in Denmark were in this type of “backdoor” EM [Danish Prison and Probation Service 2023]).

Both CS and EM aim to reach some of the same societal purposes as prisons, at the expense of imprisonment's incapacitation effect and perhaps also at the expense of deterrence effects and retaliation effects. The incapacitation effects of non-custodial alternatives obviously are lower than those of imprisonment, as the mere serving of a sentence in the community implies some risk of new crimes being committed while serving the sentence. This risk is arguably higher for CS (in which the convicted person must show up for the designated hours of community work but is otherwise free to move around) than for EM, as the constant geo-tagging of EM restricts freedom of movement and easily ties a person to a crime scene. Deterrence effects of EM and CS theoretically are also lower than of imprisonment, as serving a sentence under less restrictive conditions arguably lowers the pains of punishment, and hence lowers deterrence. Observations in qualitative studies of experiences with serving a sentence under EM somewhat counters the latter expectation, however, by showing that convicted people in general prefer EM over imprisonment, even though EM is also viewed as punitive, controlling, and painful (e.g., Vanhaelemeesch, Vander Beken, and Vandeveld, 2013). Retaliation effects – the victim and public's sense of justice – are by some

viewed as lower in non-custodial alternatives than in imprisonment. This is because the alternatives are arguably less invasive and painful to the convicted person than imprisonment. Also, again, as non-custodial alternatives are served in the community, there is a risk of the victim and offender meeting in public while the sentence is being served, which may challenge the victim's sense of justice.

CS and EM have been found to indeed reach the same or even better functions as prisons when focusing on resocialization effects and are therefore by many seen as "smart" sentences because they maximize resocialization while minimizing the damaging effects on the sanctioned person that prisons are known to have (e.g., Byrne 2016). Studies from Denmark that rely on policy-induced variation in sentencing to overcome the non-random selection of convicted persons into non-custodial alternatives document positive or null effects of CS and EM. Andersen (2015) finds higher earnings and lower dependence on social assistance among people serving in CS rather than prison. Andersen and Andersen (2014) find lower dependence on social assistance among young people allowed to serve their sentence under EM with the 2006 reform, but no effect on people older than 25 years that were affected by the 2008 reform to EM. Larsen (2017) finds improved education rates among young people affected by the 2006 reform too.

The positive effects also extend beyond the sanctioned person to the person's family members: CS (relative to imprisonment) produces lower rates of out-of-home placement (Andersen and Wildeman 2014), lower risks of youth crime convictions (sons only, Wildeman and Andersen 2017), and better educational outcomes (Anker 2023) for children. EM has also been shown to lead to lower risks of romantic partnership dissolution (Fallesen and Andersen 2017).

It is important to note that although the different non-custodial alternatives to imprisonment overall function quite similarly across the Nordic region, local differences are important. For a

discussion of similarities and differences in EM and the trends in the use hereof, for example, see Lähteenmäki (2022); focus of the current paper is on the Danish case.

Policy efficiency vs. equality before the law

With non-custodial alternatives to imprisonment, the incapacitation effect of incarceration – the removal of criminally inclined persons from communities – is basically foregone, as was already discussed. This feature imposes a key legitimacy constraint on non-custodial alternatives to imprisonment as sanction type; legitimacy hinges on the alternatives at the very least not leading to more public harm than imprisonment.

Pragmatically, this constraint has led the authorities that decide whom to allow to serve a sentence in the community (the prison services in Denmark, Norway, and Sweden but the judge in Finland, see Lähteenmäki 2022) to focus on risk reduction, i.e., the selection of those believed to have the lowest risk of breaching the terms of the non-custodial alternative and the lowest risk of committing new crimes. And the strategy seems to have been effective in the sense that rates of criminal recidivism are much lower among those who serve a sentence in EM or CS than in prison, as mentioned. The way EM and CS have been implemented thus from a public policy perspective seems to be efficient.

But because risk profiles are closely related to sociodemographic background variables, policy efficiency may clash with another fundamental aim of the Nordic countries: social equality, particularly the principle of equality before the law (which is part of these countries' constitutions). According to the United Nations' Declaration of Human Rights,⁷ people convicted of similar crimes should face similar sanctions from the state. The non-random selection of convicted persons to non-

⁷ <https://www.un.org/en/about-us/universal-declaration-of-human-rights> (retrieved April 30, 2024).

custodial alternatives essentially implies the opposite. This feature is obvious in the case of EM in Denmark. Here, if a judge sentences two people to imprisonment of identical length for identical crimes, their actual sanctions may differ. This is because the prison and probation service administratively decides that whereas one person should be allowed to avoid going to prison, the other has to because he or she does not hold a employment or can afford housing where the electronic monitoring is possible.

Labor market attachment and living arrangements are just two of the factors that discriminate between whether one may be eligible for CS and EM over imprisonment. But many other features which we typically care about when discussing social inequality correlate with such factors, implying that the risk reduction paradigm could essentially promote social inequality before the law. In this paper, I focus on prior income, education level, and ethnic minority backgrounds as examples of such features.

DATA AND METHODS

Danish register data

This study uses register data from Denmark. Most quantitative researchers in the Nordic region know this type of data by heart, as access to these full population administrative data are essentially one of our key comparative advantages in research. 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 Denmark⁸ and in compliance with the EU General Data Protection Regulation (GDPR)⁹. Unique

⁸ <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).

⁹ <https://gdpr-info.eu/> (visited on February 27, 2024).

individual identifiers allow for the merging of individual-level information across an array of registers and across time, making register data a uniquely strong and flexible data source for research. For discussions about the usefulness of Nordic register data for criminological research, see Lyngstad and Skardhamar (2011) and Andersen (2018, which focuses on Denmark).

Criminal justice agencies, such as the national police, the public prosecutors, the courts, and the prison and probation service of Denmark, provide data on all contacts with the criminal justice system. From these data, I select all cases during 1990-2020 that resulted either in a prison sentence shorter than one year or in CS. This choice is based on convictions, i.e., it is a judge who sentences persons to imprisonment or CS. For prison sentences, the Prison and Probation Service then evaluates eligibility for EM, and I include information hereon.

As outcome variable I focus on criminal recidivism as post-conviction behavior, defined as any new violation of the Penal Code that leads to conviction. I follow people in the data for 3 x 365 days from the conviction that selected them into the sample and record the date of their first recidivism crime. Starting the recidivism “clock” at the original conviction date is important to not have differential processing times in prison, CS, and EM sentences affect the follow up period. For recidivism crimes, I focus on crime dates, not the eventual conviction dates, as the lag between the two may be considerable and vary over time. In addition to information on the timing of a person’s first recidivism crime, I also construct a binary variable that takes the value of 1 if a person violates the Penal Code (for which the person is later convicted) within three years of the original conviction, and 0 otherwise. Many criminologists may view these operationalizations of criminal recidivism as overly simple and even reductionist, but the point of the analysis here is not to provide a deep dive into the nature of and structure in criminal recidivism, but rather to showcase whether, from a policy planner perspective, the use of CS and EM strains public safety.

As background variables I add gender, age, and non-western ethnic minority background from the population register. Gender is recorded biological gender at birth. Age measures age at the conviction that selects a person into the data (obtained by subtracting birth date from conviction date). I limit the data to focus only on people aged 18-60 at the time of conviction (over the full 30-year data period, 11,240 observations fall outside this window, corresponding to 3 percent of the data). Non-western ethnic minority background measures whether a person or both the person's parents immigrated to Denmark from a non-western country or not. From the education register, I mark whether a person has mandatory schooling (10 years) as highest educational attainment. The education register relies on official education credentials from the Ministry of Education. From the income register, I add prior income, which relies on all third-party reported income to the Tax Authorities. Income includes all pre-tax income from legal work and from public assistance (such as social assistance and social pensions). In the analyses, I divide people in the data into groups defined from whether they have income above or below the median income during the year before the conviction that select them into the data. Regarding the conviction that selects the person into the data, I add sentence length and information on crime type (violent crime, property crime, traffic offence). I also add 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.¹⁰

Methods

As the first step in the analyses, I provide an overview of the trend in the relevant sentences (shorter than 1 year) and ways of serving these sentences over the 30-year data period, 1990-2020. Here, I

¹⁰ As will be seen, there are no signs of any effects of the sentencing reforms under review on criminal recidivism, so the control variables are only included to analyze whether it would even make sense to use the reforms for causal inference. See also footnotes 10 and 11.

simply plot the annual number of new CS sentences and the monthly number of prison sentences that lead either to prison admission or to “admission” to EM. I also show the proportion of the total number of sentences (shorter than 1 year) that are served in CS or EM to illustrate how dependent on CS and EM the Danish criminal justice system has become over the 30-year period.

In the second step of the analyses, I focus on criminal recidivism. Here, I use the dates of the recidivism crimes to show how the cumulative recidivism rate for each of the comparison groups, i.e., people who served their sentence in either imprisonment, CS, or EM, cumulate over the first three years post-conviction. These results will indicate the societal impact of more and more people being allowed to serve in CS or EM. But simultaneously, they will show just how selective the judges and prison service are when selecting people to serve in CS or EM, according to which logic one should expect the recidivism rates of CS and EM to dwarf those of imprisonment.

Addressing the latter – the nonrandom selection of people into CS and EM – I then move to a policy evaluation setup. As in most published research on non-custodial alternatives to imprisonment, I here rely on changes in convicted people’s chance of serving in CS or EM that arise from the timing of the conviction relative to policy shocks. If a policy shock is effective (in the statistical sense), it will produce a pre-reform universe and a post-reform universe that differ only in terms of policy climate, nothing else. For example, prior to the policy reform on July 1 of 2008, only people younger than 25 years of age could be considered for EM. With the reform, the age criterion was removed, and convicted persons of any age could be considered for EM (contingent on other requirements being met, but these did not change with the reform). With the 2008 reform, those older than 25 years of age thus experienced an abrupt change in their chance of being allowed to serve a prison sentence under EM instead of in prison; a chance that was unrelated to them and their crimes (see Andersen and Andersen 2014 and Fallesen and Andersen 2016 for causal studies based on the 2008 reform in Denmark). The endeavor of analyzing the reform’s effects on criminal

recidivism is then straightforward: Given the exogeneity of the reform one may simply compare rates of criminal recidivism among people in the pre- and post-reform groups; any difference in their average outcomes will be attributable to the difference in the use of CS or EM in the groups.¹¹

But because there can be important time trends in crime, comparing people further away from the reform may be susceptible to bias. I therefore evaluate reform effects in a regression discontinuity design that effectively compares persons sentenced just before and after the policy shocks while taking the trend in criminal recidivism into account (e.g., Thistlethwaite and Campbell 1960). The estimation model is:

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

In the model, y is the dummy variable for individual i 's criminal reconviction within three years from conviction c , the dependent variable. The expression $f(t)$ expresses the functional form of the association between the running variable (time of conviction c relative to the relevant reform), which is allowed to have different linear slopes on each side of the reforms. X represents control variables. Thereby, α represents the average recidivism rate of persons convicted just before the reforms, whereas β represents the average change herein for similar (with respect to X) 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. Standard errors are clustered by the running variable, t_{ic} .

In practice and to avoid showing results for ten reforms separately, I pool data from before and after the reforms to obtain just one pre-reform and one post-reform universe.¹² I do so because focus

¹¹ The policy evaluation exercise obviously rests on several other and key statistical assumptions. It falls outside the scope of this paper to report all evidence in favor of these assumptions here, however. For each reform and in the pooled data, conditional balancing tests, unconditional balancing tests, tests of changes to the data size, and tests for breaks in outcome predictions from the sample composition do not indicate major breaches of these assumptions (although results indicate some imbalance to the observation density around the policy cutoff in the pooled data, see Table A2). For the results from these tests in the pooled data, see appendix (Figure A1 and Tables A1-A4).

here is not on showing whether each of the reforms separately had effects on people who were allowed to serve in the alternatives because of the reforms (local average treatment effects) but on the overall implications of expanding the availability of the alternatives on the overall recidivism rate. 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. Other studies have, as was already mentioned, found positive effects of several of the reforms on the outcomes of sentenced persons and their families (see e.g., Goldman, Andersen, Andersen, and Wildeman 2019 for a discussion of effects on families and children).

In the third step of the analyses, I focus on the distribution of CS and EM and their potential effects across the key background variables prior income, education, and non-western ethnic minority background. I first show the proportion of these groups that are allowed to serve their sentence in CS or EM, essentially repeating the descriptive analysis in the first analytical step by the stratifying variables. One difference to the first analytical step is that here, I present results that are controlled for crime type, sentence length, and criminal history to account for these key variables likely being unevenly distributed across persons defined from prior income, schooling, and ethnic background. Failure to control for these key variables would make it unclear whether differences in sanction types across groups would arise from crime being unevenly distributed or from inequality before the law per se. From a risk reduction perspective, one might expect the proportions to differ (e.g., those with lower education will have lower rates of CS and EM than those with higher education) simply because these features correlate with recidivism risk. But from an equality before the law point of view, such differences are problematic as they indicate that people convicted of similar crimes do not face similar sanctions.

¹² Results for each of the reforms are available on request from the author.

I then turn to the criminal recidivism outcome and repeat both the descriptive and the policy evaluation exercises from the second analytical step but split by the stratifying variables. This exercise serves two purposes: First, to show how unequal the impact of the reforms was. Second, to see whether the policy-induced variation in the chance of serving a sentence in CS or EM translates differentially into criminal recidivism across the groups.¹³

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 that were actually served in prison decreased even more, however. In the early 1990s, almost all sentences were served behind prison walls. But towards the end of the data period this number had dropped to about 3,800. Almost the exact opposite trend is seen for CS. EM only entered the sanction palette in 2005 but then quickly increased in numbers to reach almost 2,300 sentences served in this alternative in 2012. Since then, the number has declined slightly, with the last drop in 2020 likely being caused by limited use of EM during the COVID-19 pandemic.¹⁴

Panel (b) of Figure 1 shows the proportion of total sentences (of max length one year, corresponding to the solid black line in panel (a)) each year that were served in either CS or EM combined (“non-custodial alternative”). This proportion effectively indicates the chance of *not*

¹³ Again, for each subsample, the same identifying assumptions as in the pooled data apply. For the results from these tests in the pooled data, see appendix (Figure A2 and Tables A1-A4).

¹⁴ Eligibility evaluation for EM includes a qualitative interview; the type of face-to-face interaction that was restricted during the pandemic.

serving a sentence in prison although the crime and material matters of the case implies that this could just as well have been the case. 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 these non-custodial alternatives to imprisonment over the period, and with notable increases following some of the reforms that increase the use of these non-custodial alternatives (as one would expect). In the beginning of the period, almost all sentences were served in prisons. Towards the end of the period, the chance of not going to prison to serve what could have been up to a full year in prison exceeds 50 percent, and in 2016/2017 even neared two-thirds.

[Insert Figure 1 about here]

Criminal recidivism

Figure 2 shows the cumulative proportion with at least one new crime that leads to conviction, split by whether the person served a sentence in prison, in CS, or in EM. Over three years (36 months), just above half (51.8 percent) of people who served their sentence in prison were re-convicted at some point. For those who served in the alternatives, the cumulative rates were about half of that: 27.4 percent for EM and 21.5 percent for CS.

[Insert Figure 2 about here]

The large differences in recidivism rates across sanction types are not indicative of causal effects of sanction type on behavior, however. This is because the selection of people to sanction alternatives is far from random and generally aims to reduce public risk, as mentioned.

To narrow in on the causality, Figure 3 focuses on the pooled data from windows before and after each of the reforms over the 1990-2020 period. Panel (a) shows the proportion serving the sentence in a non-custodial alternative (CS or EM). Just prior to reforms, the rate was 25.6 percent (Table A5). But with reforms, the average rate increased discontinuously to 33.8 percent (totaling 33.8 percent). It is this jump in the proportion that serves their sentence in non-custodial alternatives that may be used for measuring the effect of these alternatives on criminal recidivism that cannot be confounded by the (in the data unobserved) mechanisms that select people into the alternatives. Panel (b) shows the criminal recidivism rates of the same people. If the reforms were to credibly (and collectively) alter rates of criminal recidivism, this should be visible as a discontinuous jump in recidivism rates at 0. And 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 of the alternatives for those allowed to serve in them (a LATE estimate). But results in panel (b) show no jump at 0; there appears to be no joint effect of the 1990-2020 reforms on criminal recidivism.¹⁵ Estimates from Equation 1, shown in Table A5, confirm the null. But 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; if anything, results imply recidivism-reducing but imprecisely estimated effects.

[Insert Figure 3 about here]

Inequality before the law

Figure 4 shows the proportion of the relevant sentences over the 1990-2020 period that were served in non-custodial alternatives when the data are split by key stratifying variables (prior income,

¹⁵ Supplementary results available on request from the corresponding author also show no effects of each of the reforms separately.

schooling level, and non-western ethnic minority backgrounds) and controlled for crime type, sentence length, and criminal history.¹⁶ Across the panels, it is obvious that the use non-custodial alternatives expanded dramatically across all these groups, and for all groups, the chance of not serving a sentence (of max one year or comparable for CS) inside a prison exceeds 40 percent. But it is also evident that people with higher prior income, with more schooling, and of majority ethnic backgrounds accumulate higher chances of not serving a sentence in prison – even for (on average) similar crime types, sentence lengths, and criminal histories. As there are employment criteria for being allowed into EM, at least some of the difference between those with prior income below and above the median could be mechanical; having employment increases income. But the difference arises even prior to the introduction of EM in 2005, implying that prior income matters for the assignment of CS too. One may also argue that level of schooling and ethnic minority backgrounds are strongly associated with prior income, again making some of the differences arise mechanically from employment criteria. Yet again, differences arise prior to the introduction of EM. It is thus much more likely that differences in chances of not serving a sentence in prison arise from the endogenous selection of people with lower anticipated recidivism risks to the alternatives – despite these people being convicted of the same types of crimes and (at least directly for EM and prisons) were sentenced to identical sentences.

[Insert Figure 4 about here]

From the data, it thus appears that the Danish Prison and Probation Service (in deciding on EM) and the Danish judges (when deciding on CS) favor people with more resources available to them in the allocation of non-custodial alternatives respective imprisonment. But is this endogenous selection reasonable from a public safety perspective? Results presented in Figure 5 suggest largely

¹⁶ See Figure A3 in appendix for results that are not controlled for crime type, sentence length, and criminal history. Differences across the stratifying variables are approximate twice the ones shown in Figure 3 when not controlled for crime type, sentence length, and criminal history.

yes, but with some nuance. Figure 5 shows the 36-months cumulative criminal recidivism risks by sanction type and across the stratifying variables. The lefthand plots show results for the more resourceful (as measured from higher prior income, longer education, and majority ethnic background) whereas the righthand plots show results for the less resourceful. When splitting the data by prior income and schooling, the more resourceful have lower rates of criminal recidivism per sanction type. After three years, the recidivism rate of those with higher prior income and more schooling who serve their sentence in prison are just about identical to the rate among those with lower income and less schooling who serve in non-custodial alternatives. The recidivism rates of the most negatively selected persons in the more resourceful groups thus align with the rates among the most positively selected persons in the less resourceful groups. And recidivism rates among those who serve in prisons in the less resourceful groups are extremely high, about or exceeding 60 percent.

[Insert Figure 5 about here]

The lower panel of Figure 5 shows interesting results for ethnic backgrounds. Here, rates of criminal recidivism are close to identical for people of ethnic majority and non-western ethnic minority backgrounds, per sanction type. Persons who serve their sentence in prison still have the highest recidivism rates followed by (at a much lower rate) CS and EM. But there does not appear to be remotely as marked an ethnic gap in recidivism rates across sanction types as there is for prior income and education.

Figures 6 and 7 repeat the policy reform approach to obtaining unbiased estimates of sanction type's effect on criminal recidivism on the data pooled across all reforms but split by the stratifying variables. Figure 6 shows that the policy reforms jointly benefitted the more resourceful groups to a larger degree than the less resourceful ones. The discontinuous jumps in the proportion allowed to

serve a sentence in a non-custodial alternative is obviously larger for persons with above median prior income, with more schooling, and with majority ethnic backgrounds than they are for those with lower prior income, mandatory schooling as highest educational attainment, and non-western ethnic backgrounds. Estimates reported in Table A5 suggest about twice as high. But like in the general data, Figure 7 shows that none of the reform-induced discontinuities in the chance of not going to prison leads to any discernable response in the criminal recidivism rate (see Table A5 for estimates from Equation 1 per group; once again, all estimates are negative which excludes harmful effects of the reform even within the groups).

[Insert Figures 6 and 7 about here]

DISCUSSION

The use of non-custodial alternatives to imprisonment (for less than one year) 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, but by the late 2010s, more than half of sentences that could cost up to one year in prison were served outside the prison walls. Non-custodial alternatives to imprisonment have thus become an integral part of the criminal justice system in Denmark, as it has in the other Nordic countries (Lähtenmäki 2022). This paper focused on this trend and posed questions about its implications for public policy and for the fundamental principle of equality before the law. The main overall conclusions from the study are as follows.

First, people who serve a sentence in non-custodial alternatives have much lower rates of criminal recidivism than people who serve a sentence in prison. Given the endogenous nature of who are selected to (or “allowed into”) the non-custodial alternatives, this should come as little surprise. The selection of people to non-custodial alternatives generally favors risk reduction, the

favoritism of those with the lowest risk of failure. In combination with the empirical findings in this paper, the implications of this observation are important. The use of non-custodial alternatives to imprisonment do not cause significant public harm. One might have expected so, however, given that non-custodial alternatives come at the expense of prisons' incapacitation effects. But results presented in this paper uniformly document that, at least in Denmark, the courts and the prison service succeeds in identifying people with lower risk of criminal recidivism to be allowed to serve their sentence in the community. This is seen descriptively from the low rates of recidivism among people in CS and EM. But it is also seen when using policy reforms as instruments for overcoming the comparability problem between people across sanction types; reforms that allowed a higher share of convicted people to serve their sentence in non-custodial alternatives did not affect rates of criminal recidivism to any discernable degree (and, if anything, they may have decreased recidivism slightly).

Second, although other studies have found positive effects of non-custodial alternatives on individual and family outcomes, results presented here do not find overall recidivism-reducing benefits of non-custodial alternatives. Per results presented here, differences in recidivism rates are more likely to arise from the non-random selection of people with lower recidivism risks into the non-custodial alternatives, a finding that corroborates what has previously been found in systematic reviews (e.g., Renzema and Mayo-Wilson 2005). It is important to note here, however, that existing studies referenced above focus narrowly on one or a few of the reforms that expanded the use of CS or EM, and generally focus on one or a few non-crime outcomes, such as dependence on social welfare (Andersen and Andersen 2014) and children's risk of out-of-home placement (Andersen and Wildeman 2014). I focus on the overall implications of having introduced a range of sentencing reforms over a 30-year period – many of which affected groups of sentenced people who were not part of the data in those other studies (longer sentences). Therefore, individual reforms may have

affected specific groups on specific outcomes, but just not to the degree that shows up in the broader data on criminal recidivism.

Third, the “null findings” presented in this paper are quite informative from a public policy perspective. This is because whereas a policy planner might have hoped to see positive effects of non-custodial alternatives, it nonetheless is a success to be able to reach the same outcomes but at a fraction of the cost of imprisonment and while intruding less on people’s lives. From this perspective, the true “smartness” of smart sentences may lie in a much more efficient allocation of costs and harms imposed on convicted people.

Fourth, the flipside of a “smart” focus on risk reduction is a fundamental break away from the ideal of equality before the law, which has otherwise been central to liberal justice. This is because recidivism risks generally correlate with social background, implying that risk reduction also implies selecting to the alternatives convicted people who are better off in socioeconomic resources. Indeed, over the 30-year period in this paper, chances of not serving a sentence in prison have grown differentially across prior income, level of schooling, and ethnic backgrounds – for people convicted and sentenced for similar crimes. This is a challenging but likely unintended consequence of how people are evaluated for non-custodial alternatives. Relying again on the alternative-expanding reforms over the data period, there are no signs that recidivism rates are differentially impacted by serving in non-custodial alternatives across background characteristics, however, as the reforms again uniformly show no discernable effects on recidivism. But is the unequal allocation of non-custodial alternatives then simply driven by prejudices? No, as was shown in this paper, people with fewer socioeconomic resources available to them have higher rates of criminal recidivism even within sanction types.

Fifth, it is an interesting finding that whereas a lower proportion of convicted people of non-western ethnic minority backgrounds are allowed into the non-custodial alternatives (relative to the ethnic majority group), recidivism rates are distributed almost identically across ethnic background within sanction types. This finding can have two interpretations. On one hand, the prison service might not consider ethnic backgrounds when selecting people to non-custodial alternatives, but fewer of non-western ethnic minority backgrounds simply qualify for the alternatives. On the other hand, it might also be the case that people of non-western ethnic backgrounds have higher recidivism risks overall and that the prison service therefore disfavors this group when selecting people to the alternatives; if the prison service were then to not consider ethnic backgrounds, recidivism rates among people of these backgrounds in CS and EM would be higher than for people of ethnic majority backgrounds. As it is not possible to tests between these two interpretations with the data at hand; future research should address this question.

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. As discussed, Denmark seems to weight the former higher than the latter, reflecting the pragmatic aim of reducing public risk. But because studies have found non-custodial alternatives to be beneficial to several social outcomes, the inequality in who are allowed to serve in the alternatives may end up reinforcing social inequalities in people's chances of achieving resocialization altogether. Per this logic, the prison service in Denmark may unintendedly shape their future clientele via their pragmatic selection of people to non-custodial alternatives today. Results presented in this paper do not suggest that there are causal crime-reducing effects of serving in non-custodial alternatives, but still those other prosocial effects could compound into recidivism effects in the longer term. And as studies have shown intergenerational protective effects of CS on sons' youth crime (Wildeman and Andersen 2017), the logic could extent even to the next generation. From a public policy

perspective, it seems pivotal to get more knowledge on these types of “looping” processes that may arise from the sanctioning regime today, and I encourage future research to follow this line of thinking.

Study limitations

There are several limitations to this study that bear mentioning. First, the palette of non-custodial alternatives to imprisonment consists of more alternatives than CS and EM. Various types of probationary supervision, treatment programs, and programs designed for youth exist, and these alternatives all contribute to the sanctioning regime at a given point. The focus on CS and EM in this paper was pragmatic; reforms expanding CS and EM are straightforward to analyze and there have been several of these reforms over the 30-year period under review. Also, and perhaps most importantly, both CS and EM are explicitly stated in policies as direct alternatives to imprisonment, with EM even being “just” a way of serving a prison sentence in one’s home. I invite other studies to focus on the full palette of sanction alternatives, however, to see whether the logic identified in this paper extends to all sanction alternatives.

Second, focus in this paper was on two parameters, namely who are selected into non-custodial alternatives and who are reconvicted for new crimes. From a criminological perspective, this outcome set is quite limited, as many other margins of punishment and recidivism would likely be relevant to fully understand the depth of responses to the trends in sentencing over the 30-year period. I chose to focus narrowly on just these outcomes to keep the analyses simple and just make the point that with such strong trends in how we serve sentences there can be profound and far-reaching implications which criminologists might care about.

Third, although focus of this study was intentionally “distant” from the individual reforms that produced variation in sentencing regimes used for causal inference, the pooling of data across several reforms is not necessarily ideal if one wants to understand the precise link between the reforms and behavior. The pooled data across all the reforms did not seem to violate any of the statistical assumptions needed for using this exercise for inference, nor did statistical diagnoses for data around each reform separately – yet the uniqueness of each reform’s impact may easily have been lost in the pooling process. This choice of flight height was essentially based an aim of addressing the overall implications of expanding the use of non-custodial alternatives to imprisonment as has been done in Denmark over a 30-year period. Most important are the descriptive findings that the way non-custodial alternatives have been implemented in Denmark both offer efficiency gains and produces inequality before the law, and that these two aims may ultimately be impossible to reach at the same time. This conundrum is not easily solved, but at the very least, policy makers should be aware of the potential implications of the policy choices (and the administration hereof) that they decide on.

Last, because the Nordic countries are in many ways forerunners in the use of non-custodial alternatives to imprisonment, a mentioned, it would be ideal to also have access to data from some or all the other Nordic countries to bolster the generalizability of the logics identified in this paper. Unfortunately, I did not have access to data from the other Nordic countries to perform such a comparative study; I hope to see one – or get such data access – in the future.

CONCLUSION

The use of non-custodial alternatives to imprisonment represents a textbook example of some of the dilemmas in designing public policy when fundamental principles and pragmatism collide. This

paper has demonstrated this issue by focusing on 30 years of policy expansions to the CS and EM schemes in Denmark. Specifically, the favoritism of people most likely to succeed in these non-custodial alternatives has led to people with lower recidivism risks serving their sentence in the community (which seems pragmatically preferable) but effectively challenge fundamental principles of equality before the law. How to strike the right balance between legal principles and policy efficiency therefore ultimately becomes a political endeavor.

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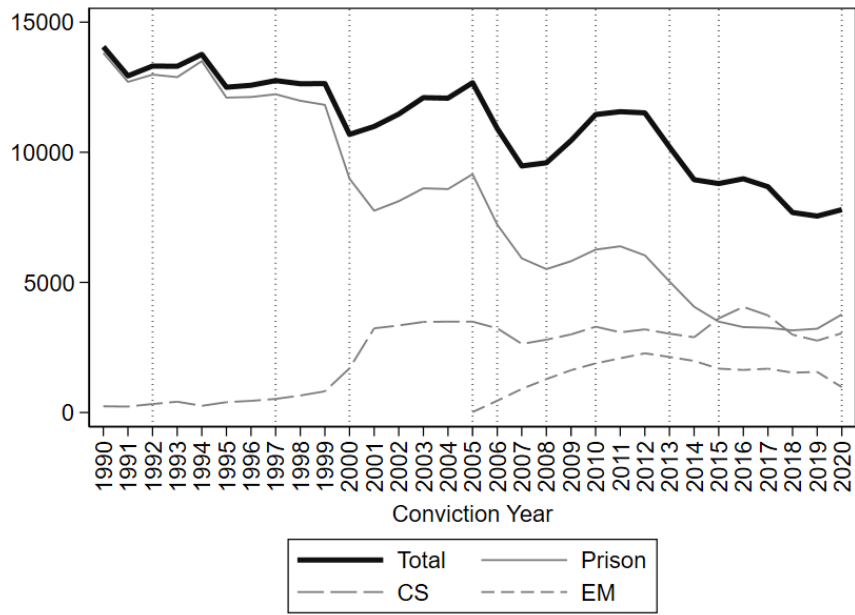
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Table 1. Overview of reforms to the CS and EM schemes in Denmark, 1990-2020.

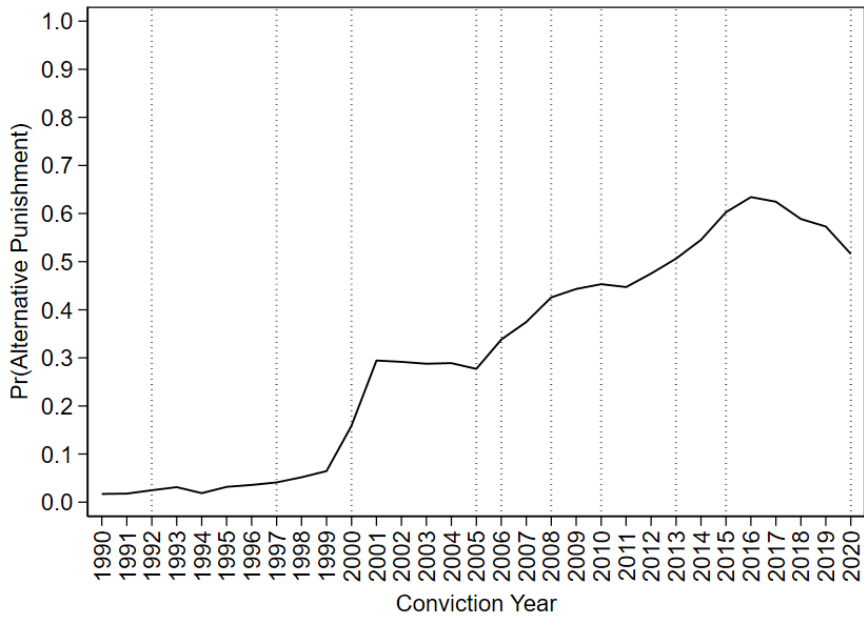
Date	Community service (CS)	Home confinement under electronic monitoring (EM)
April 1 1992	After a trial period, CS is formally approved as sanction alternative for prison sentences up to 12 months. Limited use in cases of violence, robbery, sex crimes, and drug crimes.	
May 1 1997	Suspended sentences with CS requirement 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. Sentence lengths max 3 months.
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 increases 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 serious bodily or psychological harm, and in aggravated assault cases).	

Notes: ^a The law was formally effective from July 1, 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 focus in the paper is on sentences of max one year.

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.



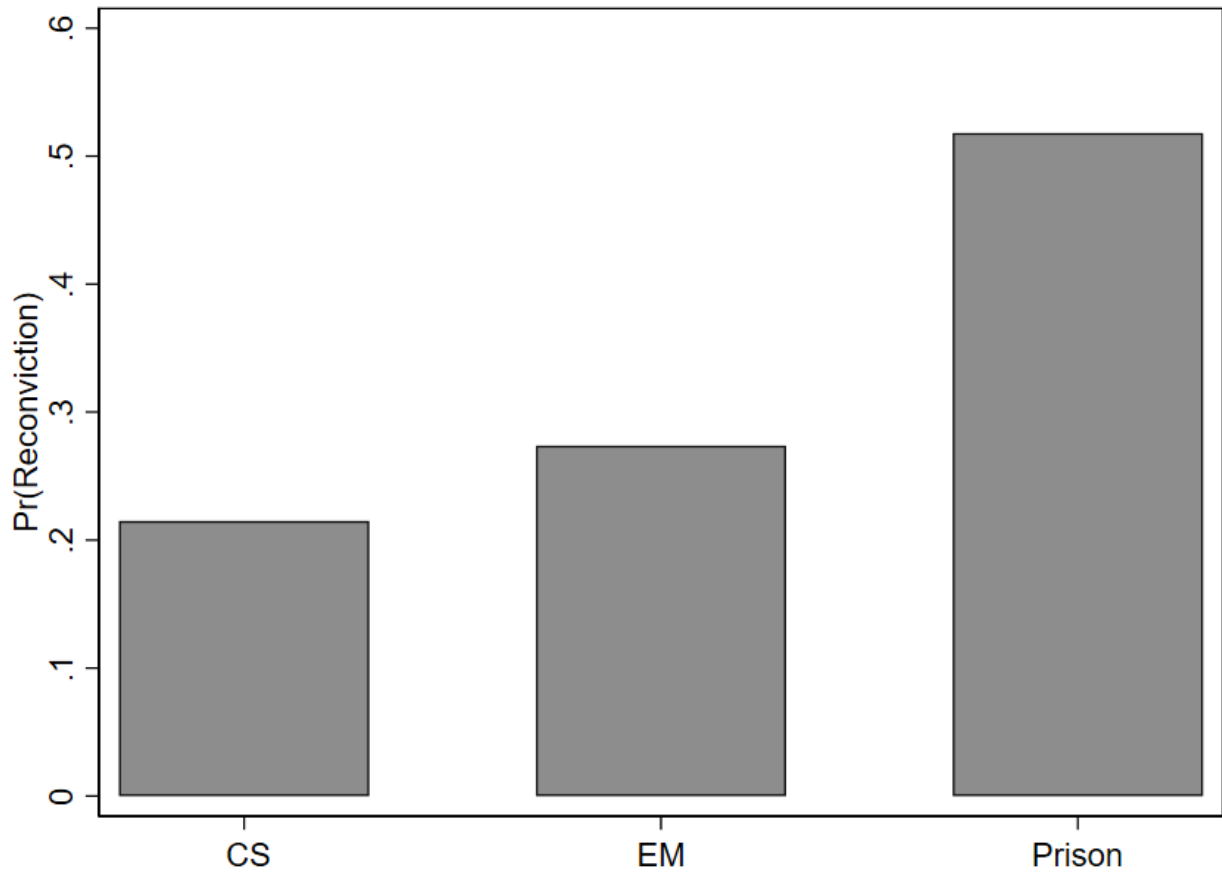
(a) Number of sentences



(b) Proportion of sentences

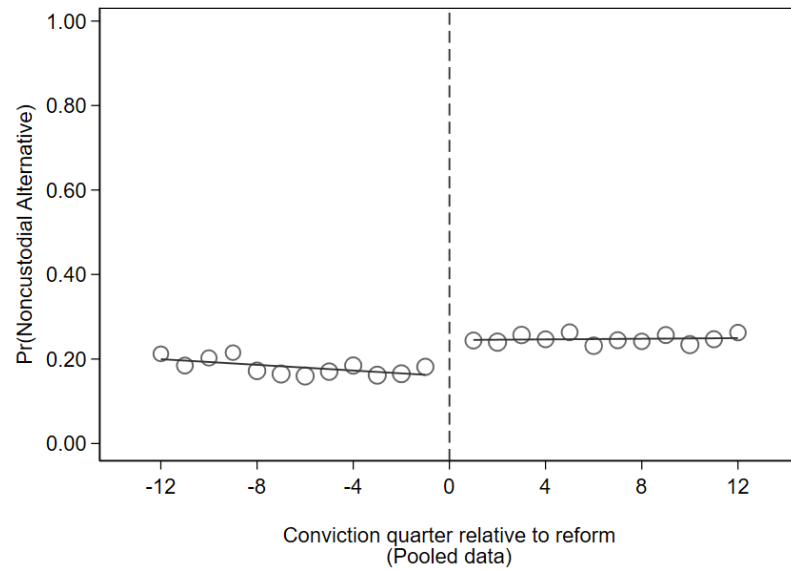
Notes. Dashed vertical lines denote years with policy reforms as listed in Table 1. “EM” reflects prison sentences where the person was allowed to serve at home under EM, and “Prison” excludes these sentences.

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

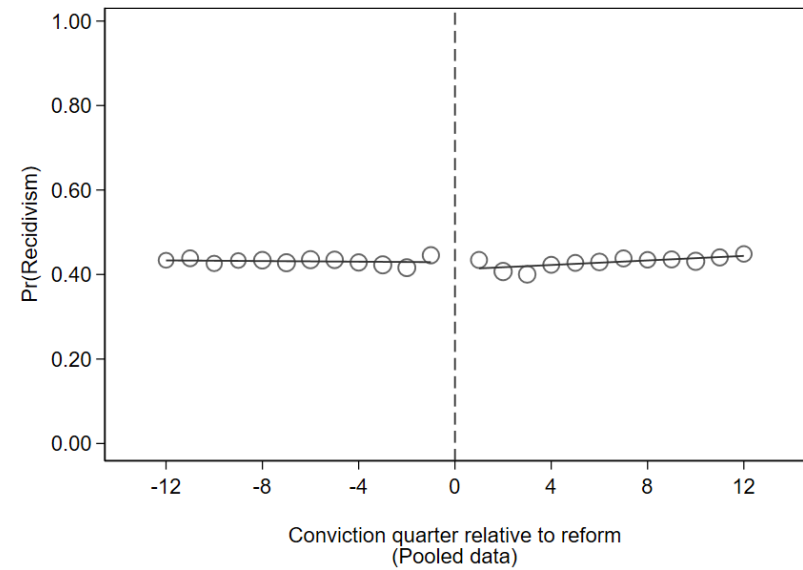


Notes: Data pooled across all sentences (shorter than one year or equivalent for CS) 1990-2020.

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



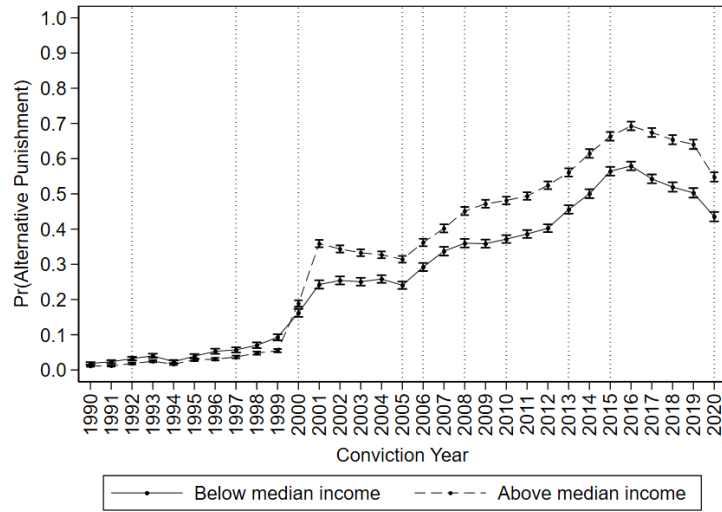
(a) Proportion of sentences served in non-custodial alternative



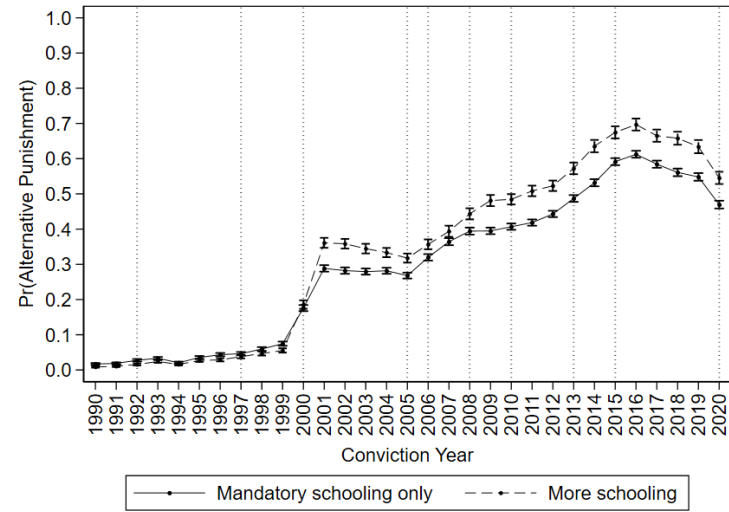
(b) Proportion convicted for new crime committed within 3 years

Notes. Data pooled across all reforms listed in Table 1 (except the reform in 2012 as affected only sentences longer than 1½ years).

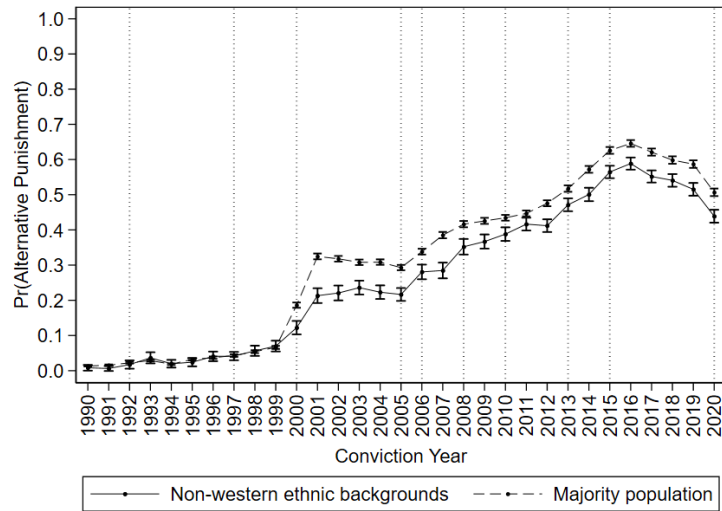
Figure 4. Proportion of sentences corresponding to maximum one year's imprisonment that was served in CS or EM, by stratifying variables.



(a) Prior income



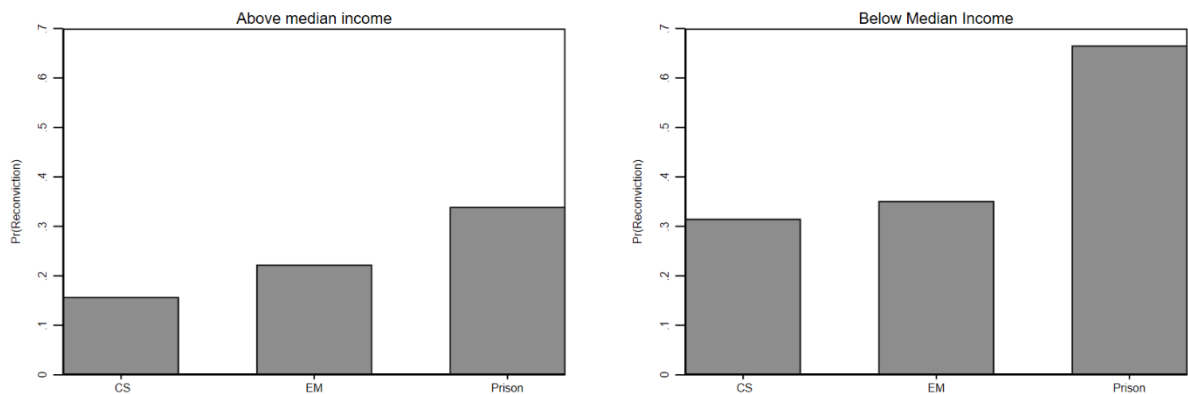
(b) Schooling



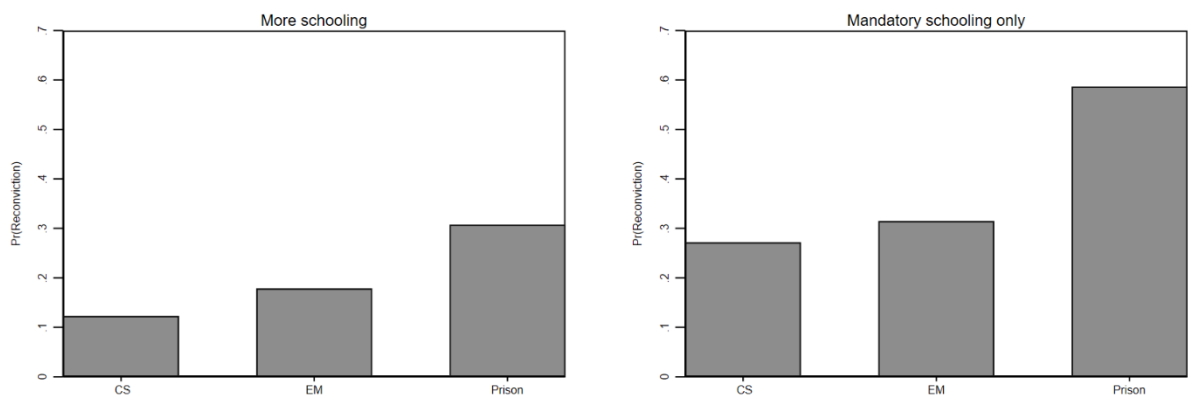
(c) Ethnic backgrounds

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

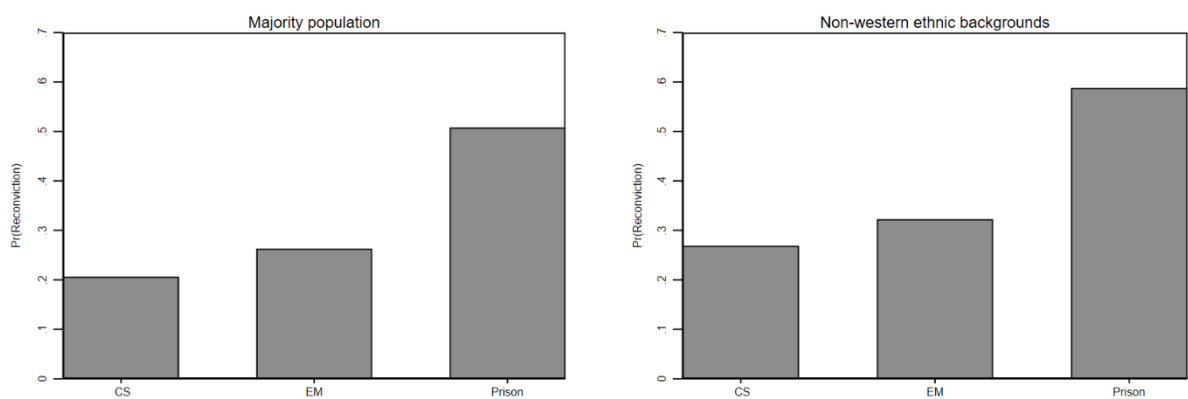
Figure 5. Cumulative proportion with recidivism 3 years after original conviction, by sentence type and by stratifying variables.



(a) Prior income



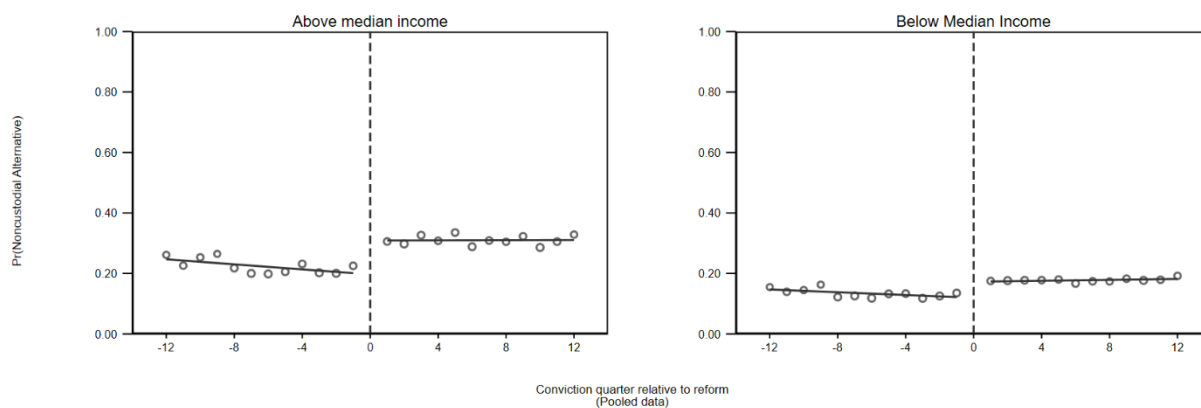
(b) Schooling



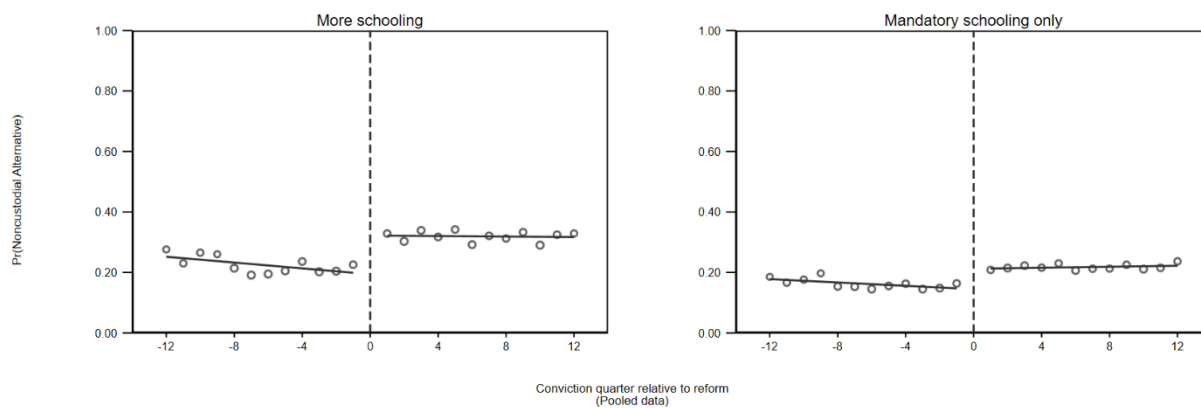
(c) Ethnic backgrounds

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

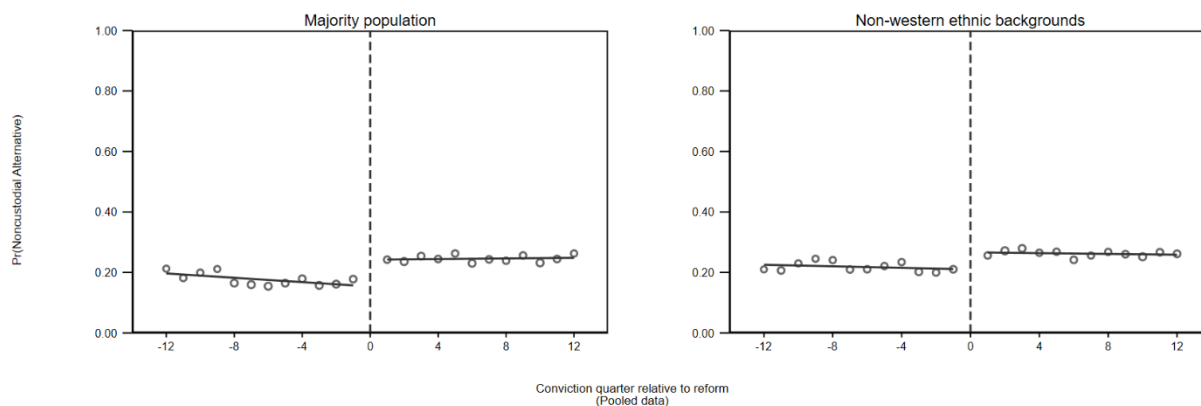
Figure 6. Proportion of relevant sentences served in non-custodial alternative (CS or EM) by conviction quarter before and after policy reforms, and by stratifying variables.



(a) Prior income



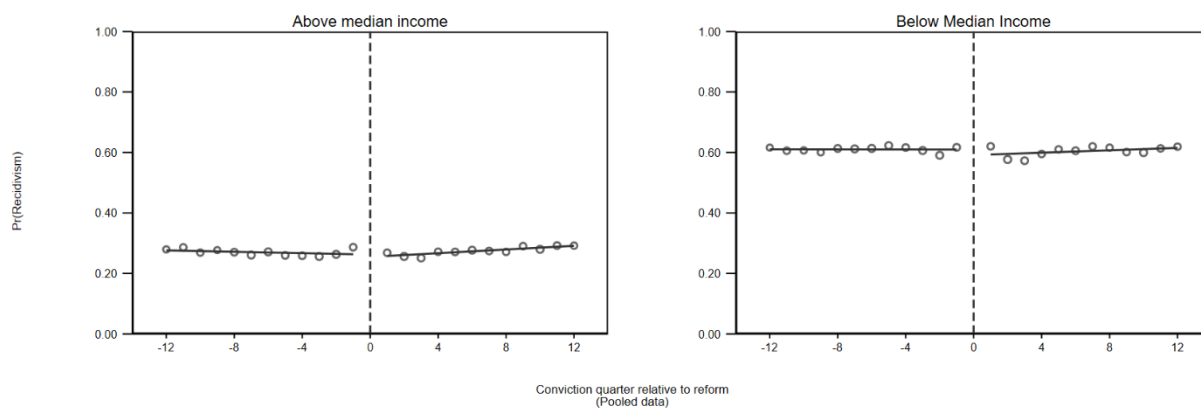
(b) Schooling



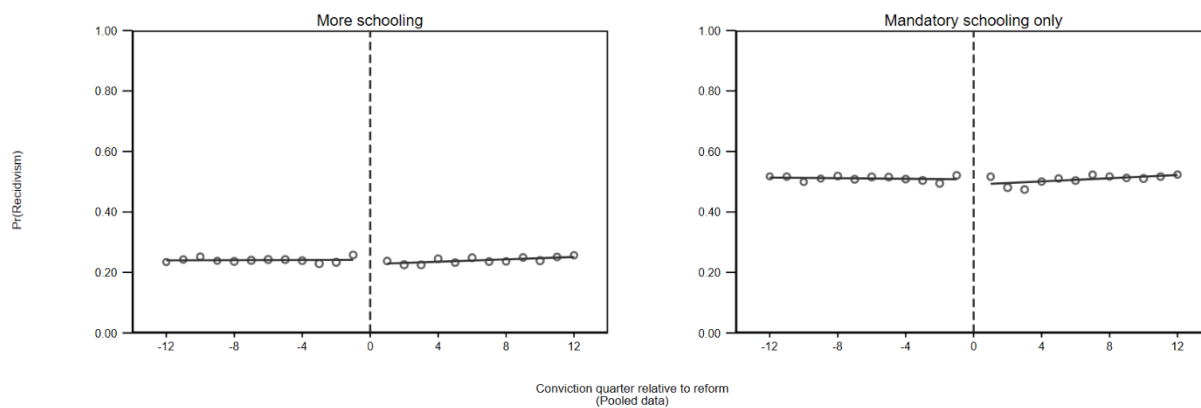
(c) Ethnic backgrounds

Notes. Data pooled across all reforms listed in Table 1 (except the reform in 2012 as affected only sentences longer than 1½ years).

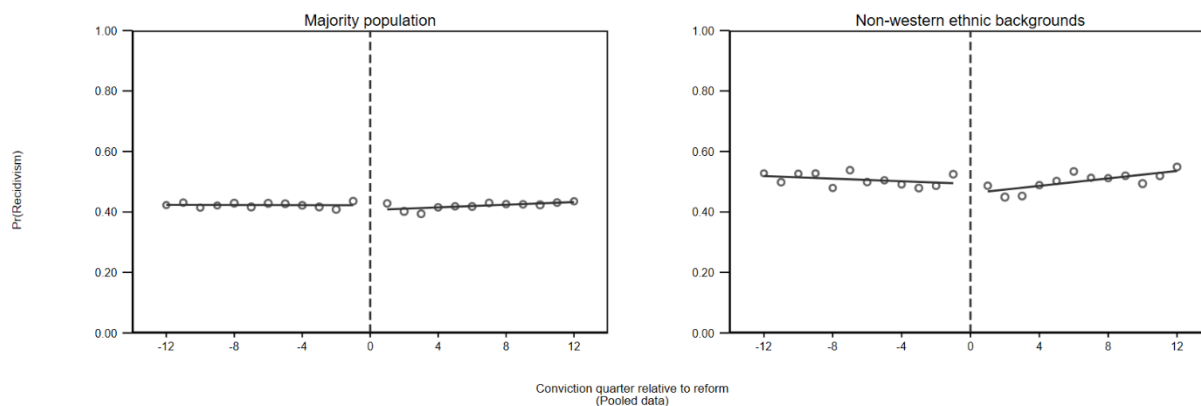
Figure 7. Proportion convicted of new crime committed within 3 years, by conviction quarter before and after policy reforms and by stratifying variables.



(a) Prior income



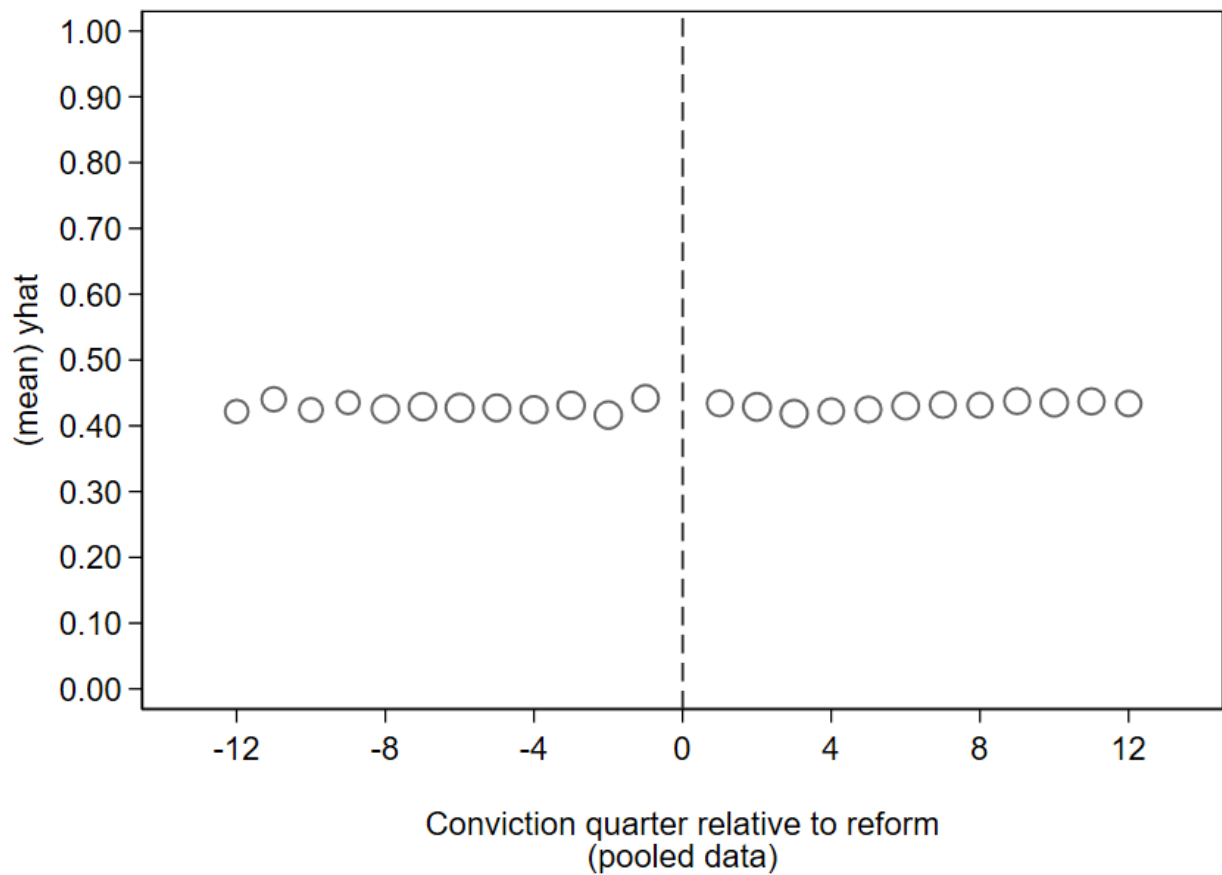
(b) Schooling



(c) Ethnic backgrounds

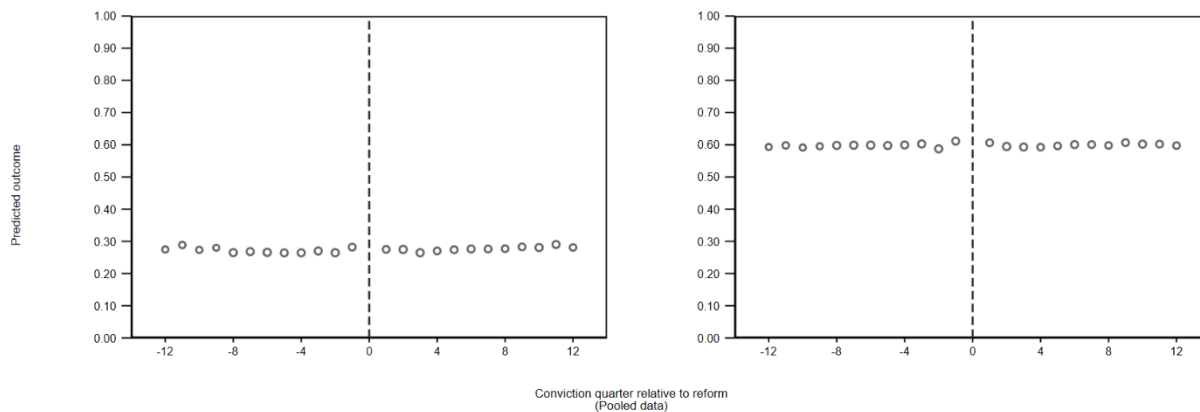
Notes. Data pooled across all reforms listed in Table 1 (except the reform in 2012 as affected only sentences longer than 1½ years).

Figure A1. Proportion convicted of new crime committed within 3 years as predicted from background variables, by conviction quarter before and after policy reforms.

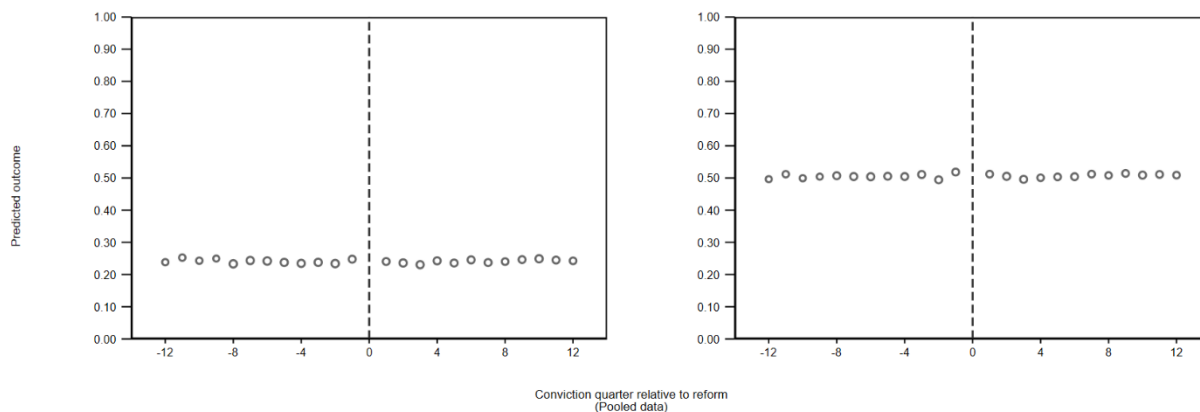


Notes. Data pooled across all reforms listed in Table 1 (except the reform in 2012 as affected only sentences longer than 1½ years).

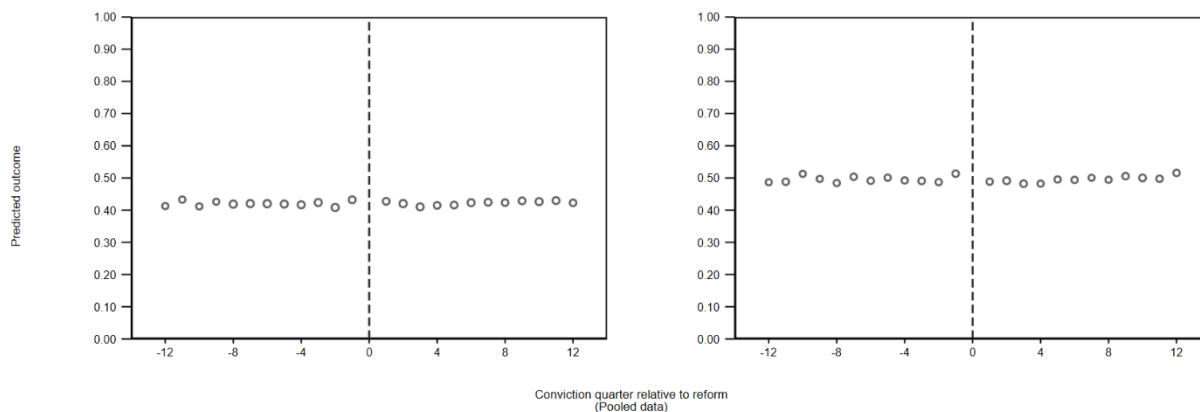
Figure A2. Proportion convicted of new crime committed within 3 years as predicted from background variables, by conviction quarter before and after policy reforms and by stratifying variables.



(a) Prior income



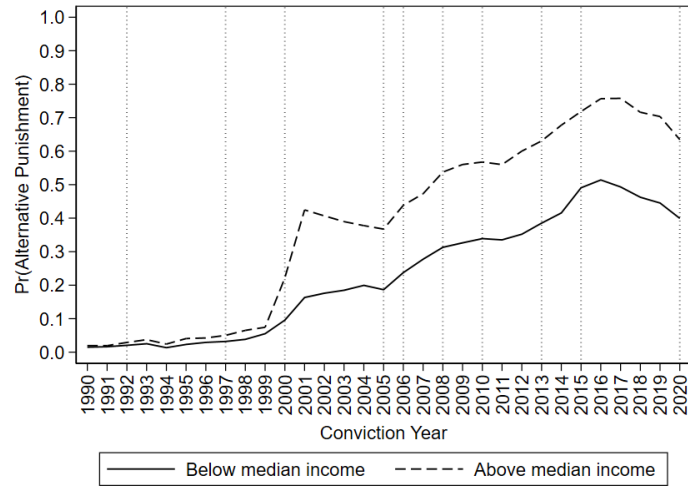
(b) Schooling



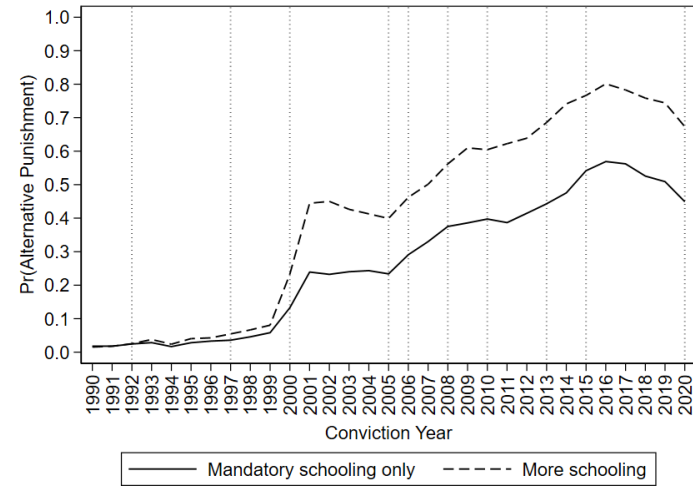
(c) Ethnic backgrounds

Notes. Data pooled across all reforms listed in Table 1 (except the reform in 2012 as affected only sentences longer than 1½ years).

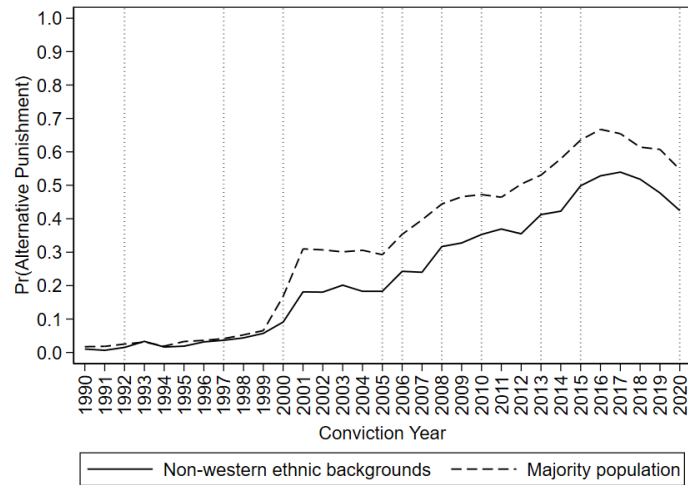
Figure A3. Proportion of sentences corresponding to maximum one year's imprisonment that was served in CS or EM, by stratifying variables, not controlled for crime type, sentence length, or criminal history.



(a) Prior income



(b) Schooling



(c) Ethnic backgrounds

Notes. Dashed vertical lines denote years with reforms as listed in Table 1.

Table A1. Results from regression-based tests of change in quarterly observation counts at time of reforms. Pooled data.

Dataset	Reform dummy D	(SE)	N
All	-1033.682	(863.542)	280,781
Above median income	-639.045	(413.927)	148,242
Below median income	-394.636	(458.358)	132,539
More than mandatory schooling	-279.030	(268.494)	82,496
Mandatory schooling only	-754.652	(602.890)	198,285
Majority population	-1013.712	(799.906)	251,663
Nonwestern backgrounds	-19.970	(92.373)	29,118

Notes. Table shows estimates from regressing the number of quarterly observations on a post-reform dummy D and controlling for the time trend in quarterly observation counts on each side of the reform.

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

Table A2. Results from McCrary tests of observation density around the reforms. Pooled data.

Dataset	Estimate	(SE)	N
All	-0.160***	(0.015)	280,781
Above median income	-0.197***	(0.021)	148,242
Below median income	-0.119***	(0.021)	132,539
More than mandatory schooling	-0.211***	(0.029)	82,496
Mandatory schooling only	-0.136***	(0.017)	198,285
Majority population	-0.178***	(0.016)	251,663
Non-western backgrounds	-0.047	(0.044)	29,118

Notes. Table shows estimates from McCrary tests of change in the observation density at the reform point.

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

Table A3. Results from unconditional balancing tests of covariates across the reforms. Pooled data.

Dataset	Female	Age	Sentence length	Property crime	Violent crime	Traffic offense	Prev. convictions	Previously incarcerated	Prior income below median	Mandatory schooling only	Nonwestern ethnic background	N
All	-0.001 (0.002)	0.231 (0.175)	-0.154 (0.959)	-0.015** (0.006)	0.028** (0.012)	-0.014 (0.016)	-0.044 (0.033)	0.003 (0.003)	-0.012** (0.005)	-0.002 (0.005)	0.007 (0.005)	280,781
Above median income	-0.002 (0.003)	0.092 (0.181)	0.706 (0.866)	-0.003 (0.005)	0.023* (0.012)	-0.028 (0.019)	-0.020 (0.021)	0.000 (0.006)	– –	0.007 (0.005)	0.007 (0.005)	148,242
Below median income	-0.002 (0.002)	0.208 (0.193)	-0.429 (1.221)	-0.023** (0.008)	0.035** (0.013)	-0.007 (0.011)	-0.029 (0.044)	0.011 (0.007)	– –	-0.005 (0.004)	0.009 (0.007)	132,539
More schooling	0.003 (0.003)	0.059 (0.216)	-0.198 (0.996)	-0.006 (0.006)	0.033* (0.016)	-0.036* (0.020)	-0.041** (0.015)	-0.006 (0.009)	0.000 (0.004)	– –	0.012** (0.005)	82,496
Mandatory schooling only	-0.003* (0.002)	0.281* (0.147)	-0.092 (1.165)	-0.019*** (0.006)	0.027** (0.011)	-0.006 (0.013)	-0.042 (0.039)	0.007 (0.005)	– 0.017*** (0.005)	– –	0.005 (0.006)	198,285
Majority Population	-0.001 (0.002)	0.248 (0.157)	-0.195 (1.017)	-0.014** (0.006)	0.027** (0.011)	-0.015 (0.015)	-0.035 (0.034)	0.007** (0.003)	-0.013** (0.005)	-0.001 (0.004)	– –	251,663
Nonwestern ethnic minority	-0.006 (0.005)	0.371 (0.285)	-1.045 (1.705)	-0.024* (0.013)	0.022 (0.022)	0.008 (0.010)	-0.136*** (0.046)	-0.021** (0.008)	-0.019 (0.012)	-0.019 (0.014)	– –	29,118

Notes. Table shows estimates from regressing each variable in the table header on a post-reform dummy and controlling for time trends prior to and following reform, by dataset shown in the first table column. Each reported estimate thus represents one regression model.

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

Table A4. Results from conditional balancing tests of covariates across the reforms. Pooled data.

Variable	All	Above median income	Below median income	More schooling	Mandatory schooling only	Majority population	Non-western ethnic backgrounds
Female	-0.001 (0.002)	-0.001 (0.002)	-0.000 (0.002)	0.004 (0.003)	-0.002 (0.002)	0.000 (0.002)	-0.011 (0.008)
Age	0.000* (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000** (0.000)	0.000* (0.000)	0.000 (0.000)
Sentence length	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000* (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Property crime	-0.002 (0.001)	-0.004 (0.003)	0.001 (0.002)	-0.004 (0.003)	-0.001 (0.001)	-0.002 (0.002)	-0.000 (0.003)
Violent crime	0.005 (0.003)	-0.000 (0.003)	0.011** * (0.004)	0.002 (0.004)	0.006** (0.003)	0.005* (0.003)	0.006 (0.006)
Traffic offense	-0.005 (0.004)	-0.009* (0.005)	-0.001 (0.004)	-0.012** (0.005)	-0.002 (0.004)	-0.005 (0.004)	0.006 (0.009)
Prior convictions	-0.001 (0.001)	-0.001** (0.001)	-0.000 (0.001)	-0.002*** (0.001)	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)
Previously Incarcerated	0.002** (0.001)	0.001 (0.001)	0.005** * (0.002)	0.001 (0.003)	0.003*** (0.001)	0.003*** (0.001)	-0.002 (0.002)
Below median Income	-0.002* (0.001)	- -	- -	0.001 (0.002)	-0.003** (0.001)	-0.003** (0.001)	-0.001 (0.004)
Mandatory schooling only	0.001 (0.001)	0.002 (0.001)	-0.001 (0.002)	- -	- -	0.001 (0.001)	-0.003 (0.004)
Non-western ethnic Background	0.003 (0.002)	0.004 (0.003)	0.003 (0.002)	0.006 (0.004)	0.002 (0.003)	- -	- -
N	280,781	148,242	132,539	82,496	198,285	251,663	29,118
p-value	0.010	0.085	0.021	0.007	0.003	0.008	0.1
F-test	3.135	1.978	2.758	3.444	4.036	3.351	1.891

Notes. Table shows estimates from regressing a post-reform dummy on the background variables in the first table column while controlling for time trends before and after reform. “F-test” and “p-value” refer to tests of

joint significance of excluding all control variables from the model.
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

Table A5. Results from regression discontinuity model of combined reforms' effect on serving in non-custodial alternatives to imprisonment and on criminal recidivism. Pooled data.

Dataset and Variable	Non-custodial alternative		Criminal recidivism		N
	Estimate	SE	Estimate	SE	
<i>All</i>					280,781
Reform	0.082***	(0.010)	-0.011	(0.007)	
Intercept	0.256***	(0.011)	0.139***	(0.005)	
<i>Above median income</i>					148,242
Reform	0.109***	(0.013)	-0.008	(0.006)	
Intercept	0.202***	(0.014)	0.131***	(0.006)	
<i>Below median income</i>					132,539
Reform	0.052***	(0.007)	-0.014	(0.010)	
Intercept	0.267***	(0.011)	0.202***	(0.006)	
<i>More than mandatory schooling</i>					82,496
Reform	0.119***	(0.015)	-0.009	(0.006)	
Intercept	0.215***	(0.014)	0.136***	(0.007)	
<i>Mandatory schooling only</i>					198,285
Reform	0.066***	(0.009)	-0.011	(0.008)	
Intercept	0.248***	(0.010)	0.182***	(0.004)	
<i>Majority population</i>					251,663
Reform	0.086***	(0.011)	-0.011	(0.006)	
Intercept	0.264***	(0.012)	0.138***	(0.004)	
<i>Nonwestern backgrounds</i>					29,118
Reform	0.044***	(0.010)	-0.011	(0.014)	
Intercept	0.218***	(0.016)	0.146***	(0.019)	

Notes: Table summarizes estimates of $\hat{\alpha}$ (intercept) and $\hat{\beta}$ (reform) from Equation 1 in the pooled data (“All”) and in each of the subsamples. All models controlled for gender, age, sentence length, crime type, prior convictions, and prior incarceration. Results in “All” also controlled for below median prior income, mandatory schooling only, and nonwestern backgrounds. Standard errors are clustered at the running variable (Z). Results only controlled for sentence length, crime type, prior convictions, and prior incarceration as well as results controlled for none of these background covariates are available from the author on request.

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