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Does sentence length affect the risk for criminal recidivism? A quasi-experimental study of three policy reforms in Sweden

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Abstract

Objectives This study examines the relationship between incarceration time and post-release recidivism among first-time incarcerated adult offenders.

Methods A quasi-experimental design was adopted consisting of three policy reforms that were treated as separate natural experiments. While holding imposed sentence length constant, these policy reforms either decreased or increased the required share of a sentence inmates needed to be incarcerated before being eligible for parole. Data consisted of large-scale administrative records containing all convictions for the Swedish cohorts born in 1958 and later.

Results Results indicate that neither increased nor decreased incarceration time had a statistically significant effect on post-release recidivism, irrespective of how recidivism was measured.

Conclusions Findings reveal little evidence for incarceration time having a criminogenic or specific preventive effect on post-release recidivism.

Keywords Incarceration length · Recidivism · Parole · Quasi-experiment

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Introduction

Sweden has long been perceived as being moderate in terms of penal attitudes, but over recent decades "tough on crime" policies have challenged this "Scandinavian exceptionalism" (Pratt, 2008; von Hofer & Tham, 2013). The themes that are currently prominent on the political agenda in Sweden indicate that a Swedish version of the "punitive turn" has emerged, which also has been expressed in multiple Swedish Government Official Reports (SOU), with examples including proposals for longer sentences for serious offenses and recidivism (SOU, 2021a; SOU, 2021b), harsher penalties for young adults (SOU, 2018), and the abolition or severe restriction of early release from prison (SOU, 2017). Consequently, the Swedish Prison and Probation Service predict a 40% expansion in prison capacity by 2030 (Kriminalvården, 2021b). This increasing trust among policymakers in the ability of incarceration and longer prison sentences to reduce crime has refocused the spotlight on the longstanding theoretical discussion on the criminogenic versus deterrent effects of prison.

Although the literature on the consequences of imprisonment is extensive, most research on the effects of incarceration on recidivism has analyzed the dichotomy of incarceration versus non-custodial sanctions (for systematic reviews on the effects of incarceration, see, e.g., Villettaz et al., 2015; Petrich et al. 2021). Research that explicitly addresses the effects of the length of incarceration is more limited, and those studies that do exist show inconsistent findings, making it difficult to draw any overall conclusions (Berger & Scheidegger, 2021; Nagin et al., 2009). A considerable amount of the research on the effects of incarceration times has been conducted in a US context, where sentencing lengths are at the higher end of the spectrum, which has subsequently led scholars to stress the importance of research based on European data (Durlauf & Nagin, 2011).

In this article, we analyze how the length of imprisonment affects recidivism for individuals who are incarcerated for the first time. We utilize three Swedish legislation reforms that may be treated as natural experiments. The reforms in question are the 1983, 1993, and 1999 parole reforms, which all either decreased or increased the amount of time an inmate was required to serve in prison prior to release on parole. The analyses are based on large-scale administrative data containing all convictions for the Swedish cohorts born in 1958 and later.

Theoretical background: from deterrent to criminogenic effects

Theories of punishment in general, and of imprisonment in particular, can be separated into two very different categories, from which two opposing hypotheses can be generated: one maintains that prison has crime deterrent effects, while the other predicts that prison has criminogenic effects.

Deterrence theory focuses on two main types of deterrence: *general* and *specific* deterrence, with incarceration playing a crucial role in relation to both

(Nagin, 1978). General deterrence can be defined as the crime preventive effect in the general public from the threat of a criminal sanction. Specific deterrence, on the other hand, focuses on the deterrent effect on the individual who experiences a sanction. Since the present study analyzes the expected deterrent effect of having experienced varying increases or decreases in sentence lengths, our focus is solely on specific deterrence. Within specific deterrence, three mechanisms can explain how incarceration may prevent recidivism. First, isolating an individual interrupts a criminal career, creating an *incapacitation effect* (Zimring & Hawkins, 1995). For an individual, however, the size of this effect decreases with time as a result of the well-known curvilinear relationship between age and crime (Hirschi & Gottfredson, 1983). Second, the experience of enduring a prison sanction might itself have a deterrent effect, preventing post-release recidivism due to concerns about being reincarcerated (Nagin et al., 2009; von Hirsch et al., 1999). Third, various correctional intervention programs may *rehabilitate* inmates and serve to inhibit an individual's criminal tendencies (Lipsey & Cullen, 2007). Many of these interventions require sentence lengths to be sufficiently long, and increasing sentence lengths may enable more inmates to participate in rehabilitative interventions (see for example Bhuller et al., 2020; Hjalmarsson & Lindquist, 2020). Since this study does not aim to estimate incapacitation effects, the mechanisms that may be present are rehabilitation and deterrence (for empirical evidence regarding incapacitation effects, see Miles & Ludwig, 2007; Piquero & Blumstein, 2007; Wermink et al., 2013). It should be noted, however, that we do not have access to data that would allow us to distinguish rehabilitative from deterrent effects.

Another set of theories suggests that a prison sentence may have (unintended) criminogenic effects. One strand of these theories describes how prisons are "schools of crime" where criminal skills are exchanged and learned within close-knit groups of individuals (see Bayer et al., 2009; Nygaard Andersen, 2019; Roxell, 2016). Adapting to prison conditions may therefore involve a normative and collective process among inmates who become socialized into embracing deviant attitudes. Inmates who are imprisoned for longer periods may potentially become even more entrenched in their criminogenic attitudes and exposed to wider anti-social networks.

Being subjected to prison may also result in a societal reaction in the form of labeling and stigmatization (Becker, 1963; Braithwaite, 1989). The mechanisms behind possible criminogenic effects of this type of labeling are twofold: First, treating and labeling an individual as a "criminal" has consequences for the self-image of the offender, who risks internalizing the criminal identity and subsequently acting in ways that are in line with this identity. Increasing sentence lengths could, accordingly, result in an even stronger internalization of the criminal identity, thus affecting post-release recidivism risks (see Harris, 1975). Furthermore, it has been suggested that an individual is more susceptible to labeling and stigmatization at the beginning of a criminal career and that the first experience of incarceration is more likely to result in such consequences than reincarceration (Motz et al., 2020; Walters, 2003). Second, society's collective discomfort with offenders may limit work opportunities for former inmates (e.g., Apel & Sweeten, 2010; Bäckman et al., 2018). Extensive periods of incarceration could exacerbate this effect, as ties to conventional society are further diminished, which may result in weaker social bonds both to individuals (e.g., family, friends, co-workers) and institutions (e.g., workplaces and organizations) that could otherwise potentially prevent the individual from recidivating (Sampson & Laub, 1997).

Lastly, there is a third strand of literature that argues that incarceration length has a minimal effect on offenders' post-release recidivism risks (Gendreau et al., 1999). In this line of research, recidivism risks are instead explained by various background characteristics and pre-incarceration risk factors. Because inmates are a highly selected group characterized by addiction problems and resource deficiencies in areas such as education, employment, and health (Nilsson, 2003), deterrent interventions may therefore not have the desired effect on recidivism (Bäckman et al., 2018).

The relationship between incarceration length and recidivism

In this section, we review the literature on the effects of incarceration length and pay particular attention to more recent quasi-experimental studies. Broadly, these studies utilize natural experiments or various matching designs to identify the relationship between incarceration length on recidivism.

A systematic review by Nagin et al. (2009) has played an important role regarding the approaches employed in more recent studies estimating the effects of incarceration length. In this review, the authors concluded that the bulk of the pre-existing literature suffered from serious methodological shortcomings. Large parts of this literature, which had been dominated by regression-based studies, suffered from issues regarding selection bias and limitations regarding the interpretation of causality. Moreover, the outcomes from these studies were remarkably heterogeneous and the authors, therefore, refrained from drawing any overall conclusions regarding the then available research. Following this review, and in line with a general discussion regarding the "causal revolution" (Sampson et al., 2013), increasing focus has been directed at the use of quasi-experimental evidence in discussions of the causal impact of incarceration length.

The first of more recent quasi-experimental studies on the effects of incarceration length was conducted by Loughran et al. (2009), who used propensity score matching to enable comparisons between juvenile offenders from two US counties who had been sentenced to varying prison terms. The results indicated no effect of incarceration length on either re-arrest rates or self-reported offending during a 2-year follow-up. Snodgrass et al. (2011) used propensity score matching with data on Dutch offenders between 12 and 40 years of age at sentencing. The authors found little evidence of a relationship between the length of prison stays and 3-year reconviction rates. Also utilizing the propensity score methodology on Dutch data, but only for adult offenders, Wermink et al. (2018) studied the short-term effects (6-month follow-up period) of sentences that were on average 4.1 months in length, and found no effects on reoffending, reconviction, or reincarceration. Using data on individuals released from Florida prisons, and using matching techniques, Mears et al. (2016)

found that longer periods in prison were initially associated with a greater risk for reconviction, but that these effects disappeared approximately 2 years after release, underscoring the importance of longer follow-up periods, since effects may dissipate over time.

Meade et al. (2013) utilized propensity score matching with data on individuals released under post-release supervision in Ohio and compared offenders that were differentiated in terms of sentence length and found no effect on the odds for rearrest during a 1-year follow-up period for offenders who had served less than 5 years. Although the authors found a small effect for offenders who had been sentenced to 5 years or more, the mechanism behind this effect was unclear, and the authors discuss that it might be due to maturation. Using parametric survival models on a dataset comprising inmates from several US states, Rydberg and Clark (2016) replicated parts of the study by Meade and colleagues. For those serving long sentences, exceeding 4 years, they found that increasing incarceration lengths reduced reconviction risks. Increased incarceration length was, however, associated with an increased risk for reincarceration due to technical violations. Since their results were heterogeneous with respect to the type of recidivism measure and crime type, the authors refrained from drawing any firm conclusions. Roach and Schanzenbach (2015) made use of the randomization of offenders to judges that occurs within a courthouse in Seattle. A two-stage least square regression analysis revealed that for each additional month incarcerated, reconviction rates decreased by 1%. This estimate was robust to the length of follow-up periods (1, 2, or 3 years). On average, the sentence lengths among inmates were relatively short (median 3 months), however, and the study was limited to offenders who had entered a guilty plea, which may have introduced some selection bias. Rhodes et al. (2018) exploited the quasi-experimental setting created by the US Sentencing Guidelines and employed an instrumental variable approach, finding that an average increase of 7.5 months in the length of incarceration reduced the 3-year reincarceration rate from 20% to approximately 19%. Because of this small impact, the authors concluded that small reductions in average incarceration lengths are possible with only minimal effects on recidivism.

Tollenaar et al. (2014) analyzed a Dutch policy reform that increased the length of incarceration for high-frequency offenders. Utilizing propensity score matching, the authors found that increasing sentence lengths for highly active offenders reduced 2-year reconviction rates by between 12 and 16%. Because offenders who had been subjected to the reform were a very problematic group characterized by addiction, unemployment, and mental health problems, the authors argued that correctional rehabilitation interventions might be a potential mechanism underlying the observed effects.

In the Scandinavian context, we are only aware of one study on the effect of incarceration length on crime (Hjalmarsson & Lindquist, 2020). The study's primary focus was not, however, directed at recidivism, but rather at health outcomes. Hjalmarsson and Lindquist's paper is of particular relevance for the present study since they too exploited the 1993 and 1999 parole reforms in Sweden. Their analyses revealed health-promotive effects of an increase in incarceration length. Regarding recidivism, the authors found that the increase in the length of incarceration produced by the reforms on average decreased recidivism. These effects were strongest for reincarceration within 12 months, which decreased by 2.9 percentage points, and for the prevalence of two or more reconvictions within 36 months, which decreased by 2.5 percentage points.[1]

A striking conclusion with regard to the more recent quasi-experimental studies is that although research designs have improved, the overall evidence remains somewhat unclear, with some studies yielding null effects and others pointing to the existence of a minor deterrent effect. Further, there are also issues regarding the generalizability in recent studies. Not only have few studies been conducted outside the USA, but the findings are also often based on specific populations of offenders, such as juveniles or inmates sentenced within a specific court system. Furthermore, quasi-experimental approaches often estimate local treatment effects at the threshold. Utilizing local estimates does increase the possibility of identifying a causal relationship, but it may also result in generalization difficulties because offenders at the threshold constitute a specific offender population. These limitations highlight the need for further studies on the effects of incarceration length across a range of contexts and groups of offenders.

The Swedish parole institution and the background to the natural experiments

In this study, we utilize three distinct reforms from 1983, 1993, and 1999, which changed the legislation concerning the required share of a sentence inmates needed to be incarcerated before being eligible for parole. Prior to 1983, inmates were eligible for discretionary parole, with the law requiring an inmate to have served two-thirds of the sentence prior to parole, although under special circumstances parole could be granted after half the sentence. In practice, discretionary parole was based on sentence length. Long-term inmates who had been sentenced to more than 24 months were eligible for parole after half their sentence. Although inmates serving between 2 and 24 months could be eligible for parole after half their prison sentence, hardly anyone serving a sentence of 2 to 12 months was released after half the sentence, but rather after two-thirds (SOU, 1981).[2] Parole was granted at some point between half and two-thirds of the sentence for inmates serving 13 to 24 months.

On July 1, 1983, the "half-time reform" was enacted, which replaced discretionary parole and introduced mandatory release on parole after serving half the prison sentence for inmates who had been sentenced to 4 months or more (proposition 1982/83:85). This affected all individuals who were either already incarcerated on

[1] Although both our own and Hjalmarsson's and Lindqvist's study employ Swedish parole reforms to study the effects of variations in the length of incarceration, the studies differ in several respects. Besides the fact that our study focuses on recidivism, we (i) utilize natural experiments that both decreased and increased incarceration time, (ii) have access to individuals' full criminal record histories, (iii) evaluate the long-term effect of incarceration length, and (iv) direct particular attention at individuals receiving a prison sentence for the first time. See the section on data, sample, and measures for further information.

[2] Inmates were required to spend at least 2 months in prison, which means that inmates sentenced to less than 3 months often spent more than two-thirds of their sentence incarcerated.

the date of the reform's introduction, or who were incarcerated thereafter. In addition to a decrease in the required incarceration time prior to parole, the length of the period of post-release supervision was also reduced, from 1–3 years to 1 year. One important rationale for the implementation of mandatory parole was the legal uncertainty created by the discretionary parole system that had been in place prior to 1983.

The reform became the subject of substantial criticism and was repealed on July 1, 1993 (proposition 1992/93:4). All inmates sentenced to between 4 and 23 months were subsequently required to serve two-thirds of their prison sentence prior to release on parole. For long-term inmates serving 24 months or more, parole was still granted after half the prison sentence. In order to avoid threshold effects in the implementation of a system involving parole after both half and two-thirds of the sentence, a graduation scale was implemented for sentences of 13 to 23 months (proposition 1992/93:4).[3]

On January 1, 1999, the parole legislation was changed once again (proposition 1997/98:96). Long-term inmates serving 24 months or more were subsequently subject to the same parole rules as short-term inmates. All those serving sentences of more than 1 month were now eligible for parole after two-thirds of their sentence. Unlike the 1983 reform, the 1993 and 1999 parole legislations were applied to offenders based on whether the date of their conviction came before or after the date of the reforms' introduction, and the extent of post-release supervision was, likewise, not changed by either the 1993 or 1999 reform and was set at 12 months regardless of the sentence length.

Figure 1 illustrates the expected change in required incarceration time before parole. For example, individuals sentenced to 12 months in prison for crimes committed after the 1993 reform are required to spend 2 additional months in prison (8 months in total) in comparison to individuals who are sentenced to 12 months during the half-time reform (6 months in total). Note that their imposed sentence lengths of 12 months are identical. The only difference is the amount of required incarceration time before parole.

The fact that parole has sometimes been discretionary and sometimes mandatory introduces some uncertainty with respect to how the reforms actually played out in terms of required prison time before parole. Using prison data, Hjalmarsson and Lindquist (2020) were, however, able to show that the 1993 and 1999 reforms both were implemented as intended and that required incarceration time before parole was increased from half-time to two-thirds.

The parole reforms of 1983, 1993, and 1999 are here treated as natural experiments in which the post-reform group has been "treated" with an either increase or decrease in the length of incarceration, while the pre-reform group acts as a control group. The study design is presented in Fig. 2 and is described further in our data, sample, and measures section.

[3] The scale employed was 8 months plus one-third of the sentence length that exceeded 12 months. For example, an inmate sentenced to 15 months was released on parole after 8 months plus 1 month.

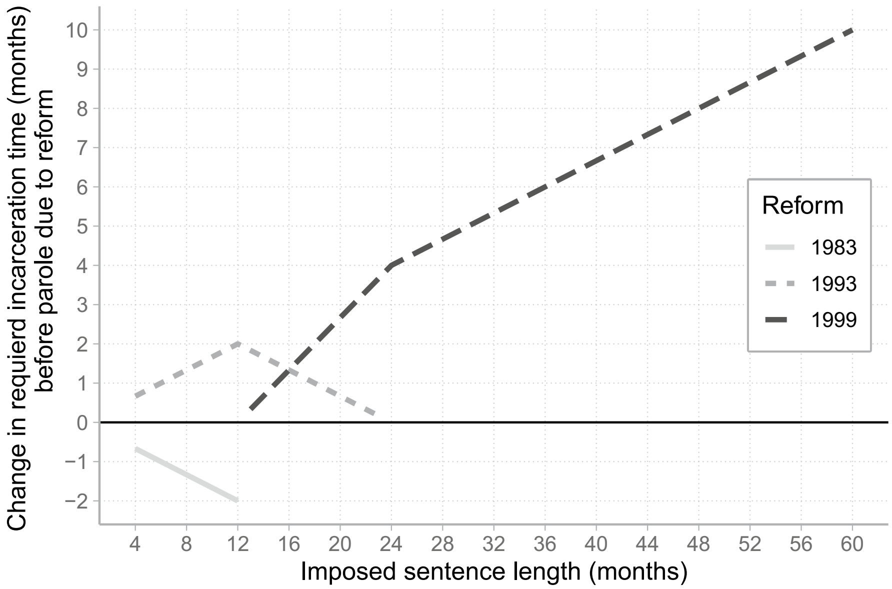

Fig. 1 The 1983, 1993, and 1999 parole reforms' effect on required time served before being eligible for parole. Notes: each reform effect should be read as the change (in months) when going from prior parole law. On July 1, 1983, parole laws were changed so that offenders sentenced to 2–24 months in prison were required to serve at least half the sentence before being eligible for parole, from previously two-thirds of the sentence. Parole laws were then again changed on July 1, 1993, requiring offenders sentenced to 4–23 months in prison to serve two-thirds of the sentence. On January 1, 1999, a reform was enacted that targeted inmates sentenced to more than 24 months in prison that required them to serve two-thirds of the sentence before being eligible for parole, from previously half of the sentence

Data, samples, and measures

Data

The study data were drawn from the convictions register maintained by the Swedish National Council for Crime Prevention (BRÅ), and cover the period 1973 to 2017. [4] This means that we have the entire conviction histories of all cohorts born after 1957 (the age of criminal responsibility is 15 in Sweden). From this register, we have extracted data on the date of the offense, the date of conviction, offense type, sanction type, and sentence length (in days). In order to censor the dataset, we have also collected data on dates of death and emigration

[4] This study was preregistered at Open Science. See https://osf.io/br875. The linking of the various registers and the anonymization of the dataset have been carried out by Statistics Sweden (SCB). The dataset is stored on, and has been analyzed via, the system used by Statistics Sweden to make microdata available for online research (MONA). Due to the Swedish Public Access to Information and Secrecy Act, microdata cannot be made publicly available.

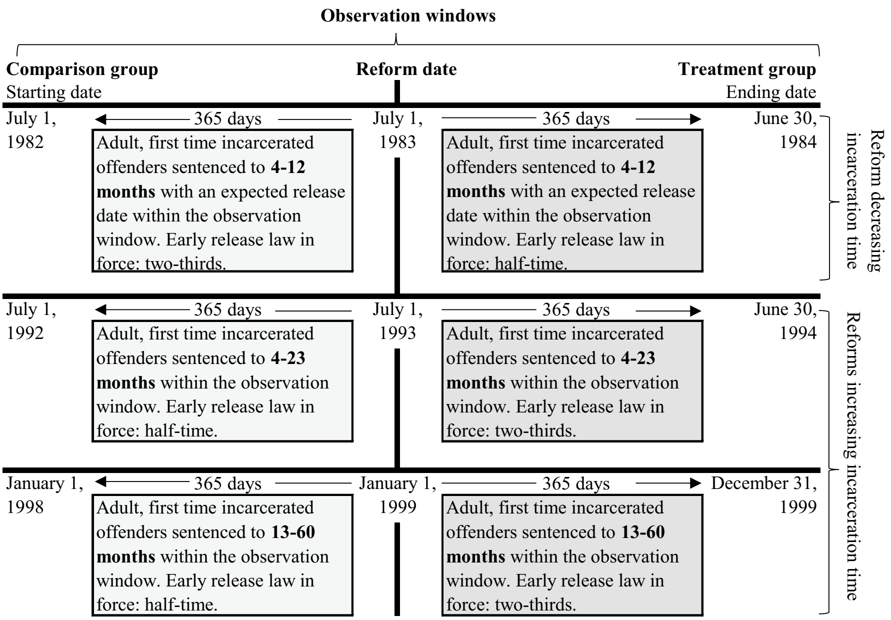

Fig. 2 Inclusion criteria and study design relative to the 1983, 1993, and 1999 parole reforms

from the Total Population Register (RTB) maintained by Statistics Sweden (SCB).[5]

Our attention is directed at offenders who have been imprisoned for the first time. For these individuals, who tend to be younger, incarceration may be more consequential than it is for individuals with multiple experiences of imprisonment (Baćak et al., 2019; Nieuwbeerta et al., 2009). Focusing on first-time incarcerated individuals also enables us to circumvent the feedback effect between imprisonment and crime. Because we are interested in isolating the effect of incarceration length, excluding the potential influence of previous exposure to incarceration is beneficial.

Analytic samples

Three different reform samples were created (see Fig. 2), one for each reform year. The reform samples were selected on the basis of five inclusion criteria. First, for the 1993 and 1999 reforms, individuals *convicted* within 12 months after the reform (July 1, 1993, and January 1, 1999) were assigned to the treatment group, while individuals sentenced during the 12 months prior to the reform were assigned to the control group. Because the 1983 reform affected all individuals

[5] We are able to link data from these registers by using the Swedish personal identification number, which is based on the individual's date of birth and an additional four-digit identification number. Statistics Sweden produces an anonymized pseudo-key to avoid the identification of specific individuals.

who were incarcerated on the day the reform was introduced — which was not the case for the subsequent reforms — the inclusion criteria are slightly different for the 1983 reform sample. Here the treatment group consists of individuals who had an expected *release date* within 12 months after the reform (July 1, 1983), while the control group comprises individuals who had an expected release date during the 12 months prior to the reform. Since our data consist of offenders in the cohorts born after 1957, our design entails certain age restrictions. This is most apparent for the 1983 reform, where the maximum age is restricted to 25 years old (i.e., those born in 1958). Second, because of the way in which the 1983 reform was implemented, offenders who were convicted prior to the reform but who had a release date subsequent to the reform were released after serving somewhere between half and two-thirds of their sentence. We exclude these offenders due to the risk for measurement error in their exposure to treatment. Third, each reform impacted specific sentence lengths in a way that varied from reform to reform, and we have therefore only included the following sentence lengths: The 1983 reform allows for estimates of the effect of a decrease in incarceration time (on average, an expected 36-day decrease) for sentences of 4 to 12 months (n = 654); the 1993 reform allows for estimates of the effect of an increase in incarceration time (on average, an expected 36-day increase) for sentences of 4 to 23 months (n = 1,688); the 1999 reform allows for estimates of the effect of an increase in incarceration time (on average, an expected 115-day increase) for sentences of between 12 and 60 months (n = 637). The upper limit of 60 months has been imposed because there are too few observations above this threshold. Fourth, the offender had no prior prison sentences (but could have a prior conviction history involving non-custodial sanctions). Fifth, the offender was not below the age of 18 at the time of the offense. In Sweden, offenders under the age of 18 are in general not sentenced to prison but rather to institutional care outside the prison system.

Outcome variables

We measure three outcome variables: *reconviction*, *reincarceration*, and *recidivism frequency*, with time at risk starting from the *expected* release date and capped at 10 years. We calculate the expected release date by taking the date on which a conviction comes into force, and adding the length of the imposed prison sentence with parole subtracted. As a result of time spent in pre-trial detention, this will not yield perfect estimates, but there is no reason to expect the potential errors, due to this factor, to vary between treatment and control groups. *Reconviction* measures whether an individual has been reconvicted for a post-release offense, *reincarceration* measures whether an individual has been convicted of an offense that led to a new prison sentence, and *recidivism frequency* is a count variable that measures the total number of offenses included in post-release convictions. Descriptive statistics for all variables are included in Table 1.

Table 1 Comparison between treatment and control group on background characteristics in the three reform samples. $N = 3002$

	1983 reform			1993 reform			1999 reform		
	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.
First prison sanction									
Age at crime	21.19	21.55	0.36**	24.28	24.87	0.6**	25.82	25.80	-0.01
Imposed sentence lengths (days)	214.47	210.28	−4.19	260.38	264.82	4.44	740.05	786.48	46.43
Criminal history up to and including first prison sanction									
Conviction frequency	5.52	5.48	−0.04	4.27	4.20	−0.07	3.68	3.56	-0.12
Crime frequency	23.05	22.32	−0.73	14.98	14.68	−0.30	12.42	12.74	0.33
Age of onset	17.18	17.52	0.34	18.67	18.92	0.25	19.34	19.04	-0.31
Crime mix (frequency)									
Violent	1.35	1.23	−0.12	1.87	2.13	0.27*	1.94	2.58	0.64*
Sex	0.03	0.03	0.00	0.10	0.11	0.01	0.19	0.23	0.04
Property	12.70	11.60	−1.10	6.26	5.76	−0.50	4.41	4.01	-0.40
Fraud	1.18	1.26	0.09	1.35	1.23	−0.12	1.03	0.89	-0.14
Vandalism	1.06	0.78	−0.28	0.63	0.71	0.08	0.53	0.47	-0.06
Traffic	3.93	4.61	0.68	2.31	2.48	0.17	1.69	1.64	-0.05
Narcotics	1.20	1.29	0.09	0.70	0.68	−0.03	1.01	1.48	0.46*
Demographic background									
Female	0.05	0.06	0.01	0.07	0.08	0.01	0.08	0.07	-0.01
Born in Sweden	0.89	0.91	0.02	0.86	0.87	0.01	0.82	0.82	0.00
Censoring									

Table 1 (continued)

	1983 reform			1993 reform			1999 reform		
	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.
Age at follow-up	49.80	50.51	0.71	46.36	46.32	−0.04	43.33	42.34	-0.99
Deceased in 2017	0.30	0.28	−0.02	0.13	0.11	−0.02	0.09	0.12	0.03
Age at decease	41.97	40.50	−1.47	38.51	37.48	−1.02	34.94	37.82	2.88
N	322	334		884	815		313	334	

Notes: * $p < .05$, ** $p < .01$

Independent variables

A dummy variable has been constructed indicating whether the offender was exposed to a reform or was subject to the parole legislation in force prior to the reform. This variable is used to estimate the reduced-form effect of being "treated" by a parole reform.

The continuous variable measuring imposed sentence length was transformed into a categorized ordinal variable, with sentence lengths being categorized differently for each of the three reforms. The following categories were created to include a sufficient number of offenders in each: for the 1983 reform, 4–5 months (n = 375), and 6–12 months (n = 279); for the 1993 reform, 4–5 months (n = 785), 6–12 months (n = 645), and 13–23 months (n = 258); for the 1999 reform, 13–23 months (n = 314), 24–36 months (n = 197), and 37–60 months (n = 126).

In the adjusted models, we control for the following criminal justice variables: age and age squared at first conviction, prior conviction frequency, prior crime frequency by offense type (violent, sex, property, fraud, vandalism, traffic, narcotics), number of prison days imposed for the first prison sanction, conviction month, and age at the time of the offense that resulted in the first prison sentence. In addition, we include the following demographic controls: sex and whether or not the offender was born in Sweden.

Analytical strategy

We employ event history analysis to analyze recidivism measured in terms of reconviction or reincarceration, and each reform is treated separately. Event history analysis allows for an estimation of the time it takes for a criminal event to occur measured from a given "at-risk" starting point (see, e.g., DeJong, 1997; Sivertsson, 2016). In essence, the length of time between two criminal events is used to estimate the hazard for recidivism, which is assumed to measure the strength of recidivism tendencies (Allison, 2014). This approach handles right-censored data with ease, which is particularly useful when using long follow-up periods. In the current study, we analyze recidivism over a 10-year period. The data are right-censored at the time of emigration, death, or the end of the period of time at risk. We utilize the precision provided by daily information on convictions, and analyze reconviction in a continuous time, where the time to reconviction is calculated as the number of days between the date of expected release from prison and a new conviction.

We estimate non-parametric Kaplan-Meier cumulative probability functions to illustrate "the speed" of recidivism over the follow-up period between the treatment and control groups in a bivariate fashion, and we employ Cox proportional hazard regression to model the association between reform exposure and recidivism. We furthermore employ Cox proportional hazard regression in relation to different subsets of categorized sentence lengths. This stratification by imposed sentence length enables us both to analyze the groups that were most impacted by the reforms in terms of increases/decreases in incarceration length prior to parole, and also whether incremental adjustments in incarceration length accelerate or decelerate recidivism timing.

Further, we use negative binomial regression to estimate the recidivism frequency.[6] Negative binomial regression is preferred over Poisson regression for our data structure, since a likelihood-ratio chi-square test indicated that the dependent variable is over-dispersed (Osgood, 2000). In contrast to the Cox-regression analyses, the negative binomial model requires that the offender had been alive and had not emigrated throughout the follow-up period.

Reform implementation and methodological considerations

Because the conviction date and not the date of the offense determined whether an individual was affected by the 1993 and 1999 reforms, the possibility of offender "self-selection" is not a particular concern in relation to the effects of these reforms. This is even less of a concern in relation to the 1983 reform, since in this case the reform was not implemented on the basis of individuals' offense or conviction dates (see the earlier section on the Swedish parole institution). However, judges might hypothetically hasten or delay a conviction in order to ensure that an offender was convicted on one side of the reform date or the other. Appendix Fig. 5b and e do indeed show that fewer conviction decisions were made during the days following the 1993 and 1999 reforms, which might indicate a preference for allowing offenders to receive half-time parole rather than two-thirds parole. However, when comparing the reform years with the surrounding years, we do not find any deviating patterns (see Appendix Fig. 5). Instead, the sorting that does seem to occur is not because of preference but more likely an annual period effect with fewer convictions during the summer months. One concern regarding our identification strategy is its assumption that the reforms only create variation in the length of incarceration and not in the imposed sentence length. One possible way the reforms might indirectly affect imposed sentence length would be if judges wished to counter the effects of the reforms. For example, in order to minimize the effect of the 1993 or 1999 reforms, judges might use their discretion and sentence offenders convicted after these reforms to shorter sentences. We have plotted the distribution of the length of imposed prison sentences separately for the treatment and control groups for all 3 reform years and see no evidence of differences in sentencing patterns between these groups (see Appendix Fig. 6).

Our quasi-experimental design may be susceptible to period effects that might produce differences between treatment and control groups that are not related to the reforms. While there is, most certainly, a multitude of developments going on in each observation window, we are unaware of any particular phenomenon that could

[6] As a result of the presence of extreme outliers, our count variable has been winsorized at the 95th percentile.

produce systematically different effects for treatment and control groups. As a way of checking for period effects, we have nonetheless compared groups who were sentenced to prison for the first time prior to and after the reform date, but who were sentenced to prison terms that were not affected by a reform (either because the sentence was too short or too long). We have also performed "placebo tests," specifying the parole reform dates as instead having occurred 1 year prior to and 1 year after the actual reform years. A further test for robustness is done by expanding the time window from 1 to 2 years prior to and after the reforms, thus increasing the total time window to 4 years. The outcomes from these sensitivity tests will be presented in "Results" and accessed in the supplementary material.

As was noted earlier in the section on the Swedish parole institution, the length of post-release supervision decreased with the 1983 reform. This might affect the comparison between the treatment and control groups in two ways. The longer parole supervision prior to the reform might have made individuals less prone to crime following their release, but it might also have resulted in recidivism being detected more frequently and more easily in this group by comparison with the treatment group.[7] For these reasons, the outcomes from the 1983 reform analyses should be interpreted with some caution.[8]

Results

Table 1 presents a comparison of demographic characteristics and criminal justice contacts between the treatment and control groups for the three reform samples. The groups are overall similar, but with some exceptions. The treatment groups for the 1983 and 1993 reforms are 4–7 months older at their age of onset than their control groups. This is expected, however, since our data consist of the cohorts born after 1957 and as a result of the temporal order of the design, the treatment groups are allowed to be 1 year older. Because of this, we control for age at crime in the adjusted model but also perform a robustness check that minimizes age differences between the treatment and control group. There is virtually no difference between treatment and control groups in the age of criminal onset in any of the reform years. The only statistically significant difference in prior crimes is observed in the 1993 reform sample where the treatment group has on average been convicted of 0.3 more violent crimes and in the 1999 reform sample, where the control group has on average been convicted of 0.6 fewer violent crimes, and 0.5 fewer narcotics crimes. Despite the overall similarity between treatment and control groups in the three reform samples, there are nonetheless slight differences, and to ensure that these do

[7] See the discussion by Roodman (2017) concerning what he refers to as "parole bias," and who argues that potential differences in recidivism rates among those serving different sentence lengths may be due to differences in the duration of post-release supervision.

[8] In an effort to control away potential bias produced by the variation in the length of supervision periods, we performed a sensitivity analysis with respect to the 1983 reform, with a shorter follow-up period. The results from an analysis where the follow-up period was reduced to 1 year did not deviate from the results from the 10-year follow-up analysis (Supplementary Table 5).

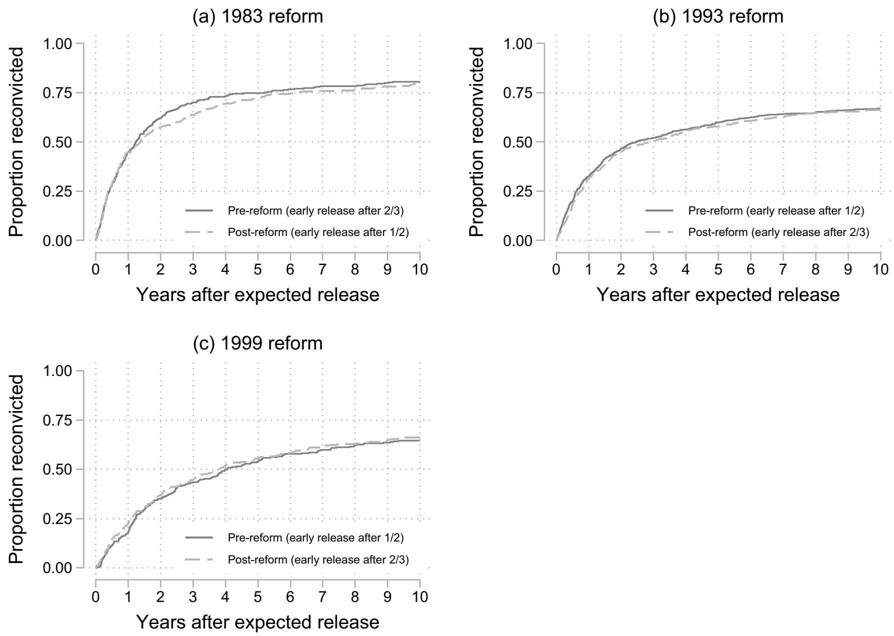

Fig. 3 **a–c** Cumulative reconviction probabilities for 1983 parole reform sample (**a**), 1993 parole reform sample (**b**), and 1999 parole reform sample (**c**) for treatment (post-reform) and control group (pre-reform)

not bias our estimates, we have controlled for these characteristics when estimating the average effect of the respective reforms on recidivism.

Before turning to our regression analysis, we first explore the extent to which our treatment and control groups have been reconvicted over a 10-year follow-up period. Figure 3 presents estimated Kaplan-Meier cumulative probability functions by treatment and control group in the three reform samples. It may first be noted that the reconviction risk in these first-time imprisonment groups is generally high, irrespective of group membership. As indicated by the steep increase over the first years following expected release, the hazard for recidivism is highest during the years immediately following release, after which it declines. This pattern replicates the conventional wisdom in recidivism research (e.g., Nygaard Andersen & Skardhamar, 2017). It is also noteworthy that although there are differences in the reconviction risk between treatment and control groups, the differences are relatively small with the largest difference between the cumulative curves is found for the 1999 sample, where the treatment group's reconviction risk is 1.5 percentage points higher than that of the control group. A similar small difference between treatment and control group is observed when recidivism is measured as reincarceration (see Appendix Fig. 7).[9]

[9] One interesting deviation is, however, that the temporary differences between the reconviction curves for the treatment and control groups that occur 1–5 years after the expected release in the 1983 reform (see Appendix Fig. 7a) are not observable when recidivism is measured as reincarceration.

Table 2 Cox regression models predicting the risk for recidivism in three reform samples. Hazard ratios (HR) and confidence intervals (CI)

	Reconviction						Reincarceration					
	Model 1			Model 2			Model 1			Model 2		
	HR	CI 95%		HR	CI 95%		HR	CI 95%		HR	CI 95%	
1983 reform												
Pre-reform	1			1			1			1		
Post-reform	0.940	0.792	1.117	1.045	0.875	1.248	0.957	0.781	1.172	0.990	0.801	1.223
1993 reform												
Pre-reform	1			1			1			1		
Post-reform	0.959	0.852	1.078	0.961	0.853	1.082	0.948	0.818	1.098	0.935	0.805	1.086
1999 reform												
Pre-reform	1			1			1			1		
Post-reform	1.053	0.868	1.278	1.052	0.859	1.289	0.989	0.774	1.262	1.050	0.813	1.356

Model 1: unadjusted. Model 2: adjusted for age at crime, squared age at crime, imposed prison days, conviction month, prior conviction frequency, prior crime frequency by crime type (violent, sex, property, fraud, vandalism, traffic, narcotic), age of first convicted crime, and whether the offender was born in Sweden, and sex. Each estimate represents results from a separate regression

*$p < .05$

Moving on to the regression models, Table 2 presents unadjusted and adjusted estimates from Cox regression models by reform sample on the hazards for reconviction and reincarceration. The estimates from the unadjusted models are parameterizations of the Kaplan-Meier curves in terms of hazard ratios (HR). For example, the difference of 1.2 percentage points in the absolute reconviction risk that we noted between the treatment and control group in the 1983 sample (Fig. 3) is equivalent to a 6% lower (unadjusted) hazard for the treatment group. For the adjusted model, the 1983 treatment group has a 4.5% increase in reconviction risk. In the adjusted models for the 1993 and 1999 reforms — where incarceration time prior to parole release was increased — we see contradictory outcomes with a 3.9% decrease in reconviction risk for the 1993 treatment group and a 5.2% increase in reconviction risk for the 1999 treatment group. In general, the patterns are repeated when we instead look at the hazard of reincarceration in the right-hand side panel. Common to all estimates in Table 2 is, furthermore, that they are small and far from reaching statistical significance at the $p < .05$ alpha level.

Moving on to Fig. 4, we examine the relationship between the different categories of incarceration lengths and post-release reconviction. The figure should be read as reflecting a stratification of sentence lengths that were all impacted in various ways by a parole reform in terms of either a decrease or an increase in incarceration time, with the control groups used as a reference (see Fig. 1 for the size of the changes for specific sentence lengths). For example, the 4–5-month group in Fig. 4a were subject to an imposed sentence of 4–5 months, and as a result of the 1983 reform experienced a decrease of approximately 3 weeks in their period of incarceration. The adjusted HR for this group is 1.041, which means that a decrease of approximately

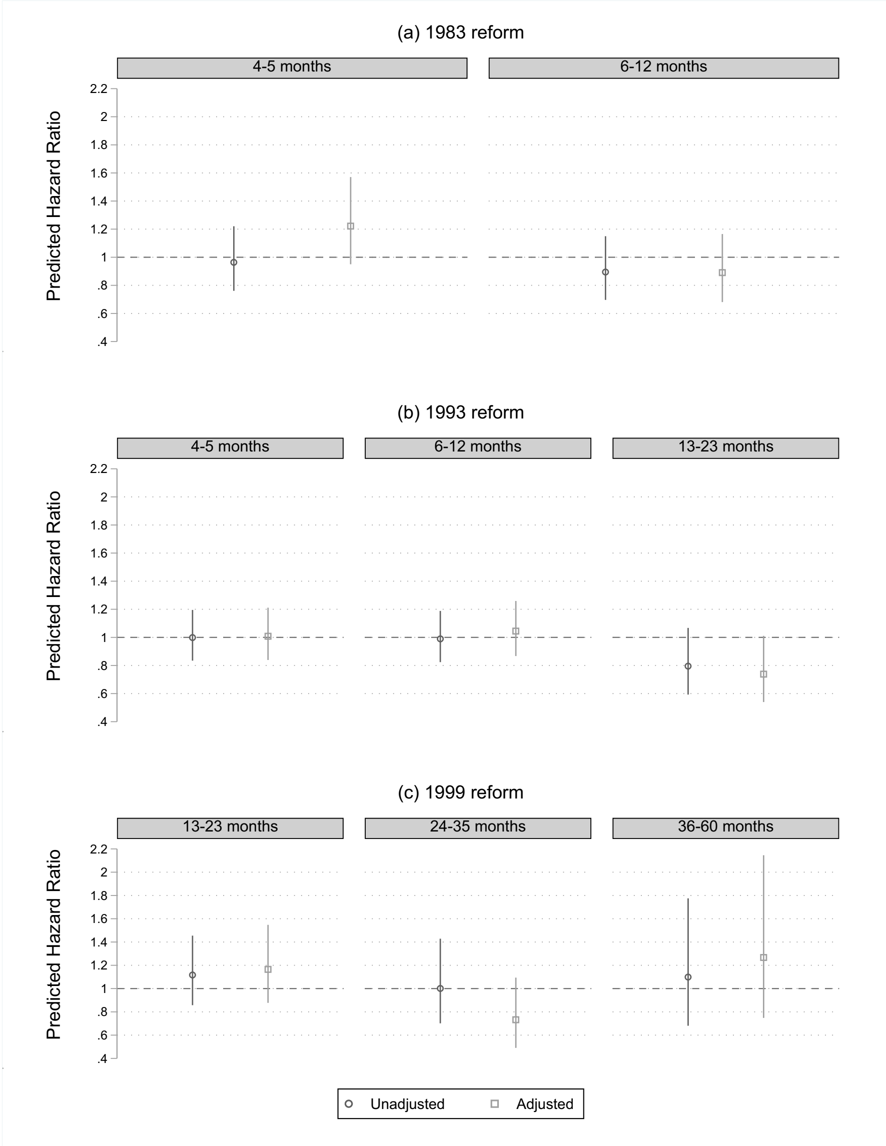

Fig. 4 **a–c** Cox regression models predicting the risk for reconviction in 1983 parole reform sample (**a**), 1993 parole reform sample (**b**), and 1999 parole reform sample (**c**). Adjusted hazard ratios accompanied by 95% confidence intervals

3 weeks in incarceration time for individuals sentenced to 4–5 months resulted in a 4.1% higher reconviction risk. All three reforms display inconsistent patterns, with estimates going in both directions and none being statistically significant. We nonetheless see a 16.4% decrease in the risk for reconviction in Fig. 4b when incarceration time was increased for inmates with the longest sentence lengths in the 1993

reform, of 13–23 months. For the group who were subject to the longest imposed sentence lengths in the 1999 reform (Fig. 4c), and who were accordingly also subject to the largest increase in incarceration time, we see an increase in the reconviction risk of 26.1%. Overall, it is difficult to draw any general conclusions regarding the relationship between categories of incarceration length and recidivism, in part as a result of the fluctuating pattern in the hazard for recidivism, but also because of the large standard errors.

For recidivism frequency, Table 3 presents estimates in terms of average marginal effects and incidence rate ratios (IRR). Over the course of the 10-year follow-up period, we see that ~4 more offenses (IRR: 1.187) were committed when incarceration time was decreased in the 1983 treatment group. When incarceration time was increased, we see an increase of 0.2 offenses during the 10-follow-up period for the 1999 treatment group (IRR: 1.025) and practically no effect on recidivism frequency for the 1993 treatment group. Again, none of the coefficients are statistically significant at the $p < 0.05$ alpha level.

Sensitivity analyses

To obtain a better understanding of the robustness of our results, we performed a series of sensitivity analyses. First, we looked at recidivism among offenders who are within the observation window (1 year on either side of the reform date) but who were not affected by the reforms because their sentences were either too short or too long (Supplementary Fig. 1). Second, we investigated if any notable changes could be obtained in our estimates by extending the observation window and thus increasing the number of offenders in our data (Supplementary Table 1). Third, we subjected our sample to various restrictions (such as age restrictions) to maximize comparability between the treatment and control groups Supplementary Table 2). Fourth, we constructed placebo reforms 1 year prior to and 1 year after each actual reform (Supplementary Table 3). None of these sensitivity tests revealed any notable changes in the results, and all estimates remained non-significant.

Discussion

In this study, we have exploited three separate natural experiments in order to measure the effect of incarceration time on recidivism (measured as reconviction, reincarceration, and recidivism frequency) among offenders incarcerated for the first time, using an extensive follow-up period of 10 years. The use of prison and sentence lengths varies widely between cultural contexts. In Sweden, the average sentence in 2020 was 13.5 months (Kriminalvården, 2021a), which can be contrasted with USA, where the average time served in federal prisons in 2012 was 37.5 months (Motivans, 2015). Only a small number of studies utilizing quasi-experimental designs have been conducted outside USA, which means that the scholarly knowledge concerning more moderate penal contexts is limited. Our results contribute to the discussion on the individual preventive effect of incarceration time, particularly

Table 3 Negative binomial regression models predicting recidivism frequency in three reform samples and with 3-year and 10-year follow-up time. Incidence rate ratios (IRR), marginal effects, and confidence intervals (CI)

	Follow-up: 3 years						Follow-up: 10 years					
	IRR	CI 95%		Marginal effect	CI 95%		IRR	CI 95%		Marginal effect	CI 95%	
Reform												
1983	1.074	0.875	1.317	0.55	−1.02	2.12	1.187	0.962	1.466	4.11	−1.02	9.23
1993	0.986	0.837	1.162	−0.09	−1.15	0.97	1.003	0.856	1.175	0.06	−2.95	3.07
1999	0.896	0.663	1.212	−0.29	−1.08	0.51	1.025	0.785	1.338	0.20	−1.92	2.31

Coefficients adjusted for age at crime, squared age at crime, imposed prison days, conviction month, prior conviction frequency, prior crime frequency by crime type (violent, sex, property, fraud, vandalism, traffic, narcotic), age of first convicted crime, whether the offender was born in Sweden, and sex. Each estimate represents results from a separate regression

*$p < .05$

with regard to the relatively unexplored effect of changes in mid-to-lower range sentences.

Utilizing large-scale administrative data containing all convictions for Swedish cohorts born after 1957, we find little evidence that increasing or decreasing the length of incarceration has a specific preventive effect on post-release offending. We were unable to detect any statistically significant effects of incarceration time, irrespective of how recidivism was measured or whether there was an increase or decrease in the time spent incarcerated. A less restrictive interpretation of the results would suggest that increasing incarceration time for short-term sentences (of less than 2 years) did not *increase* post-release recidivism, regardless of how it was measured, and instead showed a tendency towards minor decreases in recidivism. For long-term sentences of 2 years and more, the effects were the opposite, with tendencies towards an increase in recidivism when incarceration time was increased. The effects of a decrease in incarceration time were too heterogeneous (depending on how recidivism was operationalized) to draw any conclusions regarding tendencies. With regard to the relationship between categorizations of sentence length and recidivism, we found no clear relationship, with non-significant effects in both directions. It could be argued that for some sentence lengths observed in this study, the reforms only had a relatively minor impact (see Fig. 1) and that such small changes in incarceration time may not be sufficient to produce post-release effects. While we do acknowledge that this could be the case for some inmates, increases as low as 1 additional month of incarceration have still been proved to produce post-release effects on, for example, labor market attachment (Landersø, 2015), health outcomes (Hjalmarsson & Lindquist, 2020), and recidivism (Kuziemko, 2012). This suggests that the mechanisms that impact post-release behavior could be active even at minor changes in incarceration time.

As has been noted, European research on the effect of incarceration length is scarce, but previous studies have found no effect when analyzing Dutch offenders (Snodgrass et al., 2011; Wermink et al., 2018), and instances where there was a decrease in recidivism following increased incarceration time among Swedish offenders (Hjalmarsson & Lindquist, 2020). Although our observed null effect echoes the results of Wermink et al. (2018), since their data were limited to offenders sentenced to between 1 week and 15 months, there are difficulties when comparing the results. At the same time, we do observe a similar null effect for the sentence lengths of 4–5 and 6–12 months, which are partly comparable to the sentence lengths studied by Wermink et al. (2018). Further, our findings are in line with those of other second-generation studies that have examined the effects of incarceration time, and that have also found a null effect (Loughran et al., 2009; Meade et al., 2013; Mears et al., 2016; Rhodes et al., 2018; Rydberg & Clark, 2016).

As described by Mears et al. (2015), heterogeneity can be found not only in post-release effects but also in terms of the heterogeneity of in-prison experiences (i.e., treatment heterogeneity), which might explain both why some recent quasi-experimental studies have found recidivism-reducing effects (Kuziemko, 2012; Roach & Schanzenbach, 2015), but also the instances of recidivism-preventive tendencies noted in our study. Hjalmarsson and Lindquist (2020), for example, show that when incarcerated, participation in health programs could help to reduce recidivism (see also Bhuller et al., 2020; Lipsey & Cullen, 2007). Variation in participation and inmate programs may thus act as confounders and contribute to outcome heterogeneity between penal contexts, which suggests a need for further analyses to pinpoint why the effects of increased incarceration time vary. As noted by Hjalmarsson and Lindquist (2020), for some incarceration lengths, it may be that an increase in incarceration time enables participation in effective rehabilitating programs which would otherwise not be possible. The Swedish context is particular in the sense that the Scandinavian penal institutions are known for being rehabilitation-oriented with comprehensive in-prison health care and programs for education and vocational training (see Pratt & Eriksson, 2014; Ugelvik & Dullum, 2012; von Hofer & Tham, 2013). From an international point of view, Swedish prisons have one of the highest per inmate expenditures and this is in part due to the small-scale prisons, a low staff-to-inmate ratio, and the extensive rehabilitating programs. Alongside the relatively small prison population, these are features that may affect the extent to which the results reported above are generalizable to countries outside the Scandinavian context. On a similar note, because of national differences in the criminal sanction system inmate composition and thus recidivism risks differ between countries. For example, traffic offenses in Norway are punished by incarceration far more often than in Sweden (Kristoffersen, 2013), and drug possession and drug use are criminalized in Sweden as opposed to the Netherlands (Chatwin, 2003).

Before discussing the policy implications of this study, a number of limitations need to be addressed. First, because of the negative relationship between age and criminal participation (i.e., the age-crime curve), age may have a confounding effect when analyzing recidivism. In our study, this is primarily an issue for those individuals who experienced the largest increase in incarceration time (see Fig. 1), producing an age gap between the treatment and control groups at the time when the offenders were released. Second, with regard to a more general discussion concerning internal validity and unobserved confounding, it should be mentioned that although we have utilized natural experiments to minimize the influence of confounders and selection effects, we cannot rule out the existence of such biases.[10] This issue is the most prominent concern in relation to the design employed in this study, since the treatment group is observed on average 1 year after the comparison group. Because our study design limits us from controlling for period effects, we cannot with certainty rule the effect from the general crime decline in convictions witnessed in Sweden during the study period (Bäckman et al., 2020). Third, our reliance on natural experiments has meant that we have been limited to those few occasions on which these have occurred in Sweden. It would be methodological preferable if these natural experiments had been more recent in order to minimize the limitations to generalizability associated with possible differences in how the correctional services operate, but also general societal, economic, and legal changes. Examples of the latter

[10] As described in the section on the Swedish parole institution, Hjalmarsson and Lindquist (2020) evaluated whether the 1993 and 1999 reforms could be used as a natural experiments, and found that they could.

are the criminalization of the purchase of sex (Levy, 2014) and zero-tolerance drug policies (Lenke & Olsson, 2002), both part of a general trend toward a more punitive crime policy (Tham, 2001). To some extent, these changes may limit the comparability between reforms since the time distance between them have potentially resulted in variation with respect to the composition of the inmate population over the three reform periods. Fourth, although the results regarding different categories of incarceration lengths provide us with important knowledge with regard to nonlinearity, stratifying incarceration lengths in this way involves a loss of precision in our estimates, which can be seen in the large confidence intervals. Fifth, because we do not have exact dates on prison entry and release, we are limited to approximations of the release date (see outcome variables section). The estimation of release dates could produce bias if there were a reason to believe that the period between receiving a conviction and starting one's sentence differed between the periods before and after a given reform. However, we have no reason to expect any systematic differences between the treatment and control groups in any of the reform samples. Sixth, because treatment heterogeneity may be critical to the understanding of confounders that might impact treatment effects, differences in quality or intensity of prison programs (and other in-prison experiences) may be an issue when generalizing our results to penal contexts that vary from the Swedish correctional system, in terms of both sanctioning policies and also the emphasis on rehabilitation. At the same time, other parts of Western and Northern Europe have similar policies and conditions, and a similar focus on rehabilitation, and thus generalizability should be possible to a broader context than just Sweden.

Limitations aside, the policy implications of this study are not clear. In general, the issue of the effectiveness and crime-reducing potential of custodial sanctions and longer incarceration times is complex with heterogeneous effects depending on offender characteristics and the environment in which the inmate is held. An increase in incarceration time could allow for further rehabilitative interventions for some at-risk individuals serving shorter sentence lengths, but this presupposes that the environment in which they are incarcerated has the resources to correctly identify at-risk individuals, and sufficient social and health programs. In contrast to non-custodial sanctions, incarcerating offenders with no prior prison record is, however, associated with increased post-release recidivism risks (Nieuwbeerta et al., 2009; Toman et al., 2015; Walters, 2003). In addition, there is evidence from Scandinavia showing crime-reducing benefits of electronic monitoring among individuals who have not previously been incarcerated (Andersen & Telle, 2019). From a policy perspective, redirecting individuals without prison records to alternative sanctions may therefore be a more effective means of reducing crime, as well as being more cost-effective. As suggested by this study, the overall crime-control benefits of increasing incarceration time for first-time incarcerated offenders may be questioned.

Appendix

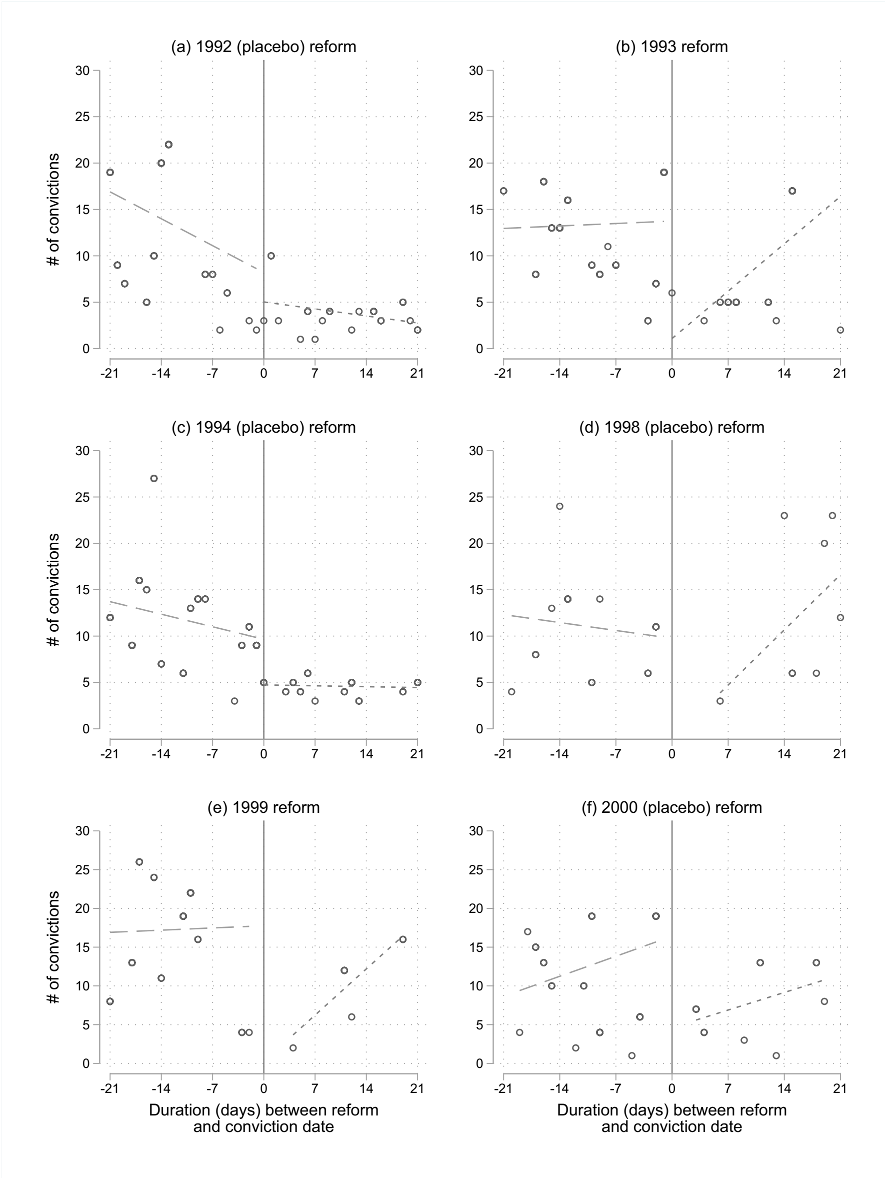

Fig. 5 **a–f** Number of convictions (per day) prior and after July 1, 1992, placebo reform (**a**), July 1, 1993, parole reform (**b**), July 1, 1994, placebo reform (**e**), January 1, 1998, placebo reform (**d**) January 1, 1999, parole reform (**e**), and January 1, 2000, placebo reform (**f**)

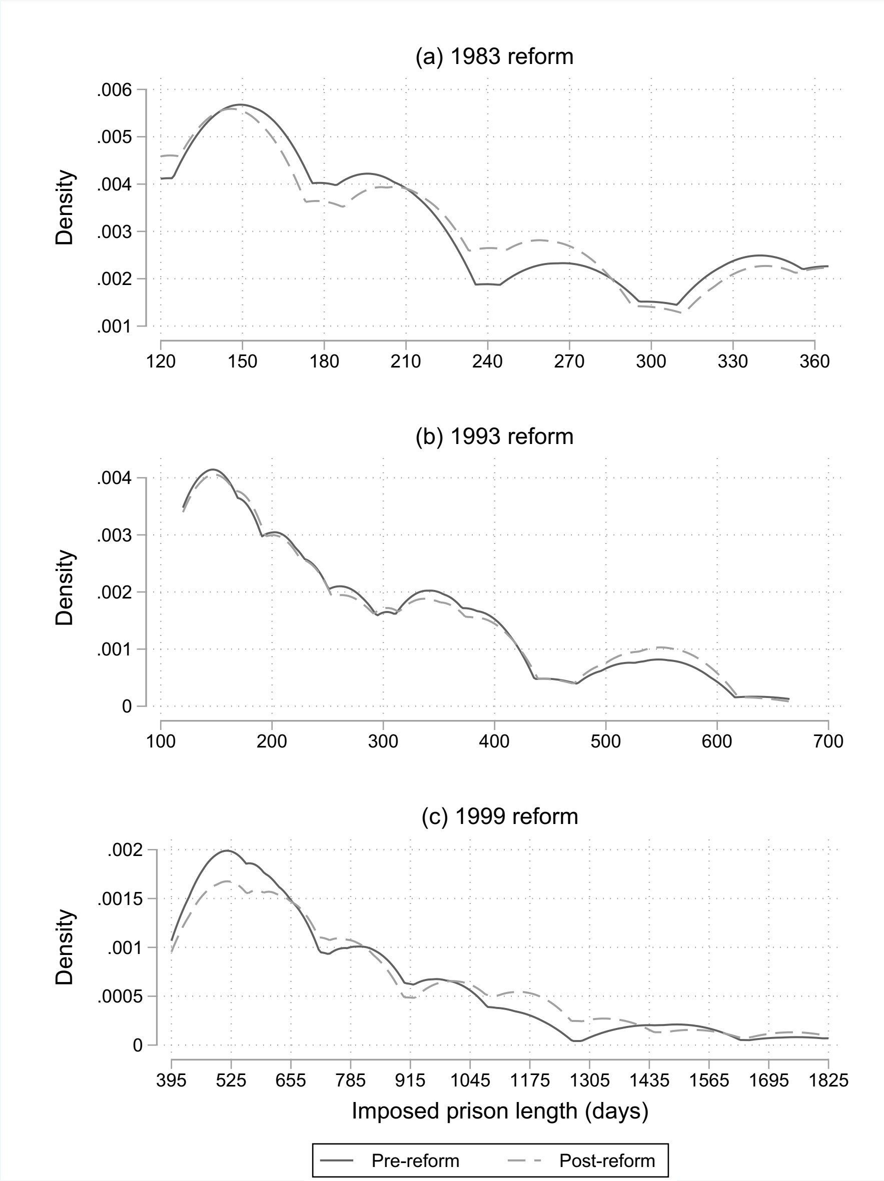

Fig. 6 **a–c** Densities of imposed sentence length in 1983 parole reform (**a**), 1993 parole reform (**b**), and 1999 parole reform (**c**) for treatment (post-reform) and control group (pre-reform). Only sentence lengths that are affected by the reforms are presented

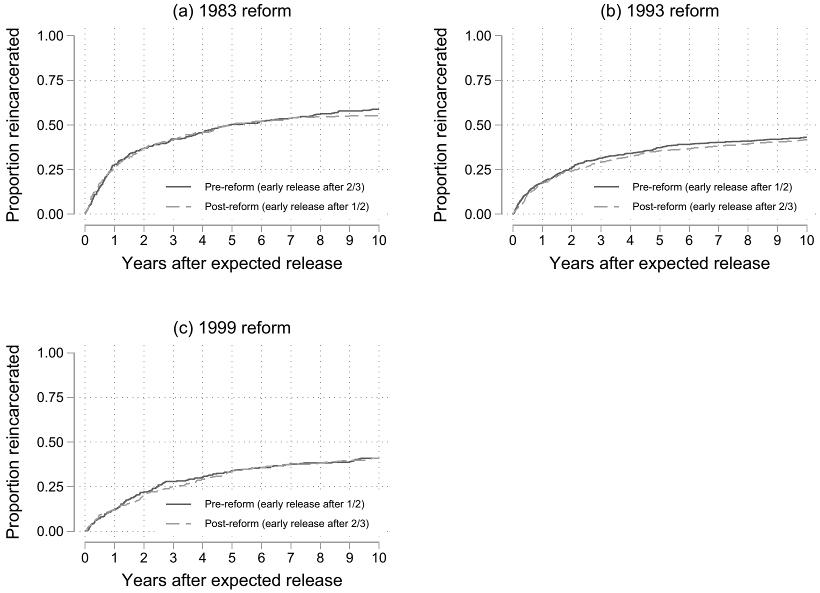

Fig. 7 **a–c** Cumulative reincarceration probabilities for 1983 parole reform sample (**a**), 1993 parole reform sample (**b**), and 1999 parole reform sample (**c**) for treatment (post-reform) and control group (pre-reform)

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s11292-022-09513-1.

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