Department of Criminology

Stockholms universitet

Does Paying Extended Time for Crime Foster Recidivism?

A Natural Experimental Study on the Dose-Response Relationship Between Incarceration Time and Recidivism in Sweden

Master thesis in Criminology, 30 credits

Criminology
Advanced level
Spring semester 2019
Enes Al Weswasi

Abstract

Incarceration is from a policy perspective often used with the rationale that it has a deterrent effect on offenders and that increasing incarceration length could reduce recidivism. Few studies with a robust counterfactual design have, however, tried to assess the relationship between imprisonment and recidivism.

By exploiting a natural experiment, this study investigates the causal relationship between incarceration length and recidivism and the dose-response relationship. Heterogeneous effects of increased incarceration time, with respect to gender, age, immigrant background and offence type, are also considered. In one of the few studies done outside the US, the present study uses a Swedish dataset consisting of adult offenders (N=10 454), who have received their first prison sentence and of no more than two years (average 7.5 months). A linear probability model is used to estimate the probability of recidivism with follow-up periods of 1, 3 and 5 years.

The findings suggest that exposure to increased incarceration time exerts an overall null-effect on future rates of recidivism and without observed heterogeneity. From a policy perspective, the lack of evidence that increasing incarceration length yields a decrease in recidivism rates indicates that prison sentences could be reduced with as much as four months or that the required time an inmate has to be incarcerated, before being released for parole, could be reduced to half-time.

Keywords

Dose-response, incarceration, linear probability model, natural experiment, parole, recidivism

Contents

1. Introduction

Sweden has for a long time been perceived as a modest country in regards of penal attitudes but recent decades 'tough on crime' policies have challenged this 'Scandinavian exceptionalism' (Pratt 2008; von Hofer and Tham 2013; Barker 2013; Tham 2018:chap. 3). Increasing incarceration lengths is, by policymakers, often argued having a specific deterrent effect on offenders; increase the negative consequences of committing an offence, and the offender will abstain from relapsing into crime. Average prison sentences in Sweden has, in part as a consequence of the escalation of punitiveness, increased by 50 per cent since the 1980s (Tham 2018:44). The punitiveness has increased even further in recent years (Tham 2018:72–74) and discussions among Swedish politicians are now – throughout the political spectrum – of increasing the penalties for young adults (SVT 2018a), abolishing or severely limiting parole (SVT 2018b) and building more prisons (SVT 2019). Recent decades' rediscovery of the prison and the increasing trust, from policymakers, that incarceration could render reduced delinquency (Estrada 2004:420) has brought the spotlight back to criminology's longstanding theoretical discussion on the criminogenic versus deterrent effects of prison.

The present study will be the first Swedish study, and one of the few studies done outside the US, estimating the causal relationship between increased incarceration length and recidivism. Establishing causality will be done by exploiting a natural experiment which will enable a counterfactual design, with a treatment and control group. Incarceration length will furthermore be portioned and used in what medical researchers term 'dose-response relationship', estimating the magnitude in the response after exposure to a specific 'dose' of increased incarceration length (Loughran et al. 2009; Nagin et al. 2009:121–122 & 167–169). Finally, heterogeneous effects of exposure will be measured with the intention to establish if incarceration affects individuals differently depending on age, gender, immigrant background or committed offence type.

1.1. Targeting the Research Problem

In research on the observed increase in punitiveness, the 'penal turn', described by Garland (2001), has resulted in a politicisation of crime where experts are less heard. For criminology, this increases the importance of robust studies which aid and promote a rational public debate on criminal policies. A debate which is sometimes lacking in times of 'penal populism'.

As the forthcoming literature review will show, a large number of studies have tried to establish how, why, and if there is a causal link between incapacitation and potential

relapse into delinquency. Criticism has, however, emerged in the last decade underlining some of the methodological issues that past research has been facing. In their systematic literature review, Nagin et al. (2009) highlight various shortcomings in the existing literature; issues of inference that future research should pay attention to. The authors state that studies often estimate the effects of incapacitation on recidivism but ignores the question of how incarceration length affect recidivism (Nagin et al. 2009:128). This study will try to address this issue by estimating the effects of an overall increase as well as 'doses' of incarceration time and recidivism.

An issue when trying to establish causal mechanisms is that it requires a comparison with the counterfactual; asking the question of how rates of recidivism would have unfolded had the individual not received increased incarceration time. Nonexperimental studies (which are the overwhelming majority of social science studies and not exclusive to criminology) has tried account for this by including numerous controls in their regression-based models (Wermink et al. 2018:1061; Nagin et al. 2009:133–134). Issues of bias in the selection processes are nevertheless still salient regardless of how well defined the implemented control variables are which has led academics to recommend randomised studies or natural experiments (Villettaz et al. 2006:42–44; Nagin et al. 2009:184). By implementing an experimental design, potential issues of selection bias could be avoided when doing further research on the effects of imprisonment on recidivism. Since the current study will try to pinpoint causality, the issue of selection bias is thus critical. The identification and exploitation of a natural experiment done in this study do, hopefully, account for true randomisation.

Furthermore, to what extent results from current studies are generalizable is to some degree dependent (besides methodological issues) on one factor: the context of penal climate in the observed setting. Punitive attitudes, prison climate and general 'harshness' in penal policies vary between countries. In 1999, Gendreau et al. (1999:19) described how studies outside the US are "urgently required", but despite that, only a relatively small literature with a robust methodology has, to this date, analysed the effects of imprisonment on recidivism in countries that are less punitive than the US (Wermink et al. 2018).

1.2. Purpose & Research Questions

The purpose of this study is to estimate the effects of time in prison on recidivism. In order to pinpoint causality, a Swedish natural experiment will be exploited, that resulted in inmates being required to spend more time incarcerated before being released for parole.

The study aspires to answer the following research questions:

i) *How did the increase in required incarceration length before being released for parole, from half-time to two-thirds, affect recidivism?*
ii) *What is the dose-response relationship between incarceration length and recidivism?*
iii) *Does the effect of increased incarceration length on recidivism vary depending on gender, age, immigrant background, or offence type?*

1.3. Delimitation

The natural experiment, which this study exploits, only affected individuals who received a prison sentence of two years and less, thus the outcome of this study is mainly limited to offenders sentenced to maximum two years. Furthermore, the natural experiment resulted in an increase in incarceration length only for offenders serving a minimum of three months.

Although offenders between the ages of 15 to 18 can receive a prison sentence (particularly serious crimes), only adult offenders of 18 years and above are included in this study. Also, recidivism is measured as being reconvicted for an offence. This study does, therefore, not claim to cover all committed offences during the study period but only registered reoffences during a maximum follow-up period of 5 years.

2. The Swedish Punitive Turn

How Western governments react and try to control delinquency has markedly changed since the 1970s (see O'Malley 1999; Young 1999; Wacquant 2001; Pratt 2007:chap. 2). Garland (2000), a prominent scholar and theorist in the field of penal institutions, describes this transformation as a 'punitive turn'. What Garland (2001:chap. 6) elegantly describes in his book, *The culture of control*, is a new cultural formation of a 'crime complex' of late modernity.[1] In this 'crime complex' fear of crime is rampant, 'crime consciousness' is institutionalised by the media, crime is politicised, the general public lacks trust in the criminal justice system, risk avoidance is high, and the crime victim is emphasised.

The cause behind this shift in policy and discourse is construed in different ways. For Garland the punitive turn is primarily the cause of an increase in exposure to crime; especially among, what Garland calls, 'the professional middle class'. As delinquency became a normal 'social fact' among the liberal and educated middle class, so did an increase in punitive attitudes within this relatively large and cultural powerful group that otherwise has had a general trust to the penal-welfare framework. Politicians understood early the political gains that could be made by politicising crime and thus the increased use of 'tough on crime' rhetoric and what Pratt (2007:3) calls 'penal populism'.

However, Tham (2001:412–413; 2006; 2018:139–142) and Estrada (2004:437) argue that crime rates cannot be the primary explanation of the penal turn since penal shifts have occurred in countries where there has not been a rise in delinquency – such as in Sweden.[2] What instead is in play is a sharp increase in individualisation since the end of the 1970s; crime was no longer perceived as a damaging act against society but an act perpetrated by an individual against another individual (see also Demker et al. 2008; Tham et al. 2011). Thus, public sentiment shifted from being 'what's best for the collective society' to 'what's best for me'; a crime victim centred position. Demker and Duus-Otterström (2009) describe in their study on the transformation of the political discourse in Sweden a similar development. The authors argue that the emergence of punitive policies in Sweden is a product of individualization of the causes of crime, emerging in the 1970s that a decade later led to the increase of victimization in criminal policies but also to the side effect of increased fear of crime (see figure 1). Balvig (2005:183–184) in his critique of crime rates as a universal

[1] The idea of a 'crime complex' and the punitive turn is also discussed in Garland's articles *The limits of the sovereign* (1996) and *The culture of high crime rate societies* (2000).

[2] Victimization has, as a matter of fact, either been stable och decreased for the middle- and high-income groups in Sweden during the period when crime policies shifted (Nilsson and Estrada 2003).

explanation of the penal turn, draws a similar conclusion in his study on the penal development in Denmark. Demker et al. (2008:327) argue that an observed shift has also occurred regarding the penal attitude in the general population of Sweden with an increase in 'tough on crime' attitudes.[3]

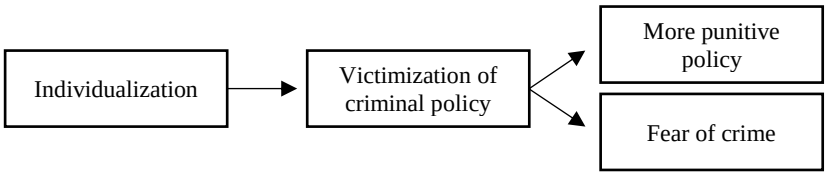

Figure 1. The causal chain of the punitive turn (from Demker and Duus-Otterström 2009:290).

One of the consequences of the punitive turn in Sweden is a shift in the theory of punishment. In 1977 The Swedish council of crime prevention (BRÅ) published a report with the title *New penal system* (*Nytt straffsystem*) with the suggestion of a return to a neoclassical approach to delinquency, from the previous rehabilitation ideology which was the cornerstone of Swedish criminal policy for decades; a reutrn to punishment as a deterrent rather than a mean to rehabilitates. Although the report was not well received by policymakers (Andersson 2002:73–78; Tham 2018:58), it was in some sense telling of the penal shifts that were emerging.

In a historical portrayal of how criminal policies have developed since the 1960s, Tham (2018:75–79) argues that the politicisation of criminal policies has come at the expense of experts influence. Von Hofer has, nevertheless, described how "Sweden still emerges as the country where the philosophy of individual prevention, based on a wide variety of sanctions, is most pronounced […]" (2005:64–65). It is this context that the present study unfolds; in an era of 'penal populism' that pushes for increased use of prison and longer sentences.

[3] Jerre and Tham (2010) have problematized interpretations like these and instead describes how the general public often underestimates the actual levels of punishment and when asked how 'tough' penalties should be, often times suggests sentences lower than actual levels than judges prescribe.

3. Theoretical Understanding of Imprisonment

Scholarly explanation of punishment and incapacitation is a depiction with two different and contradictory messages; one of prison as a crime deterrent and one of prison as a criminogenic setting. The following section will describe both these positions to provide the reader with a theoretical platform when centring the subject of the effects of imprisonment on recidivism. The theoretical section ends with a discussion of how theory is used in the present study.

3.1. Deterrence Theory of Punishment

The intellectual roots of *deterrence* – the concept of omission of a deviant act because of its repercussion or punishment – can be traced back to the works of two philosophers: Jeremy Bentham (1780) and Cesare Beccaria (1764). Bentham (1780:chap 3, xxi) argued, in his book *Principles of Morals and Legislation*, that two opposing and rational notions guide an individual's behaviour: *pleasure* (actions that provide benefits) versus *pain* (actions that risk leading to costs). He furthermore believed that the legal and political system could best curb delinquency by highlighting the costs of deviant behaviour and thus making the net sum of the misconduct negative and therein less desirable to perceive (Bentham 1780:chap 3, xxiv).

What deterrence theory argues is that state efforts to reinforce morality or by sanctions, such as prisons, work as a means of averting individuals from criminal behaviour. Deterrence theory includes two main types of deterrence; *general* and *specific* deterrence, and the key sanction for this study - incarceration - has a crucial role in both type of deterrence (Nagin 1978, 95–97). Within general deterrence, the individual who receives the punishment is set as an example by the criminal justice system, for other potential offenders to observe.

With the backdrop of Bentham's ideas of pain and pleasure, general deterrence is well defined within economic theory. It describes criminals as capable of making rational choices and thus postulates that effective policies of deterrence must rest on calculations on how to make sure that the benefits of crime never outweigh the cost of the punishment (Becker 1968:179–180 & 207–209; Cook 1980:216–218).

Specific deterrence, on the other hand, describes how sanctions are made to correct individuals who have already taken part in delinquency. The goal with the punishment is less about the public, and instead about adjusting the individual offender into a docile citizen who will not reoffend (see von Hirsch et al. 1999). This is done either by rehabilitating the inmate or emphasizing the inconvenience of being incarcerated. In other words, the main division between general and specific deterrence is the personal experience of the punishment;

the latent threat of being sanctioned a punishment versus the personal experience of undergoing a prison sentence anticipates a deterrent effect. Specific deterrence also holds that the longer the incarceration length, the larger the effect is on deterring future reoffending, by increasing the notion of the severity of the punishment (Orsagh and Chen 1988:157–159).

Since the present study analyses how increased time spent incarcerated affect recidivism, the theoretical focal point when discussing incarceration will be on the specific deterrent ability that prison possibly has. However, the clear cut between the above concepts of deterrence could be criticised on different levels. In their effort to reconceptualise general and specific deterrence, Stafford and Warr (1993:127–128) argue that both types of deterrence can, simultaneously, operate for any given individual meaning and that it is thus problematic to exclude one type of deterrence when analysing recidivism when both could be in action. It should lastly be noted that the present study does not have the ability to establish if a possible decline in recidivism if found in the empirical material, is because offenders have been rehabilitated or deterred by the experience of being subjected to incarceration.

3.2. Prison as a Criminogenic Setting

Criminological theories on imprisonment suggest a rather different outcome from being exposed to a prison sentence than deterrence theorists. Here the onset of the argument is that prison is a unique and highly personal experience that reshapes one's thoughts and respectively, how others perceive one's character. The experience inside the 'society of captives' as Sykes (1958) characterises prison or when they enter 'the prison community' (Clemmer 1940) has unintended consequences that go beyond the idea of prison as a deterrent.

One genre of theories, which argue that prison is criminogenic, describes how prison functions as 'schools for crime'. In his study on how and why inmates develop subcultures inside prisons, Sykes (1958) discovered that prison subcultures are nothing more than a means to survive the frustrations – or the 'pains of imprisonment' – that inmates undergo while being locked up. Adapting to harsh prison conditions involves a normative and collective process among inmates who become socialised to embrace deviant attitudes. Prison thus provides a social learning environment for criminals, consistent with *social learning theory* (Akers 2010:51–56) but also with Sutherland's (1947) *differential association theory* that argues that criminal skills are exchanged and learnt within intimate personal groups, such as in prison.

Another set of criminogenic theories dictates that being subjected to prison does far more than change personal attitudes; it creates a societal reaction consisting of labelling and

stigmatisation aimed at 'deviants' (Becker 1963; Braithwaite 1989). What labelling theorists argue is that the societal reaction, in itself, contributes to the risk of a self-fulfilling prophecy; convicts who do not have the intention to reoffend might relapse due to the public stigmatisation. The mechanisms behind this hypothesis are twofold. Firstly, treating an individual as 'criminal' has consequences for the self-image of the offender who risks, because of the label, to adopt and internalise the criminal identity and subsequently act out in ways that are in line with the identity. Tannenbaum (1938:20) was among the first to describe this phenomenon: "[t]he person becomes the thing he is described as being". Secondly, the labelling, stigmatisation and society's collective discomfort with offenders, limits opportunities for individuals who are released from prison, resulting in difficulties obtaining a steady job and income. This puts the individual in a situation facing economic strain, which can lead to delinquency in an effort to secure an income (Merton 1938:678–679), or the absence of social bonds to individuals (i.e. family, friends, co-workers) and institutions (i.e. workplace, organizations, church) that could, otherwise, potentially keep the individual from relapsing into criminality (Hirschi 1969; Sampson and Laub 1993; 1997).

3.3. The Role of Theory in Present Study

Punishment and imprisonment is a core topic in criminology, and the literature on the subject is huge and has multiple theoretical approaches (Garland 1991:chap. 1). A significant part of the theories that discuss how offenders are affected by imprisonments can be, albeit roughly, split into two antagonistic positions; one theoretical positioning that argues that prison acts as a deterrent and one side that sees prison as a criminogenic setting. The purpose of this study is to estimate the impact of time served on future offending. Thus, the purpose has the potential to reveal in what direction the empirical material points to with regards to these two theoretical positions; does an increase in incarceration time have a specific deterrent ability on recidivism or does it increase the probability of recidivism and thus indicating a criminogenic effect? The role of theory in the present study is thus of deductive manner where the two theoretical positions are contrasted against the empirical material.

4. Literature Review

The body of literature that studies the effects of incarceration is vast, and several meta-analyses and systematic reviews have synthesised and reviewed the literature. These types of studies will be in focus in the forthcoming literature review, with one subsection dedicated to studies on the effects of custodial versus non-custodial sanctions, and one subsection covering studies on the effects of incarceration length and recidivism. In this summary of published meta-analyses and systematic reviews, five studies have been identified based on the criteria that they analyse in some way the effect imprisonment has on recidivism.[4] Thereafter, a review of the so-called second-generation studies that estimate the dose-response relationship between incarceration and recidivism. The literature review will conclude with a summary of studies on the heterogeneity of incarceration.

4.1. Custodial Versus Non-Custodial Sanctions

In one of the earliest attempts to take stock on the existing literature on the effects of incarceration sentences on recidivism, Gendreau et al. (1999) performed a meta-analysis consisting of studies dating from 1958 to determine if prison reduced reoffending. The main conclusion of the study was that "none of the analyses conducted produced any evidence that prison sentences reduce recidivism" (Gendreau et al. 1999:18). The results did, however, not indicate that prison sanction, in comparison to community-based sanction, was criminogenic either.

Villettaz et al. (2006) conducted a systematic review and meta-analysis of literature published between 1960 and 2003. Their conclusion echoed that of Gendreau et al. (1999); null-result when comparing custodial sanctions to alternative sanctions in both the systematic review and meta-analyses. The study was, however, more methodologically robust since the authors only included studies that met certain levels of methodological criteria. Another set of conclusions was that experimental studies are rare exceptions and follow-up periods seldom extend beyond two years (Villettaz et al. 2006:3).

An updated version of the above study was published in 2015 by Villettaz et al. with an even larger emphasis on research design, but with the same area of focus. Although almost ten years had gone by since their previous study, the authors still concluded that few experimental studies have been conducted and follow-up periods were still generally short

[4] Jonson's meta-analysis (2010) is excluded from this review because it is in large parts an extension to the systematic review of Nagin et al. (2009).

(showing the strength of the present study which uses an experimental design with a follow-up period of five years). The authors' systematic review of relevant studies suggested that incarceration is followed by higher recidivism rates than non-custodial sanctions.

With the ambition of expanding previous reviews, Nagin et al. (2009) examined an even larger body of research in their systematic review. With regard to effects of custodial versus noncustodial on recidivism rates, experimental studies, matching studies, either by variable-by-variable or propensity score or regression-based studies showed all a predominant criminogenic effect of prison. The most peculiar finding in their review was that no overall conclusion could be drawn from the regression-based studies since they all suffered from fundamental analytic flaws concerning neglection of confounding factors such as age when released from prison.

4.2. Incarceration Length & Recidivism

Last decades empirical research on the effects of imprisonment on recidivism has expanded to observe how rates of recidivism are affected by various incarceration lengths. In Gendreau et al.'s (1999) meta-study, the authors' results point to a slight increase in recidivism rates among inmates who received 'more' (average of 30 months) versus 'less' (average 13 months) incarceration time. When the results were sub-divided into inmate's risk-levels, the results showed a positive correlation between prison time and recidivism among both the high-risk group and low risk (but with a larger effect size for the low-risk group). More than 90 per cent of the included studies were, however, from the US and conducted in the 1970s, which should be considered when inferring the results to other regions or contexts.

Nagin et al. (2009) had difficulties, in their systematic review, finding an overall conclusion regarding incarceration length and recidivism. At the time when the authors did their systematic review, only two studies (Berecochea and Jaman 1981; Deschenes et al. 1995) had been published that used an experimental design and the authors drew the conclusion, based on these two studies, that longer sentences either had no effect or a minor preventive effect. The authors identified three matching studies (Jaman et al. 1972; Kraus 1981; Loughran et al. 2009) and the studies published between 1972 and 1981 found, albeit based on dated material, that longer prison sentences were associated with higher recidivism. Loughran et al. (2009) found that imprisonment length neither had a criminogenic nor a preventive effect. However, the study itself and its outcome will be further discussed below because this study is

the first, in the new generation of studies, with a robust and different methodology than previous studies, that analyse incarceration length and recidivism.

4.3. Dose-Response Relationship on Recidivism; Second-Generation Studies

Two generations of research traditions can be distinguished in the empirical literature on incarceration time and its effect on recidivism (Wermink et al. 2018:1061). In general, the first generation of studies did not explicitly study effects of prison time but instead, included time served in prison as a control variable (among others) in their regression-based studies (Nagin et al. 2009:169). Most of the studies in the systematic reviews and meta-analyses, described above, falls into the category of first-generation studies. Second generation studies are specially designed to estimate the dose-response relationship between sentence length and recidivism by either a matching design or the use of an instrumental variable method. In short, dose-response studies estimates the magnitude in the response (e.g. recidivism) after exposure to a specific 'dose' of treatment (e.g. incarceration length).

Since the publication of Nagin et al.'s (2009) systematic review, there have only been eight studies published (nine in total) who uses a counterfactual design with the possibility of drawing a conclusion about a causal connection between incarceration length and recidivism. These studies and Loughran et al. (2009), who was the first among the new generation of studies, will be briefly reviewed below.

Loughran et al. (2009) used a longitudinal dataset consisting of juvenile offenders to estimate a dose-response relationship between incarceration length and recidivism. To address the possibility of selection bias, the authors used propensity score matching to 'balance' groups of delinquents, who were incarcerated for different amounts of time. The authors found neither a preventive nor a criminogenic effect of longer imprisonment length.

Among the second generation studies, Loughran et al. (2009) and Snodgrass et al. (2011) is the only to use data that include young offenders. Snodgrass' et al. (2011) data consist of offenders between the age of 12 and 40 from the Netherlands. The results from their propensity score matching were to a large extent the same as the outcomes of Loughran et al. (2009); little evidence of a relationship between incarceration time and recidivism, across age groups, on a follow-up period of three years. The authors found, however, that individuals who receive longer first-time sentences subsequently tend to be sentenced to more days during the follow-up period.

A second study to use a dataset with Dutch offenders (and the only other second-generation study conducted outside the US), Wermink et al. (2018) analysed the short-term effects of imprisonment length on recidivism. Using propensity score matching and a follow-up period of six months, the authors concluded that incarceration time does not affect recidivism rates. The outcomes of their study are of particular relevance for the present study since it only includes short-term sentences (<15 months).

Mears et al. (2016) used the matching methodology generalised propensity score, and found a relationship between the length of stay and recidivism; up to one-year terms increased rates of recidivism but then, decreased rates for sentences between one and two years. No effect whatsoever was found for incarceration times between two to five years. Rates of recidivism decline for inmates sentenced to terms longer than six years, but these estimates were associated with large standard errors making the outcome difficult to inference on long-term offenders.

Similary, Meade et al. (2013) used propensity score matching and although the results showed a slightly negative association between increases of prison length and recidivism after 13 months the only meaningful and significant difference in recidivism came from the group sentenced to terms longer than 6.5 years who had the lowest rates of recidivism. Meade et al. (2013) did not have any issues with large standard errors for the long-term offenders, making their results more reliable than the outcome from the long-term offenders in Mears et al. (2016). With regards to offenders serving less than two years (a more relevant group for the current study) recidivism rates were lower, although the treatment effect was small, for individuals who received less than 7 months in comparison to the group who received a dose of 7-16 months incarceration (Meade et al. 2013:536–538).

In a more recent study, Rydberg and Clark (2016) replicated to some degree the outcomes of Meads' et al. (2013) indicating a decline in recidivism rates as incarceration length increases from 12 months or more. The authors noted, however, that there was considerable heterogeneity among different offence types which led the authors to conclude that no clear support was found for either deterrent or criminogenic effects of incarceration time.

The last three studies all exploited, in various ways, the same quasi-experiment and all used instrumental variables method in order to estimate how increases in prison sentences affect reoffending. All three studies capitalised on the experimental nature of i) when offenders randomly receive a sentencing judge and ii) the U.S. sentencing guidelines structure which is a point-based system on predetermined characteristics such as age and a prior criminal record that judges use when they determine an appropriate sentence. Judges do, however,

interpret the guidelines differently (due to subjective proclivities) thus adding variation in sentence lengths depending on the judge and not the endogeneity solely based on the offender's committed offence.

Kuziemko (2012) exploited the discontinuities[5] in the Georgia parole guideline grid and found that an additional month of time served lowered the recidivism rate by 1.3 percentage points. Roach and Schanzenbach (2015) point to a similar finding: one additional month of sentence reduces recidivism by about one percentage point with the greatest effect in recidivism on the first year of release. The treatment effect is, however, limited to low-level offenders due to the rather small average median sentence in their dataset of 4 months. Lastly, Rhodes et al. (2018) use both a regression discontinuity design and an instrumental variable method to estimate the relationship between prison length and recidivism. Their findings stand out in relation to the previous two studies; they find that an offender's criminal trajectory is not much affected by an increase of 7.5 months of prison. The treatment effect did not indicate any heterogeneity, irrespective of criminal record, education level, sex, race or offence seriousness. The last variable, offence seriousness, is of particular strength in their study since the authors had, in comparison with Roach and Schanzenbach's (2015) study, ability to include offenders convicted of serious crimes and not exclusively low-level offenders.

In an attempt to synthesise the results from the second-generation dose-response relationship studies, one might conclude that there is no clear pattern whether increases in incarceration time is deterrent or criminogenic. The safest estimation is that the relationship between incarceration time and recidivism is of a null-effect which echoes the findings of recent systematic reviews (Nagin et al. 2009). As mentioned earlier, only two second-generation dose-response studies have been conducted outside of the US, which makes the current study an important contribution to the literature on the effects of incarceration on recidivism.

4.4. Effects of Imprisonment on Different Demographic Groups

Scholars have argued that imprisonment should not be seen as a universal experience that has homogenous effects for all groups of individuals (Mears et al. 2015). This assertion stems from various explanations, such as complex 'baggage' of experiences individuals carries with them

[5] See Shadish, Cook, and Campbell (2002:207–220) for an explanation on how discontinuities are used to isolate causality.

throughout life (i.e. drug abuse, victimisation) or individual differences in biological evolvement (i.e. age-related maturity).

One such divergence is recidivism rates after incarceration depending on gender. In Sweden, recidivism rates for women, within a three years follow-up period, is 24 per cent while the corresponding number for men is 31 per cent (Kriminalvården 2017:64). A one-of-a-kind study estimating gender differences in recidivism depending on offence type, and using propensity score matching, found that incarceration, in comparison to non-custodial sanctions, increased male's likelihood of recidivism in all offence types while incarceration only increased women's likelihood of recidivism for property and to some degree drug crime (Mears et al. 2012:375). A meta-analysis estimating gender differences in recidivism rates depending on more versus less incarceration time found that more incarceration time was associated with higher adverse effects for women than it did for men (Smith et al. 2002:13).

The inverse relationship between age and recidivism rates is one of the first and most discussed findings in the criminological field (Hirschi and Gottfredson 1983:556–561; Farrington 1986:191–195; Moffitt 1993; Carlsson and Sarnecki 2015:33). The relationship between reoffending and immigrant background or race is less clear. Native-born individuals in Sweden and foreign-born individuals have about the same rate of recidivism (BRÅ 2012a). Studies from the US show that black males have higher rates of recidivism, in comparison to other groups (Mears et al. 2008) but when controlling for risk-factors, black individuals who have lower risk scores for recidivism still have a higher probability of being register for a re-offence. This suggests that pervasive racism and increased surveillance could explain a large part of the heterogeneity (Berry et al. 2018). Black individuals who reenter a racial, and unequal economic area, after being released from prison, have a higher risk of recidivism than those who do not, indicating that social bonds and strain are mediators for recidivism (Reisig et al. 2007).

5. Methodology

One of the main challenges with causal inference in social science and especially in criminology is the difficulties conducting research that includes randomised treatment and control groups. Estimating the causal effects of prolonged imprisonment requires a treatment group, which receives a 'dose' of some sanction, and the counterfactual: a control group which does not receive the treatment. Ideally, but highly unethically, setting for this kind of experiment would be if judges randomly differentiate offenders into two groups and assign one group with longer sentences than the other. One might argue that this already occurs since offenders do receive prison sentences of various lengths. However, the sentencing process lacks randomisation, meaning that it is not pure coincidence who receives a short versus long sentence but rather one's action and predisposition to criminal behaviour, resulting in selection bias where one group most likely include more individuals with a higher tendency of delinquency.

5.1. The Natural Experiment

In order to isolate causality, the present study exploits a natural experiment that occurred in 1993 when the Swedish parliament scrapped the 'half-time reform'. By repealing the reform, the required time incarcerated before being released for parole increased from half-time to two-thirds. Parol as an institution was introduced in 1906 in Sweden and has gone through several critical changes during its more than hundreds of years in existence. A committee[6] was appointed in 1979, by the then centre-right government, to examine the system of the correctional services (SOU 1986). In 1981 the committee published an interim report (SOU 1981) concluding that albeit having flaws, the parole system should remain. Alterations were, however, recommended in order to promote less overall incarceration, legal equality, and maintaining the harmonisation of Nordic criminal laws. A government bill was put forward, by the social democratic government, the following year that was in line with the proposals made by the committee and the bill was implemented in 1983. Parole after serving half of the prison term was now not only intended for offenders serving a minimum of two years but for all serving a sentence which includes a minimum effective time in prison of two months. Additionally, parole became mandatory from previously being facultative (Ekbom et al. 1996:164–167).

[6] *Fängelsekommitén* (Ju 1979:04)

The half-time reform survived only ten years and was dismantled July 1st, 1993 resulting in parole legislations being reverted in large parts to conditions before 1983. Offenders sentenced to less than two years became once again incarcerated for two-thirds of their prison term before parole but with a new required minimum time of one month incarcerated before parole (parole was still kept mandatory).[7] The increase in required incarceration time can be translated to an effective increase between 1 day, for offenders sentenced to just over 3 months in prison, to 117 days for offenders sentenced to 2 years in prison (see figure 2).

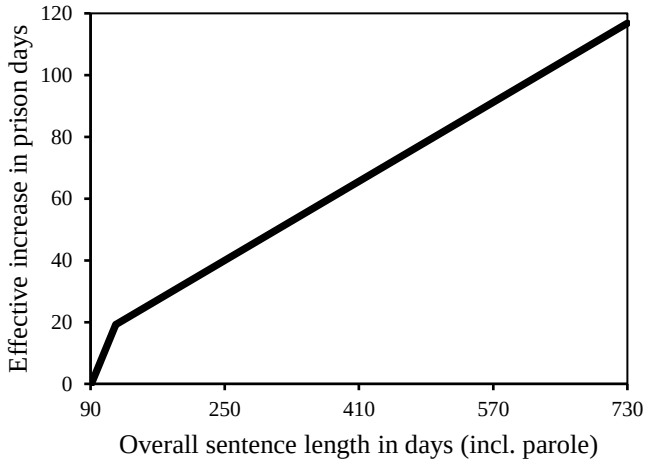

Figure 2. The effective increase in incarceration time.

The increase in required incarceration time before parole is the exogenous shock that the present study will exploit when estimating how increased prison sentences affect recidivism. In this study, the treatment group consists of adult offenders who have committed a crime between July 1st, 1993 and July 1st, 1994 (a period when offenders were released after serving two-thirds of their prison sentence) that resulted in a prison sentence. Consequently, the historical control group consists of offenders who have committed a crime between July 1st, 1992 and June 30th, 1993 (a period when offenders were released after serving half of their prison sentence) that resulted in a prison sentence. The overall research design of the study is illustrated in figure 3.

[7] Half-time parole for offenders senteced to minimum of two years was abolished in 1999 and two-thirds became standard (SOU 2017:69–71).

Figure 3. Research design based on the dismantling of the half-time reform on July 1st, 1993.

5.2. Data Source

The data used, when measuring crime in this study, is the Swedish conviction register, but additional administrative registers are applied, through matching by a unique personal identifier, to add information and validate the data. The dataset is part of the research project *The inequality of crime*, conducted at the Department of Criminology, Stockholm University. [8] Offenders in the study population are between the age of 18 and 75, who have received their first prison sentence of no more than two years, for a crime committed between July 1st, 1992 and July 1st, 1994.[9] [10] All offenders incarcerated less than 90 days are excluded in this study because the increase in prison time that the dismantling of the half-time reform generated did not provide an effective increase in prison time for these individuals (see figure 2). Figure 4 shows the distribution of prison length among the offenders included in this study.

Every Swedish resident has a unique personal identification number enabling a crossmatching between the Swedish conviction register, and other administrative registers such as the Swedish mortality register, used to identify and exclude offenders who have deceased within the follow-up period, and the Swedish migration registered, used to identify and exclude offenders who have emigrated within the follow-up period. Furthermore, individuals who have received a deportation order have been excluded from the data material. The requirement of a personal identification number consequently means that individuals who are not registered residents or individuals who do not have a personal identification number (i.e. newly migrated individuals) are excluded.

[8] The dataset used in this study has been created by Fredrik Sivertsson (Department of Criminology).
[9] In Sweden, offenders under the age of 18 are in general not sentenced to prison but instead institutional care of young persons but exeptions to this does occure for particularly serious crime. 46 adolescents under the age of 18 was initially found in the dataset but droped because of their unique characteristics and low number. Additionally, individuals under the age of 18 are even more rare nowadays, than in the 1990s (SVT 2013), making their presence in the dataset even more problematic (e.g. external validity), if kept.
[10] Principle offence if multiple offences (see section *5.4.3. independent variables* for a discussion regarding principal offence).

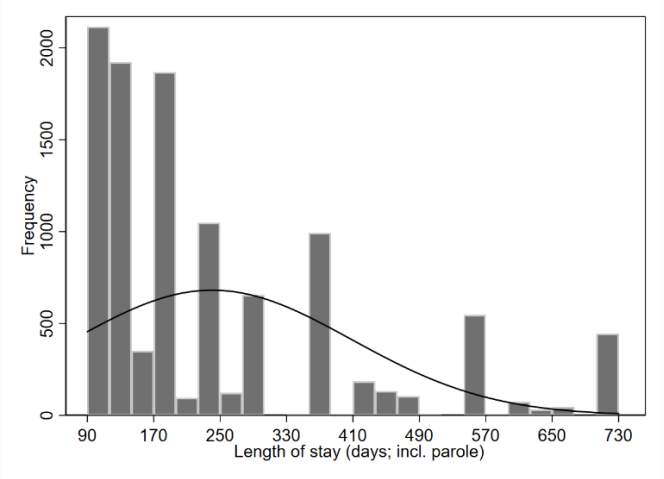

Figure 4. Distribution of length of stay (in days; parole included; N = 10 454).

The main strength of this dataset is that it is a comprehensive and complete population, from a high-quality longitudinal register (see Hovde Lyngstad and Skardhamar 2011 for an overview of Scandinavian register data and its potentials for criminological research) strengthening the validity and extensive follow-up periods. The Swedish conviction register has relatively high coverage in relation to of actual crimes rate due to the 'legality principle', which stipulates that police and prosecutors must report all cases of crime (see Lappi-Seppälä and Tonry 2011). Police reports are, also, non-negotiable, meaning that the prosecutor and the defendant's lawyer can not between them decide to file or dismiss charges. Additionally, statistics from International crime victims survey (Dijk et al. 2007:110) show that Sweden is among the top countries as regards the extent to which members of the general public report crime to the police.

5.3. Methodological Considerations

When using a historical control group, one must consider any historical change or any other circumstances that might affect the two groups' comparability (Shadish et al. 2002:chap. 5). Two such events could be the Swedish financial crisis 1990-1994 that lead to a quadrupling of unemployment rates and substantial cuts to welfare expenditure (see Bergmark and Palme 2003) and the Yugoslav wars. The wars forced people to seek asylum elsewhere, and Sweden received 90 000 refugees between 1992 and 1993. Victim surveys show that victimisation (of violence, threats or theft) did not increase during the 1990s (SCB 2004) indicating that the finacial crisis might not have had an overall impacted on crime rates.[11] Regarding refugees

[11] Nilsson and Estrada (2003:668-670) has, however, shown that victimization during the 1990s did increase but primarly for low-income groups.

from former Yugoslavia, no available statistics implies that these individuals are more prone to recidivism.

Another set of historical events that have potentially disturbed the comparability are two policy reforms. Minor drug offence became on the same date as the half-time reform was scrapped, punishable by up to six months in jail, from previously being an offence punished by the imposition of a fine (Tham 2018:104–106; BRÅ 2000:13–14). More importantly, the change in law meant that police received the authority to demand urine samples from individuals who were suspected of having consumed drugs. Individuals in the control group who were released from prison prior to the law change might, therefore, have had a lower likelihood of being convicted of a drug offence, in comparison to individuals in the treatment group who have, during their whole follow-up period experienced a more intensive control. However, this is only the case during a small duration in the follow-up period for the control group.

The second policy reform that possibly effects the comparability between the treatment and control group is the community police reform.[12] Its purpose was in part to decentralise the police (SOU 1985:62) and was implemented between 1993 and 1998 in several steps (BRÅ 2001). The clearance rate for the police decreased during these years, and some argue that the reason why the clearance rate decrease was because of the reform came in a time of austerity due to the financial crisis, which resulted in resource deficiency within the police organisation (BRÅ 2001; BRÅ 2002:35–37). A reduction in the clearance rate after 1993 could mean that there was, during a short period, possibly a higher likelihood of prosecution for individuals in the control group in comparison to the treatment group who were affected by the reform during their whole period.

Lastly, two issues that are not historical events but still important issues to underline. Firstly, not all inmates are being released on parole. Roughly three quarters are being released for parole, while the last quarter is being kept incarcerated for the duration of their entire sentence.[13] These individuals could, unfortunately, not be identified in the dataset. There are, therefore, individuals in the material who have not been subjected to an increase in required incarceration time before release. There are no reasons, however, to expect systematic bias in the prevalence between the treatment and control group of these individuals.

[12] *Närpolisreformen*
[13] Due to difficulties obtaining statistics from 1992-1994, presented numbers are for 2017 (Kriminalvården 2017:61).

Secondly, recidivism is measured as documented reconviction, meaning that a large number of potential reoffences will not be picked up by this study (i.e. the dark figure of crime). Unregistered offences could become a methodological problem if there is a systematic pattern in the selection process; whose crime does and does not end up in the judiciary system ending with a sample bias. However, under-reporting is presumedly not a considerable issue for this study since it is not systematic; levels of the dark figure of crime is likely similar for both the treatment and control group. Furthermore, the present study has the ability to test if there are signs for disturbances in the comparability between the treatment and control group, by including what is called a sensitivity analysis.

5.3.1. Sensitivity Analysis

In short, sensitivity analysis is used to determine the robustness of estimations by examining the extent to which results are affected by changes in methods, models, or data (Porta 2015:259). Two sensitivity analysis will be performed in this study. The first test narrows down the study population from two years to two weeks, with a control group consisting of offenders who have been convicted for a crime one week up until July 1st, 1993 and a treatment group consisting of offenders who have been convicted for a crime committed one week from July 1st, 1993 and forward. This test hopefully excludes possibilities of disturbances in comparability from policy changes, changes in levels of the dark figure of crime and demographic or socioeconomic transformations during the time of the analytical sample. The second analysis uses the whole population but excludes drug crimes circumventing the possible effects of the stricter control of drug users, which possibly have led to higher rates of drug convictions.

5.4. Measures & Operationalization

5.4.1. The Outcome Variable: Recidivism

Future criminal behaviour, which acts as the dependent variable in this study, is measured through the dichotomous indicator reconviction, with 0 representing no reconviction and 1 representing reconviction. As previously mentioned, one of the strengths with the present study is its extensive follow-up period of five years, which is a feature few equivalent studies manage to include (Villettaz et al. 2015:9). The follow-up period is broken down into three follow-up periods: recidivism within 1, 3 and 5 years. This setup enables both analyses of short-term and long-term effects of increased prison length. The literature on recidivism describes that released

offenders experience various difficulties in the period shortly after release increasing risk of early recidivism (Ramakers et al. 2014) and recidivism rates are at it highest during the first year of release, as compared to subsequent years. In Sweden, recidivism rates for prisoners released in 2011 reached 40 % after three years but had already reached 26 % after one year (BRÅ 2018:16–17).

The date of the conviction acts as an approximation when the prison sentence initiates and is furthermore the date when the follow-up period starts. It means, however, that a certain period of the follow-up includes time incarcerated and subsequently the parole period. This could affect the comparability between the treatment and control group since individuals in the treatment group spend more time, during their follow-up period, not incarcerated (because of the difference in parole time). The follow-up period of 5 years does, hopefully, minimise the disturbance in comparability since the difference in time not spent incarcerated, between the treatment and control group, is small relative to the extensive follow-up period. It does, nevertheless, highlight the limitation with the 1-year follow-up which in large parts captures individuals who are incarcerated (since mean sentence length is 7.5 months).

5.4.2. Parole Reform Variable

A dummy variable is created for the end of the half-time reform with the value 0 for individuals who have committed a crime before July 1st, 1993 and the value 1 for individuals who have committed a crime on July 1st, 1993 and onward.[14] Offenders who committed separate offences during both periods were randomly put into one of the two groups. The advantage of the dummy variable is that it acts as an instrumental variable (Morgan and Winship 2015:chap. 7) that is exogenous[15] and its shock has a direct impact on the independent variable incarceration length (see 'The natural experiment' section). It has, at least in a theoretical sense, an effect on recidivism but only through the endogenous[16] variable incarceration time (see figure 5). By the introduction of the instrumental variable, the model isolates a variance in incarceration time that is not affected by the error term: the behaviour of the offender.

[14] If an individual has commited multiple crimes (in one conviction) the specified date is the date when the principal offence was commited (see section *5.4.3. independent variables* for a discussion regarding principal offence).

[15] An exogenous variable act as a factor in a model that is independent from the other variables in use and is not determined by the causality by the other variables; offenders has no impact on the exogenous variable (but instead determined by lawmakers).

[16] An endogenous variable is, unlike exogenous variable, determined by the functional relationship by the model; offenders has a direct impact on the endogenous variable (by the acts of deviance and its severity).

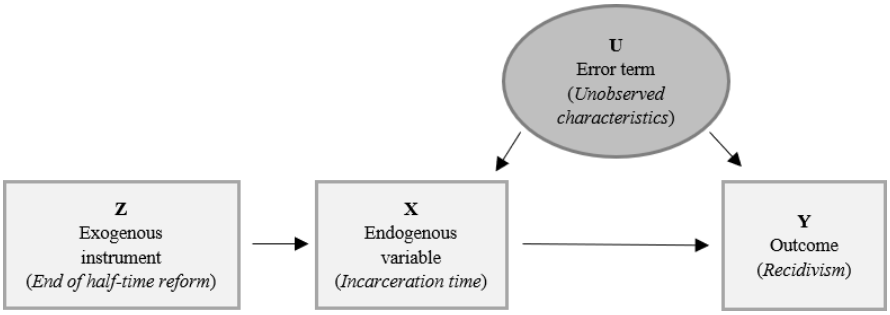

Figure 5. Illustration of the use of an instrumental variable

5.4.3. Independent Variables

The main variable in this study is incarceration time, which is measured as imposed prison sentence in days specified in the court decision. Note that the specified sentence includes parole and not effective time incarcerated. In line with the dose-response framework, imposed prison days, initially, a continuous variable, was transformed into a new six-category ordinal variable and used for the second research question regarding the dose-response relationship. Each value, except for the first group, has a cutoff of four months with imposed incarceration lengths of 3-4, 5-8, 9-12, 13-16, 17-20, and 21-24 months.

The third research question in this study addresses the effects that the transition from half-time to two-thirds parole had depending on gender, age group, immigrant background, and offence type. Nagin et al. (2009:135) describes in their systematic review that these characteristics and demographic variables (and prior record) are a minimum necessary set of variables to incorporate and one of the reasons why they are included in this study.

Gender is a dichotomous variable with the available values of woman or man. Age groups are broken down into the categories 18-20, 21-24, 25-29, 30-49, and 50> years. The shorter cutoffs for the first three age groups (in comparison to the other age groups) is because it enables an analysis of how recidivism is unfolded for younger individuals who show the highest prevalence of recidivism (Sivertsson 2018a:63–68). Additionally, offenders below the age of 21 receive, by law, a reduced prison sentence (up to 33 per cent lower) than offenders over the age of 20 (see SOU 2018). The age group consisting of offenders 18-20 has thus additional analytic importance. Immigrant background is a four-category variable which can assume the values i) born in Sweden, at least one Swedish-born parent, ii) born in Sweden, no Swedish-born parent, iii) born in the rest of Europe, USA, Canada, Australia or New Zealand, or iv) born in the rest of the world.

Offence type is a nominal five-category variable which can assume the values i) violent offence, ii) sex offence, iii) property offence iv) drug offence, v) traffic offence, and vi)

other offences[17] (see table A.1 for categorisation of offence types). One of the main strengths with the dataset used for this study is that it is coded so that it does not only include principal offences[18] but all registered offences specified in a conviction. The method of reporting by principal offences means that in a conviction with multiple offences, only the most severe offence, according to the range of punishment, is reported (Sivertsson 2018a:62; BRÅ 2006:41). Not accounting for this might risk underestimating less serious offences, and this study avoids this by using a dataset that includes all registered offences, regardless of severity. Table 1 shows summary statistics of the independent variables, including a test of the mean tendencies between the treatment and the control groups.

Table 1. Descriptive characteristics of the treatment and control group (N= 10 454)

	Control group	Treatment group	Difference
Demography			
Male offenders	0.94	0.95	0.00
Female offenders	0.06	0.05	0.00
Born in Sweden, at least one Swedish-born parent	0.70	0.71	0.01
Born in Sweden, no Swedish-born parent	0.05	0.06	0.00
Born in the rest of Europe, USA, Canada, Australia, or New Zealand	0.16	0.14	-0.02***
Born in the rest of the world	0.08	0.09	0.01
Offences			
Mean age when the offence was committed	33.75	33.5	-0.25
Violent offence	0.39	0.39	0.00
Sexual offence	0.03	0.03	0.00
Property offence	0.63	0.60	-0.03**
Traffic offence	0.29	0.29	0.00
Drug offence	0.20	0.22	0.02**
Other offence	0.28	0.28	0.00
Mean sentence length (in days)	246.00	234.36	-11.64***
Number of observations	5 676	5 067	

* p<0.05; ** p<0.01; *** p<0.001 for t-test of difference (rounded digits) between control and treatment group.

5.4.4. Control Variable

Number of entry crimes during the population period (July 1st, 1992-July 1st, 1994) was not constant but had a rather slight negative trend (see figure 4) which means that a control variable that accounts for the change in crime trend needs to be included. Not including a control variable for this may result in an underestimation of a potential decline in recidivism. The control variable indicates the distance in days between the first day of the population period and the crime date. For example, an individual who has committed an entry offence on July

[17] This category is highly heterogeneous due to it being a wide variety of crimes.
[18] *Huvudbrottsprincipen*

5th, 1992 has the value of 5. Furthermore, as seen in figure 6, the crime trend is slightly curve-linear, and this is also accounted for by including a squared version of the variable values making it a second-degree polynomial function which generates a regression with a potentially better fit.[19]

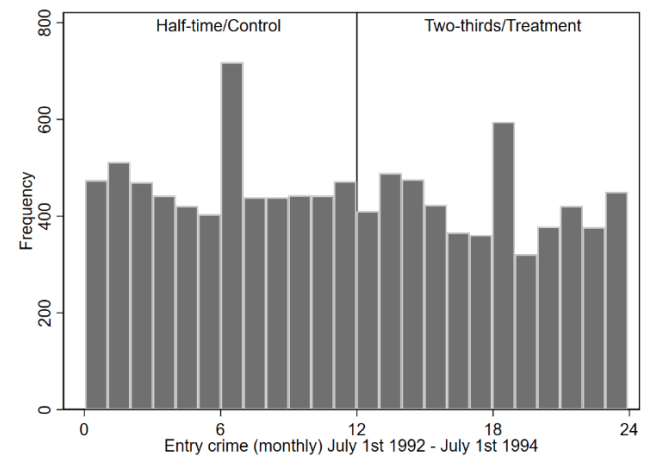

Figure 6. Date of crime (entry crime) during population period (reference line, date of policy reform).

5.5. Linear Probability Model

Estimating the incidence of recidivism among released offenders will be done using multiple linear regression. However, since the outcome variable is discrete 0/1, the regression model becomes a linear probability model (LPM). Linear regression uses the method of ordinary least square (OLS) to determine the line, with the best fit, that most effective explains the average correlation between variables. The results from a regression with LPM yields these results in terms of probability changes (Mood 2010:78–79). A value of 0.00 represents a 0 per cent probability of recidivism and 1.00 represents 100 per cent probability of recidivism.

A more common approach when using a binary dependent variable is to use a logit model.[20] The argument against LPM is that the predicted probabilities presented may fall outside the range of 0 to 1 and thus risk losing explanatory value. A second issue is concerning heteroscedasticity when using OLS to predict binary outcome values. One of the assumptions of linear regression is that residuals must be normally distributed in order to provide accurate estimates (Mood 2010:78). Ignoring this assumption could lead to wrong error terms and consequently, inaccurate p-values. In order to correct for this, heteroscedasticity-robust

[19] A third-degree polynomial function was also tried against the data but without receiving any improved fit.
[20] LPM is also less of a popular method in sociology and more frequent in economics (Mood 2010:78).

standard errors (also know as robust standard errors) will be used when presenting the results (Mood 2010:81).

The shortcomings of a logit model, and why LMP is more favourable for this study, is because logistic regressions could potentially produce biased results, leading to inaccurate conclusions (Allison 1999; Hellevik 2009; Mood 2010; Williams 2011). This is due to odds ratios being sensitive to error variance (the unobserved/unmeasured variance), in the dependent variable, across groups or samples, and conceivably lead to incorrect conclusions regarding differences (e.g. claims of differences when there does not exist any or vice versa).

The constructed estimation equation for this study is the following:

$$y_j = f(\text{Crime\_Trend}_j) + \alpha Reform_j + X_j\beta + u_j \quad (1)$$

where y_j is the relevant outcome (indicator if released offender $_j$ has been reconvicted, with follow-up periods of 1, 3 and 5 years); $f(\cdot)$ is a first- and second-degree polynomial in distance in time from the first day of the population period to the entry crime (to control for the downward and curve-linear trend in crimes during the population period); $Reformj$ is an indicator of whether inmate j is required to serve two-thirds of their prison sentence before parole; X_j is a vector of covariates (the demographic and offence type variables). u_j is an unobserved error term.

5.5.1. Interaction Effects

Interaction effects will be used when estimating the effects of the dose-response relationship, and the question concerning how different demographic groups and individuals convicted for specific offence types are affected by an increase in incarceration length. An explanatory variable might have an increased or decreased impact on an outcome depending on the interplay with other variable and estimating this is what is referred to as an interaction effect (Field 2017:chap. 11:3).

The interaction effect between the parole reform dummy and the different incarceration lengths of 3-4, 5-8, 9-12, 13-16, 17-20, and 21-24 months will produce the dose-response relationship between specific increases in incarceration time and their impact on the predicted probability of recidivism. For instance, offenders serving 21-24 months were subjected to an effective increase in incarceration time of 98-117 additional incarceration days

when the required incarceration time before parole was extended to two-thirds, from previously half-time.

The only variable not being measured through interaction is gender. This is because the initial interaction between gender and the reform dummy rendered a too high (>10) variance inflation factor (VIF) value when running the regression diagnostics, indicating that the interaction had problem with multicollinearity (see table B.1-6 for VIF values). Instead, the regression for gender is done separately; one for females and one for males.

Presenting interaction effects are often complicated and intuitively difficult to interpret, and therefore presented in this study by plotting the marginal effects, from the interaction term, on a graph. This is done by using the *marginsplot* command in Stata (see Williams 2012). Regressions tables and the marginal effects tables are included in the Appendix.

5.6. Ethical Considerations

In accordance with the General Data Protection Regulation (GDPR), individual-level data are seen as 'personal data' and research conducted on sensitive data requires an ethical approval according to §21 of the Swedish Ethical Review Act. As mentioned in the data source section, the data material for this study is part of a larger research project, and the project has been approved[21] by the Ethical Review Board in Stockholm and therefore, in accordance with relevant legislation.

Furthermore, the dataset used in this study has not been stored on a private disk but is instead accessed through Statistics Sweden (*Statistiska centralbyrån*) Microdata Online Access (MONA) system. One of the four main concepts stated by The Swedish Research Council, which should be taken into consideration when conducting research, is *confidentiality* (Vetenskapsrådet 2017). As a researcher, one has the obligation according to the confidentiality concept "not to communicate information given in confidence, and entails protection against unauthorised persons partaking of the information" (Vetenskapsrådet 2017:40). Although the individuals included in this study have been anonymized, processing the data through on a secured disk has ensured that the handling of the data has been done in line with the confidentiality principle.

[21] Reference number: 2016/46-31/5

6. Results

The results section will begin by presenting the descriptive statistics for each of the explanatory variables included in this study, for the treatment group (TG) and control group (CG). It will then be followed by the results on how the overall increase in required incarceration before parole affected recidivism and subsequently the dose-response relationship which will show how specific doses of increased incarceration time affect offender's probability of recidivism. Lastly, the potential heterogeneity of the increase in required incarceration time will then be examined. Each of the sections is meant to answer the three research questions for this study separately.

6.1. Descriptive Statistics

Table C.1 presents recidivism rates in the treatment and control group broken down into incarceration time, demographic characteristics and offence types. As seen in the table, the treatment group has a 0.7 to 0.75 percentage point lower rate of recidivism throughout the three follow-up periods. Roughly 41.5 per cent in both groups reoffended within the first year from release which increased to a rate of about 73 per cent recidivism when including reoffense 5 years from release. None of the differences, regardless of the follow-up period, indicate any statistically significant differences between the treatment and control group in recidivism rates.

The table also reveals disparities between various imposed sentences for the treatment and control group. The largest differences between the groups can be observed for those serving 17-20 months, with treatment group having 5.3 percentage points lower rate during the 1-year follow-up, which thereafter increased to 7.6 percentage points differences during the 5-year follow-up period. Serving 5-8 months were the only incarceration length that produced a higher recidivism rate for the control group. The difference did, however, only occur during the 3- and 5-years follow-up period (up to 1.3 percentage points higher recidivism rate for the control group).

An important observation, regarding the descriptive statistics on incarceration length and recidivism, is that the overall small differences between the recidivism rates of the treatment and control group are due to the low differences in recidivism in the groups serving 3-4 and 4-8 months, which consists of more than two-thirds of the entire population. However, having served the longest incarceration time (21-24 months) yields a lower difference in recidivism, than the observed differences after having served 8-12 or 16-20 months, meaning that no clear covariation between incarceration length and recidivism can be discerned.

Breaking down the treatment and control group into demographic subgroups, starting with females and males, reveals a distinct gender gap in recidivism. While the male treatment group have a lower recidivism rate than the male control group, females in the treatment group tend to do worse than their respective control group. Recidivism rates for females in the treatment group are 8.1 percentage point higher than the control group during the first year from release and the difference increases to 11.2 percentage points differences when including recidivism 5 years from release. Males in the treatment group, on the other hand, have a stable 1.2 percentage points lower recidivism rate throughout the follow-up periods.

When comparing recidivism rates among different age groups, offenders between the ages of 18 and 24, show in all three follow-up periods the largest differences, with treatment group having the lower rate of recidivism. When observing recidivism during the 1-year follow-up period for ages of 18 to 24, the differences are about 2.9 percentage points but increases slightly to 3.7 percentage points when including recidivism 5 years from release. Offenders over the age of 50 are the only age group where the treatment group has a higher rate of recidivism throughout the follow-up periods than the control group, with 1.7 percentage points difference during the 1 year follow-up period that increases to 2.5 percentage points difference when including recidivism up to 5 years from release.

Immigrant background shows that individuals born outside of Sweden and who are in the treatment group have a higher recidivism rate than the corresponding control group during the 5-year follow-up period. The results are inverse for offenders born in Sweden; rates are lower for the treatment group than the control group.

Continuing with offence type , it can be noted that offenders sentenced for violent crime have the largest recidivism gap, with the treatment group having a lower rate, when analyzing recidivism within the first year (3.5 percentage point difference) but then decreases when including recidivism within five years from release (1.8 percentage point difference). Offenders incarcerated for a drug or property offence constitutes the only group where the treatment group has an overall larger recidivism rate than the control group, although the differences are small.

6.2. The Overall Effects of an Increase in Incarceration Time on Recidivism

Table 2 presents the estimates from the linear probability model on the probability of recidivism after the increase in required incarceration time before parole. Just as when interpreting OLS-regression, the estimate reflects the change in the dependent variable

associated with a one-unit change in the independent variable. Aside from the control variables for the crime trend (see section 5.3.4 Control variable), table 2 only contains one independent variable, which is a dummy variable representing the treatment group.

Table 2. Results from linear probability models on the risk for recidivism after treatment

	Follow-up period: 1 year	**Follow-up period: 3 years**	**Follow-up period: 5 years**
Two-thirds/Treatment	-0.0310	-0.0185	-0.0267
	(0.0193)	(0.0186)	(0.0177)
Constant	0.413***	0.674***	0.735***
	(0.0148)	(0.0141)	(0.0134)
Observations	10 454	10 076	9 703
R^2	0.000	0.000	0.000

Robust standard errors in parentheses. All estimates are after controlling for trend in entry crimes.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The three models in table 2 represent the three follow-up periods capturing recidivism committed within 1, 3 and 5 years from release. The outcomes show that although there is a tendency of reduced recidivism rates after the reform, none of the estimates for the reform/treatment group are statistically significant. Thus, the main message from table 2 is that exposure to increased incarceration time did not result in any significant difference, in comparison to the control group; both in regard to short-term and long-term recidivism.

6.3. Dose-Response Relationship

The relationship between extended incarceration time and recidivism will here be presented by means of interaction plots for each follow-up period. Each plot consists of five incarceration lengths for the treatment group and control group, respectively. The lengths and their corresponding effective increase in incarceration time (in parenthesis) are 3-4 months (1-19 days), 5-8 (20-39 days), 9-12 months (40-58 days), 13-16 months (59-78 days), 17-20 (79-97 days), and 21-24 months (98-117 days).

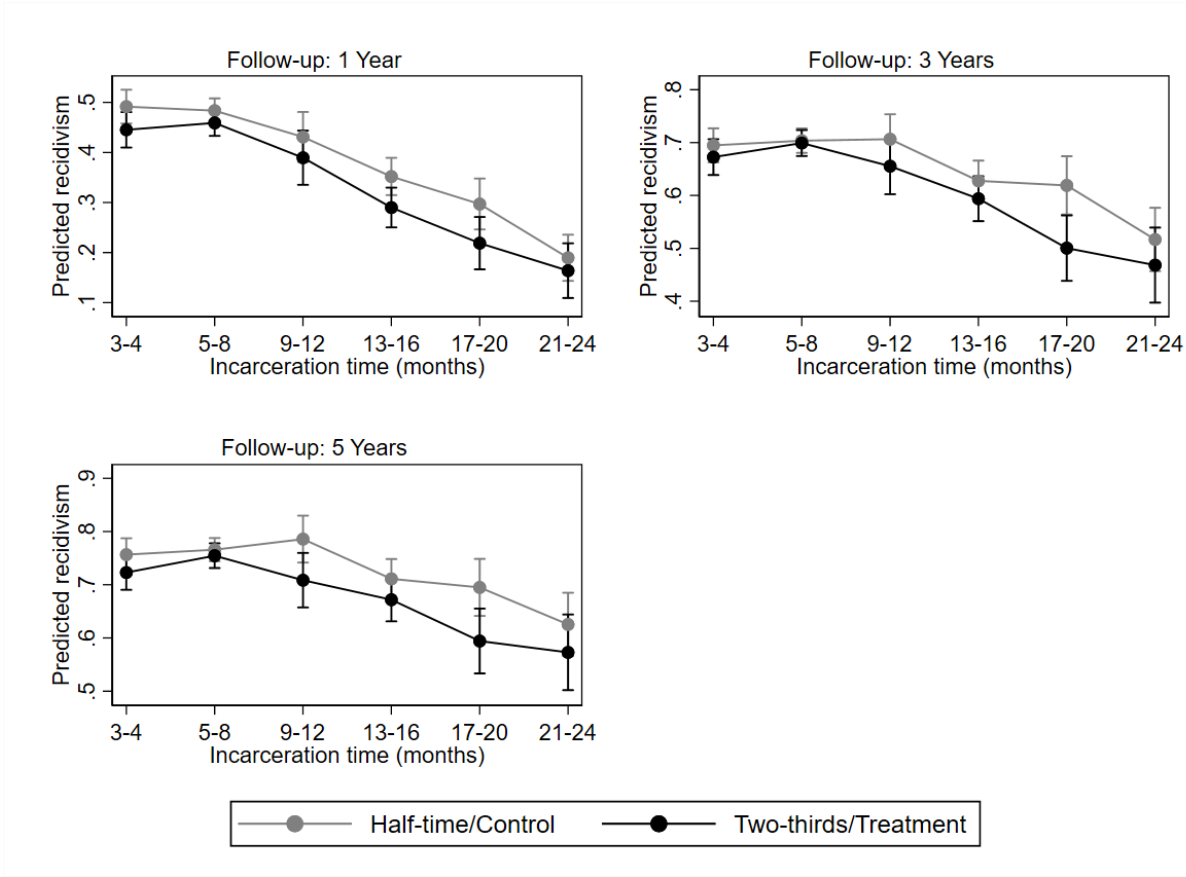

Figure 7. Predicted recidivism depending on incarceration time for control and treatment group during follow-up period 1, 3 and 5 years. 95% CI. Note different scales.

Figure 7 (see table D.1-6 for estimates) shows the margins plot for the interactions between incarceration lengths and the two-thirds/treatment dummy variable, based on the linear probability model. The overall outcome from the dose-response relationship is that none of the different doses of increased incarceration time resulted in any statistically significant difference in short-term nor long-term recidivism. The only estimate that was statistically significant was for individuals in the treatment group serving 17-20 months (CG: 61.9 %; TG: 50 %), with an effective increase of 79-97 days, when observing recidivism within 3 years from release. Their probability of recidivism declined with 11.9 percentage points, but the difference was reduced and became non-significant when including recidivism 5 years from release. There are tendencies for larger differences between the treatment and control group when observing incarceration lengths of 9 months and above, but the differences do not increase with each incremental increase but rather decreases when being exposed to the highest incarceration length of 21-24 months.

6.4. Incarceration Length Heterogeneity

Arguments have been made that there are heterogeneity in incarceration on various demographic or social groups (Mears et al. 2015), and this part of the study will explore if there is any evidence of heterogeneous effects of increased incarceration length in the empirical material in use. The predicted effects of increased incarceration time on recidivism for males and females will be presented separately in two tables. The results for age, immigrant background and offence type will then be presented by plotting the predicted probabilities of recidivism as margins plots.

6.4.1. Gender

Table 3 presents the estimates from a linear probability model on the probability of recidivism for females after the increase in required incarceration time before parole. It shows that none of the estimates for females are statistically significant. There are, nonetheless, tendencies for greater probabilities of recidivism after exposure to increased incarceration time. When observing recidivism during the 1- or 3-year follow-up period, the average probability of recidivism among females in the treatment group is approximately 10 percentage points higher than the probability among females in the control group. The estimate is almost halved, however, to 4.4 percentage points differences when including recidivism during the 5-year follow-up period. The estimates in Table 4 describe the results for males and they are, just as for females, not statistically significant but do, however, not indicate the same tendencies for increased probability of recidivism after increased incarceration time.

Table 3. Results from linear probability models on females' risk for recidivism after treatment

	Follow-up period: 1 year	**Follow-up period: 3 years**	**Follow-up period: 5 years**
Two-thirds/Treatment	0.1011	0.0950	0.0441
	(0.0828)	(0.0811)	(0.0793)
Constant	0.3852***	0.6548***	0.6642***
	(0.0604)	(0.0597)	(0.0587)
Observations	579	561	545
R^2	0.007	0.010	0.018

Robust standard errors in parentheses. All estimates are after controlling for trend in entry crimes.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4. Results from linear probability models on male's risk for recidivism after treatment

	Follow-up period: 1 year	**Follow-up period: 3 years**	**Follow-up period: 5 years**
Two-thirds/Treatment	-0.0385	-0.0254	-0.0311
	(0.0198)	(0.0191)	(0.0181)
Constant	0.4148***	0.6757***	0.7394***
	(0.0153)	(0.0145)	(0.0138)
Observations	9 875	9 515	9 158
R^2	0.000	0.000	0.000

Robust standard errors in parentheses. All estimates are after controlling for trend in entry crimes.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6.4.2. Age

Figure 8 presents the marginal plot (see table D.1-6 for estimates) for the different age groups and their individual interaction with the two-thirds/treatment dummy. The main finding is that none of the predicted probabilities for the interactions was statistically significant when observing short-term or long-term recidivism. The only occurrence of statistical significance was during the 3-year follow-up period for offenders between the ages of 21-24 (TG: 63.9%; CG 71.7%).

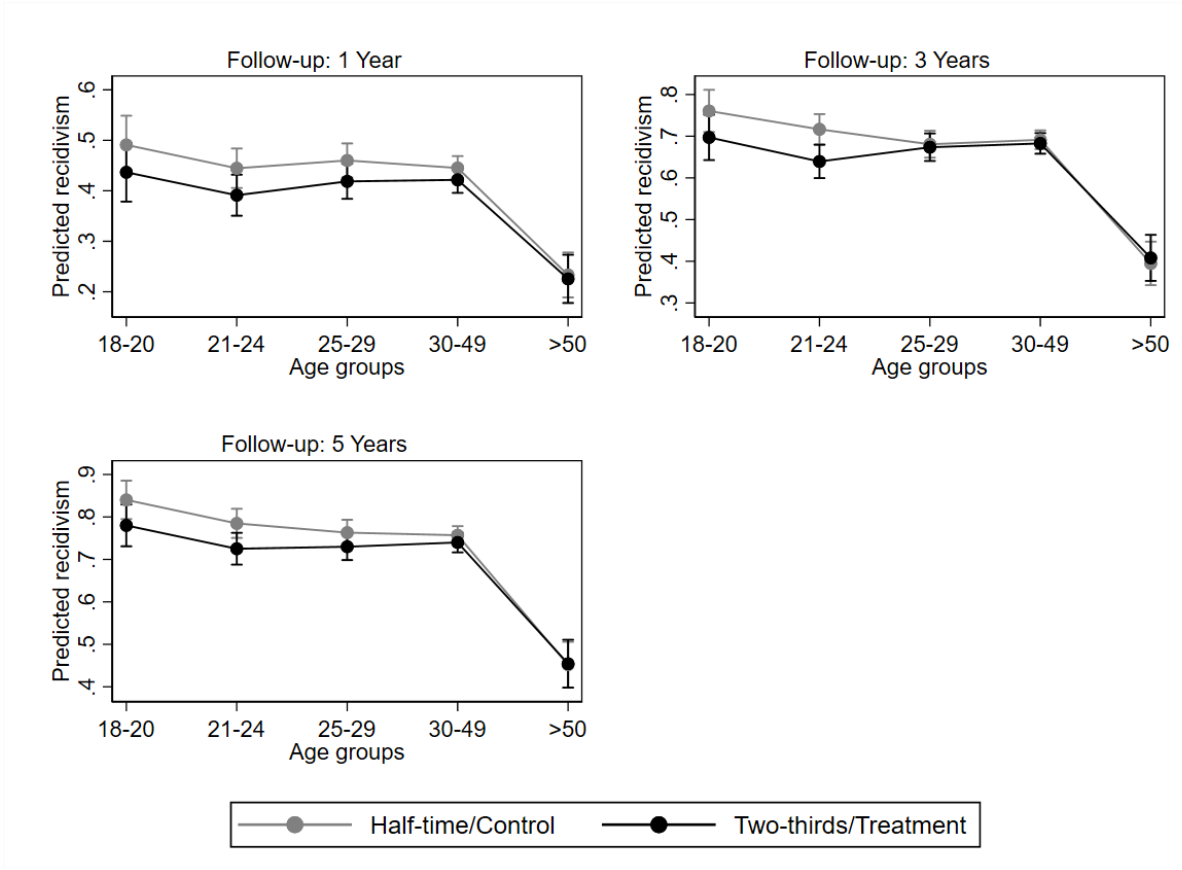

Figure 8. Predicted recidivism depending on age, for the control and treatment group during follow-up period 1, 3 and 5 years. 95% CI. Note different scales.

6.4.3. Immigrant Background

Figure 9 (see table F.1-6 for estimates) presents the marginal plot for the immigrant background and the individual interaction with the two-thirds/treatment dummy. Increasing incarceration time did not result in any statistically significant difference, regardless of background during the 3-years and 5-year follow-up periods. The only estimate that provides a significant probability was for individuals born in rest of Europe, USA, Canada, Australia, or New Zealand during the 1-year follow-up (CG: 38.6; TG 40.8) where the treatment group had a 2.2 percentage point greater predicted probability of recidivism.

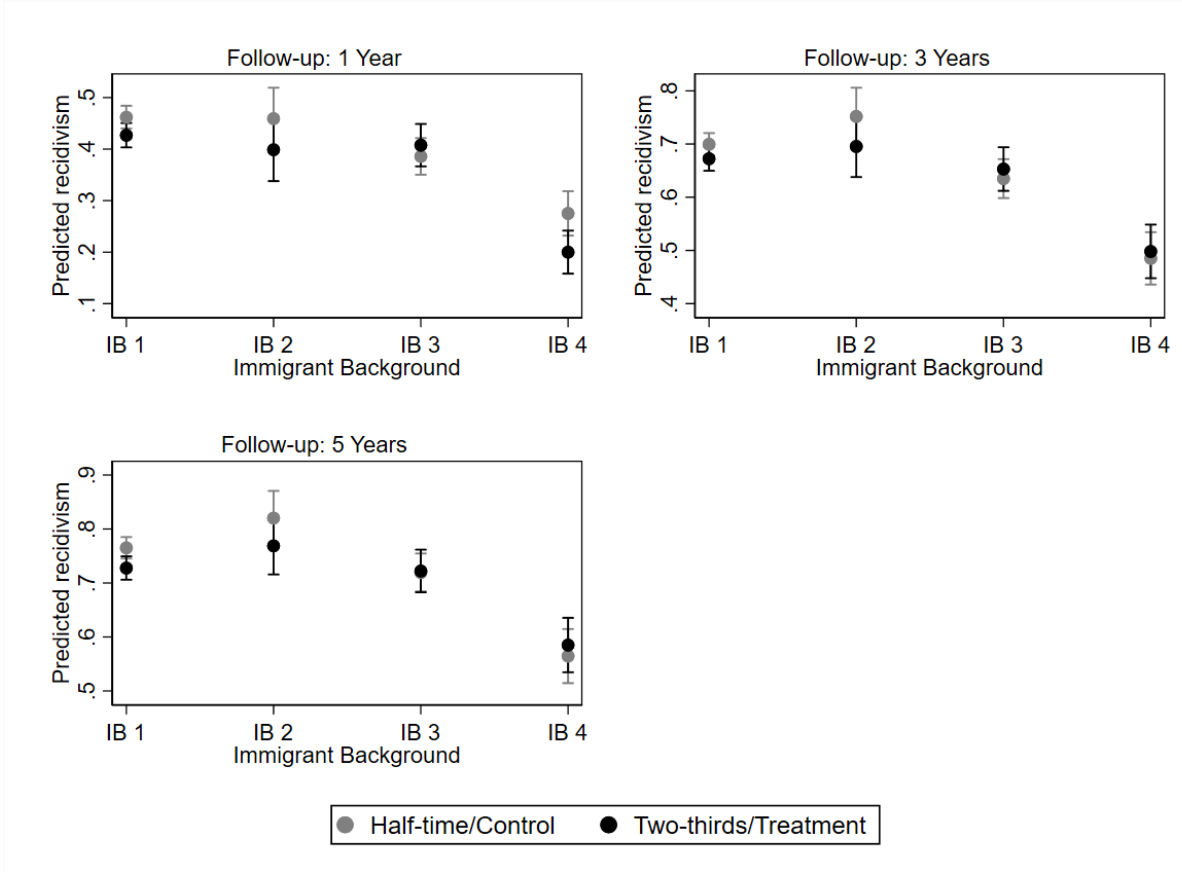

Figure 9. Predicted recidivism depending on immigrant background, for the control and treatment group during follow-up period 1, 3 and 5 years. 95% CI. Note different scales.
IB 1: born in Sweden, at least one Swedish-born parent; IB 2: born in Sweden, no Swedish-born parent; IB 3: born in the rest of Europe, USA, Canada, Australia, or New Zealand; IB 4: born in the rest of the world

6.4.4. Offence Type

Figure 10 (see table G.1-6 for estimates) presents the marginal plot for the interaction between offence types and two-thirds/treatment dummy. The results show no statistically significant difference after exposure to increased incarceration time, regardless of offence type or follow-up period. It is noteworthy to see the narrow confidence intervals (sex offences excepted) and

practically non-existing differences in the marginal effects between treatment and control group which demonstrates clearly the null-effect of the increase in incarceration time on recidivism.

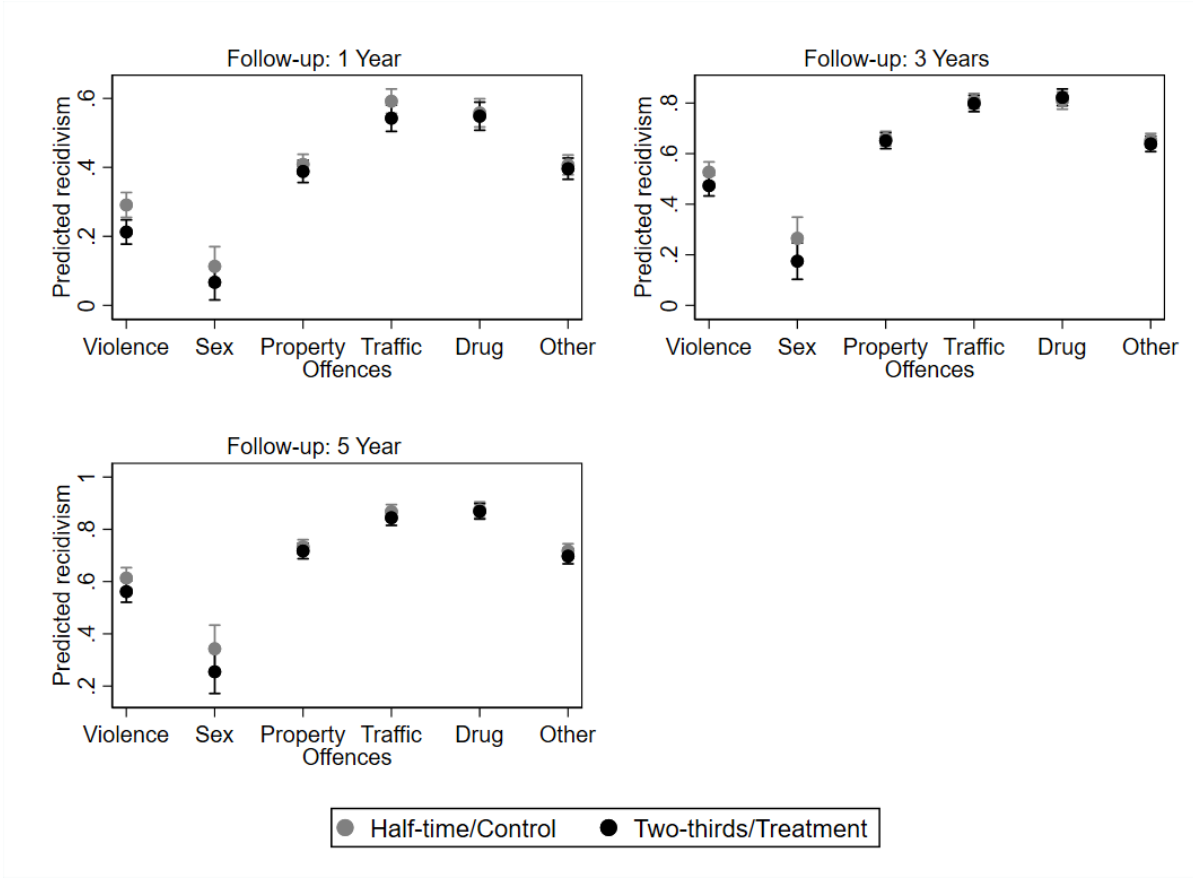

Figure 10. Predicted recidivism depending on offence type, for the control and treatment group during follow-up period 1, 3 and 5 years. 95% CI. Note different scales.

6.5. Sensitivity Analysis

Table H.1 in presents the estimates of the probability of recidivism after the increase in required incarceration time, but with the narrowed time frame of one week, respectively for the treatment and control group each. Table H.2 presents likewise the estimates for recidivism but with offenders convicted for a drug offence excluded. None of the two tests generated any statistically significant differences in the predicted probability of recidivism. These additional analyses strengthen the reliability of the previously described overall null-effect of increased incarceration time on recidivism.

7. Discussion & Conclusions

This part of the paper is divided into three separate sections that independently confront the outcomes in specific ways. The first part discusses the conclusions and connects it to the previous literature as well as the theoretical positions of prison being deterrent versus criminogenic. The second part discusses the identified limitations with the present study and how further research could approach the subject of the effects of incarceration length on recidivism. Lastly, the policy implications that this study has the ability to provide is outlined.

7.1. Understanding the Relationship Between Incarceration & Recidivism

The current study has exploited a natural experiment that occurred in Sweden where incarcerated offenders, due to policy reform, were subjected to an increase in required incarceration time before parole. This exogenous shock has enabled a setup of a treatment group, consisting of offenders exposed to an increase in incarceration time, and a control group, containing offenders who were not subjected to the increase in incarceration time. In one of the few studies conducted outside the US with an experimental nature, this study has been able to explore the causal relationship between increased incarceration time and recidivism.

The first research question in this study was how recidivism was affected by the increase in required incarceration length, due to the shift from half-time parole to two-thirds parole. One of the strengths of this study has been its extensive follow-up period of 5 years, which few studies covering the topic have been able to execute (Villettaz et al. 2006:3). Three separate follow-up periods of 1, 3 and 5 years have been used in order to capture short-term and long-term recidivism, and no statistically significant difference in the predicted probability of recidivism was observed after the increase in required incarceration time before parole - regardless of the time frame.

The findings of the present study are to some parts in line with those of previous dose-response studies. Two of these studies (out of nine in total) have observed Dutch offenders; individuals from a country with a penal climate close to that of Sweden. Both studies are in line with the outcomes of this study. The outcomes of the dose-response relationship mirror widely the outcomes of the study of Snodgrass et al. (2011), who studied offenders serving an average of 6,7 months incarcerated, which is close to the average time for the offenders in this study, with a 3 years follow-up period. Wermink et al. (2018) also observed a null-effect of incarceration time and recidivism, albeit only analysing recidivism during a 6-

months period.[22] Three of the US studies (Loughran et al. 2009; Meade et al. 2013; Rhodes et al. 2018) also observed little evidence of a relationship between incarceration length and recidivism.[23]

The overall null-effect of increased incarceration time on recidivism, witnessed in this study, puts the claim of prison's specific deterrent effect into question. One possible explanation of why the increase in incarceration time did not yield less recidivism could be that the specific deterrent capability of parole was reduced when offenders had to spend more time incarcerated and less time on parole. Parole is in part meant to be an opportunity for inmates to rehabilitate into society. Furthermore, an individual under parole is supervised by a parole officer who is making sure that the released inmate is not deviating, although this potentially produces a *net-widening* effect, expanding the scope of control for lower-level delinquency (see Cohen 1985:50–56).

As discussed in the theory section, substantial parts of the criminological theories on punishment and imprisonment argue that prison could potentially have coercive effects; exposure to incarcerations increases the risk of recidivism due to socialization processes inside prisons (that can enable continued socialization outside) or labelling effects, when released, that cause social stigmatization. Not observing any overall criminogenic effects in the results does, however, not dismiss the potential coercive effects that incarceration might entail. The population in this study was offenders serving less than two years incarcerated (average sentence length of 7.5 months). The modest increases in incarceration time might not be enough to affect socializing sufficiently, internalize a label or adapt to a deviant subculture that fosters antisocial behaviour. This could especially hold true for the lower end of the incarceration length.

Although the overall effect of the increase of required incarceration time reveals an interesting outcome, the dose-response relationship provides an even further understanding of how recidivism responds to specific increases in incarceration time. This leads to the second research question regarding the dose-response relationship between imposed incarceration length and recidivism. Regardless of the provided dosage, no significant differences were observed between the treatment group when observing short-term or long-term recidivism. The only estimate that was statistically significant was for individuals in the treatment group who

[22] Average sentence length in their study was 4.1 months, which is approxamitly 3.5 months lower than the average for the offenders in this study.
[23] Note that the study made by Loughran et al. (2009) were made on serious juvenile offenders which is a group not included in this study.

served 17-20 months, with an effective increase of 78-97 days, when observing recidivism within 3 years from release. The predicted probability of recidivism was 11.9 percentage points lower after receiving an increase of 78-97 days.

This might, at first glance, seem like an outcome that confirms the specific deterrent effect of higher doses of increased incarceration time. However, there are two circumstances questioning such an interpretation. Firstly, while the estimate is significant when observing the treatment group during the 3 years follow-up period, remember that it has no significant differences in either 1- or 5-years recidivism. This also pinpoints the importance of an extended follow-up period that manages to capture long-term recidivism. Secondly, the treatment group had tendencies, albeit not significant, of lower predicted probability of recidivism. However, the predicted probability of recidivism for the treatment group did not follow a linear path with an increase in the difference in predicted probability with each incremental dosage, but rather close to a curve-linear relationship. The predicted probability of recidivism for offenders who were subjected to the largest dose of 98-117 additional incarceration days was lower than the probability of recidivism for those who received the second largest dose: a dose of 78-97 additional incarceration days. The largest dose even generated smaller differences between the treatment and control group than the halved dose of additional 39-58 incarceration days. Interpreting and generalising the outcome, from the 3 years follow-up of the group serving an additional 78-97 incarceration days should, therefore, be made with caution.

What should however be noted is that a substantial part of the overall small difference between the treatment and control group, described when discussing the first research question, is due to the modest difference observed for offenders who received the two lowest doses, of 1-19 and 20-39 additional incarceration days. They stand together for approximately two-thirds of the entire prison population and thus have a large impact on the overall outcome. Not observing any considerable differences between the treatment and the control group in the lowest dosage does not come, however, as a great surprise since few would argue that a dozen additional days inside prison can substantially impact recidivism in any direction.

Both second-generation dose-response studies of Roach and Schanzenbach (2015) and Kuziemko (2012) observed a 1-1.5 percentage point decline in the probability of recidivism with a one-month extra prison sentence (the reduction in Roach and Schanzenbach's study was mainly during the first year from release). Such a decline in the probability of

recidivism was not observed in the dose-response, from this study, when adding 20-39 incarceration days but instead yielded a non-significant difference.

In general, the outcome of the dose-response relationship observed in this study echos the outcome of the second-generation studies that do not observe any relationship between additional incarceration and reoffending (Loughran et al. 2009; Meade et al. 2013; Rhodes et al. 2018; Wermink et al. 2018). The outcomes of this study also complement the qualitative studies with offenders who describes that the severity of the punishment does not deter them from subsequent involvement in crime (Raaijmakers et al. 2017).

7.2. Gender, Age, Immigrant Background & Offence Type

An important part of the understanding of how incarceration affects offenders and recidivism is to explore if incarceration exerts heterogeneous effects concerning various demographic groups. The third and last research question this study asked was if the effect of increased incarceration time on recidivism vary depending on gender, age, immigrant background, or offence type.

Marginal effects for males and females pointed at different directions, and although none of the estimates was statistically significant, they do merit a further discussion. Females' predicted probability increased with as much as 10 percentage points when incarceration times was increased. Even though the predicted probability must be interpreted with caution, due to large standard errors, it points to a potential need for further research into the heterogeneous effects of incarceration depending on gender.

Absence of statistically significant estimates for females is in part because they are only a small part of the study population but also because of the inclusion of the control for the downward trend in entry crimes during the population observation period. A necessary and important control for statistical inference. As seen in the descriptive statistics in table C.1 no subpopulation – except females - displayed a statistically significant difference between the treatment and control group, in all three follow-up periods. There could, therefore, be a potential tendency of a criminogenic effect when increasing incarceration lengths for females.

The potential increase in recidivism for females, in comparison to males, is in line with some of the previous literature that likewise sees a more adverse effect of incarceration length for females (Smith et al. 2002:13). A theoretical explanation of why incarceration could have a more corrosive effect on females is that labelling and stigmatization might not manifest itself in the same way for females, as for men; transgressive behaviour by

females do potentially lead to more severe stigma making a re-entry to society, after incarceration, more difficult (see Steffensmeier and Allan 1996; De Li and MacKenzie 2003; Giordano et al. 2004). Using a Swedish dataset, Estrada and Nilsson (2012:17) observed that female offending is associated with more negative consequences, especially in terms of social exclusion among midlife females and thus the authors concluded: "that it appears that involvement in crime exacts a higher cost for female offenders".[24]

Moving on to the effects of increased incarceration length for different age groups, increasing incarceration time does not provide any statistically significant difference, regardless of age group, on short-term or long-term recidivism.[25] Individuals under the age of 25 who were subjected to increased incarceration time did, however, have the largest marginal effect, among the age groups in the predicted probability of recidivism. These estimates should, as previous non-significant estimates, be interpreted with caution and not regarded as proof for a treatment effect of increased incarceration length but rather that further enquiry could be of interest. Much of the literature on how juveniles and young adults are affected by incarceration focuses on custodial versus none-custodial sanctions and argues, for the most parts, that incarceration is criminogenic (BRÅ 2012b). It is surprising, with this in mind, to not see any indications of this in the probability of recidivism for young adults who were subjected to an increase in incarceration length. Not observing any considerable criminogenic effects on offenders over the age of 24 might not be all to unanticipated, considering the age-crime curve and the overall decline in crime after the age of 20 (see Sivertsson 2018b:5–6 for the Swedish age-crime curve) that might mitigate effects by an increase in incarceration length.

In regards of differences in recidivism depending on the immigrant background; increasing the required incarceration time before parole did not statistically significant affect recidivism regardless of immigrant background.[26] There are some difficulties extending the previous literature with the results regarding the immigrant background since an overwhelming

[24] There were signs in the present dataset of females over the age of 29 being the main group that caused female's increase in probability of recidivsm. Unfortionatly, becase females were a relativly small part of the population no further analysis of this was feasible due to large standard errors.

[25] The single statistically significant estimate was for individividuals between the ages of 21 and 24 who had a 7.7 percentage point decline in 3-year recidivism when exposed to increased required incarceration time before parole. Giving this occurrence any meaningfull explanation is difficult since there was a null-effect in both short-term and long-term recidivism.

[26] One exception did occure which were short-term recidivism for individuals born in born in rest of Europe, USA, Canada, Australia, or New Zealand, who had a 2.2 percentage point increase in the probability of recidivism after the increase in incarceration time. However, this effect declined and became none-significant when observing recidivism during a longer follow-up, as all other estimates.

part of the literature that studies the effects of incarceration is conducted in the US who, in their scholarly work, emphasise race rather than the immigrant background.

One drawback with the dataset concerning foreign-born individuals is that deported individuals are excluded from the dataset. This could explain why the base rate of recidivism is much lower for foreign-born individuals than native-borns. However, the importance of this study is not the differences in baseline recidivism rates between groups but rather the size of the differences in effect after increased incarceration time.

Lastly, concerning the effects of incarceration length on various offence types, the outcome from the interaction between offences types and the increase in incarceration time demonstrates clearly how non-deterrent an increase in incarceration time is. Other studies have observed heterogeneity in the effects across conviction offences (Rydberg and Clark 2016), but the results from the current study observe no statistically significant difference regardless of offence type. As mentioned in the result section, the small differences in marginal effects and the small standard errors demonstrate effectively the null-effect that increasing incarceration lengths have on recidivism.

7.3. Limitations & Suggestions for Further Research

When generalising the conclusions of this study on a larger context, three aspects regarding the external validity is of relevance: the time frame, the sentence length, and the social context. In order to exploit the natural experiment, the population used in this study had to be drawn from a period of almost thirty years back. Trying to replicate the study, with a different population or sample, would thus be of interest. The time gap could arguably limit external validity when trying to apply the conclusion to contemporary policies on incarceration and crime deterrence. This is, however, mainly an issue if there have been extensive changes in prison, living or societal conditions that might affect deterrence and prevalence of recidivism.[27]

Another issue that must be addressed is the delimitation in this study to offenders who have received a maximum penalty of two years in prison, and its effect on generalizability to offenders with more extensive sentences. Could the measured effect of incarceration on recidivism in this study be extended to offenders incarcerated beyond two years? Firstly, roughly 85 per cent of inmates in Sweden is serving a maximum sentence of two years (Kriminalvården 2017:38) meaning that the outcomes of this study apply to an overwhelming

[27] For example, having access to welfare programes reduces the overall prevalence of recidivism (Yang 2017) and thus cuts to public assistance programmes could lead to increased recidivism risks.

majority of inmates regardless how well it applies to more serious offenders. Secondly, estimating probabilities of recidivism for offenders serving more extensive sentences is problematic because a decline in recidivism for a long-term inmate does not necessarily translate to a specific deterrent effect but rather that the individual has 'aged out' of crime during their stay. Thus, the limitation to sentences less than two years could be a methodological advantage as it is not necessary to confront the difficulties of disentangling additional endogenous effects, such as the effects of maturation on recidivism risk. Loughran et al. (2009:711–712) observed a dramatic heterogeneity among offenders who served more than 15 months and concluded that it could be difficult to generalise outcomes from short-term sentences to long-term sentences. Likewise, Mears et al. (2016) found in their study that longer time served initially increases recidivism but only for offenders serving less than two years and a null-effect after two years. However, one way of estimating if long-term offenders display the same pattern as offenders serving a maximum sentence of two years is by trying to replicate this study but instead exploiting the almost identical natural experiment as this study took advantage of. In 1999 offenders serving more than two years were subjected to the same policy change regarding an increase in required incarceration time before being released for parole, from half-time to two-thirds.

On a similar note, the implications this study provides are somewhat limited to sentencing contexts that are similar to those of Sweden, especially when considering that this study only observes sentences lower than two years. As previously discussed in the punitive turn section and elsewhere, Sweden has a relatively moderate penal clime and not as harsh and not as extensive as, for example, in the US. Sweden is, however, not alone in having a moderate penal climate; parts of Western and Northern Europe have similar conditions, and thus generalizability is possible in broader contexts than just Sweden.

Lastly, it could be of interest in future research to estimate changes, using the present dataset, in survival time after the increase in incarceration time by using, for example, a proportional hazards model (see Altman 1991:387–388)**.** DeJong (1997) observed in her study difference in the timing of a rearrest depending on ties to conventional society, and similar analysis could be done with the current dataset in order to estimate how various living conditions affect recidivism.

7.4. Policy Implications

By using a robust counterfactual design that ensures causality, the outcomes of this study have both theoretical and policy implications. From a theoretical perspective, the observed null-effect on offenders' criminal trajectory after increasing incarceration time does challenge the perception that incarceration length has a specific deterrent effect on offenders. This has, obviously, essential policy implications. The first implication is that required incarceration time before parole could be reduced, without any considerable negative consequence. At the moment, discussions among Swedish politicians – across the political aisle – is that parole should either be abolished or only used exceptionally (SVT 2018b). The results of this study provide no evidence that limiting parole, and thus increase incarceration time, would reduce recidivism.

A second interpretation of the results is that it shows that sentences could be reduced with as much as four months. This stems from the fact that there were no statistically significant differences in the dose-responses; from smallest to the largest dose of additional incarceration days. A reduction in sentence length has considerable economic benefits. A quick back-of-the-envelope estimate shows that a four-month reduction in incarceration length would translate into an annual savings of approximately 400 000 SEK per inmate. [28] Reducing prison sentences would also address an urgent matter regarding the shortage of prison beds in Sweden. As much as one-third of all Swedish prisons are reported being overcrowded (SVT 2019) and reducing the average length of stay would substantially ease the pressure.

For policymakers, to suggest that prison sentences should be reduced might be perceived as provocative, in a time when politicians try to over-bid each other over who is the toughest on crime. Politics is, however, in part about prioritising. Reducing unnecessary costs for the correctional services could enable investments in schools, hospital, and other welfare programmes that benefit all citizens, instead of directing resources to incarcerate people for the sake of 'penal populism' and symbolic politics.

[28] The daily cost of an incarcerated individual in Sweden is 3 445 SEK (Kriminalvården 2019:21).

Literature

Akers, R. (2010). *Social Learning and Social Structure: A General Theory of Crime and Deviance*. New Brunswick & Lonon: Transactions Publishers.

Allison, P. D. (1999). Comparing Logit and Probit Coefficients Across Groups. *Sociological Methods & Research*, 28(2), pp. 186–208.

Altman, D. G. (1991). *Practical statistics for medical research*. Happman & Hall/CRC.

Andersson, R. (2002). *Kriminalpolitikens väsen*. Stockholm: Kriminologiska institutionen, Stockholms Universitet.

Balvig, F. (2005). When law and order returned to Denmark. *Journal of Scandinavian Studies in Criminology and Crime Prevention*, 5(2), pp. 167–187.

Barker, V. (2013). Nordic Exceptionalism revisited: Explaining the paradox of a Janus-faced penal regime. *Theoretical criminology*, 17(1), pp. 5–25.

Beccaria, C. (1764). *Beccaria: 'On Crimes and Punishments' and Other Writings*. Cambridge University Press.

Becker, G. S. (1968). Crime and punishment: An economic approach. In: *The economic dimensions of crime*. Springer, pp. 13–68.

Becker, H. (1963). Outsiders: Studies in the sociology of deviance.

Bentham, J. (1780). *An introduction to the principles of morals and legislation*. London.

Berecochea, J. E. & Jaman, D. (1981). *Time served in prison and parole outcome : an experimental study*. Sacramento: California: Department of Corrections. Article; Article/Report.

Bergmark, Å. & Palme, J. (2003). Welfare and the unemployment crisis: Sweden in the 1990s. *International Journal of Social Welfare*, 12(2), pp. 108–122.

Berry, K. R., Kennedy, S. C., Lloyd, M., Veeh, C. A. & Tripodi, S. J. (2018). The Intersectional Effects of Race and Gender on Time to Reincarceration. *Justice Quarterly*, pp. 1–30.

BRÅ (2000). *Kriminaliseringen av narkotikabruk*. Sweden: Brottsförebyggande rådet (BRÅ).

BRÅ (2001). *Hur – Var – Närpolis: En granskning av närpolisreformen*. Brottsförebyggande rådet (BRÅ).

BRÅ (2002). *Varför har brottsuppklaringen minskat? En analys av orsakerna*. Brottsförebyggande rådet (BRÅ).

BRÅ (2006). *Konsten att läsa statistik om brottslighet*. Brottsförebyggande rådet (BRÅ).

BRÅ (2012a). *Återfall i brott - Mönster i risken för återfall bland lagförda personer*. Stockholm: Brottsförebyggande rådet.

BRÅ (2012b). *Särbehandling av lagöverträdare 18–20 år*. Stockholm: Brottsförebyggande rådet.

BRÅ (2018). *Kriminalstatistik 2011 - Återfall i brott*. Stockholm: Brottsförebyggande rådet.

Braithwaite, J. (1989). *Crime, shame and reintegration*. Cambridge University Press.

Carlsson, C. & Sarnecki, J. (2015). *An introduction to life-course criminology*. Sage.

Clemmer, D. (1940). *The prison community*. New Braunfels, TX, US: Christopher Publishing House.

Cohen, S. (1985). *Visions of social control: Crime, punishment and classification*. Polity Press Cambridge.

Cook, P. J. (1980). Research in criminal deterrence: Laying the groundwork for the second decade. *Crime and justice*, 2, pp. 211–268.

De Li, S. & MacKenzie, D. L. (2003). The Gendered Effects of Adult Social Bonds on the Criminal Activities of Probationers. *Criminal Justice Review*, 28(2), pp. 278–298.

DeJong, C. (1997). Survival analysis and specific deterrence: Integrating theoretical and empirical models of recidivism. *Criminology*, 35(4), pp. 561–576.

Demker, M. & Duus-Otterström, G. (2009). Realigning criminal policy. Offender and victim in the Swedish party system over time. *International Review of Sociology*, 19(2), pp. 273–296.

Demker, M., Towns, A., Duus-Otterström, G. & Sebring, J. (2008). Fear and punishment in Sweden: Exploring penal attitudes. *Punishment & Society*, 10(3), pp. 319–332.

Deschenes, E., Turner, S. & Petersilia, J. R. (1995). *Intensive Community Supervision in Minnesota*. RAND Corporation. Product Page.

Dijk, J. J. M. van, Kesteren, J. van & Smit, P. (2007). *Criminal victimisation in international perspective: key findings from the 2004-2005 ICVS and EU ICS*. Den Haag: Boom Juridische Uitgevers : Wetenschappelijk Onderzoek-en Documentatiecentrum.

Ekbom, T., Engström, G. & Göransson, B. (1996). *Brott, straff och kriminalvård*. Stockholm: Natur och kultur.

Estrada, F. (2004). The Transformation of the Politics of Crime in High Crime Societies. *European Journal of Criminology*, 1(4), pp. 419–443.

Estrada, F. & Nilsson, A. (2012). Does It Cost More to Be a Female Offender? A Life-Course Study of Childhood Circumstances, Crime, Drug Abuse, and Living Conditions. *Feminist Criminology*, 7(3), pp. 196–219.

Farrington, D. P. (1986). Age and crime. *Crime and justice*, 7, pp. 189–250.

Field, A. (2017). *Discovering Statistics Using IBM SPSS Statistics*. Fifth edition. Thousand Oaks, CA: SAGE Publications Ltd.

Garland, D. (1991). *Punishment and modern society : a study in social theory*. Oxford: Clarendon.

Garland, D. (1996). The limits of the sovereign state: Strategies of Crime Control in Contemporary Society. *The British journal of criminology*, 36(4), pp. 445–471.

Garland, D. (2000). The culture of high crime societies. *British journal of criminology*, 40(3), pp. 347–375.

Garland, D. (2001). *The culture of control: Crime and social order in contemporary society*. University of Chicago Press.

Gendreau, P., Goggin, C. & Cullen, F. T. (1999). *The effects of prison sentences on recidivism*. Ottawa, Canada: Solicitor General.

Giordano, P. C., Cernkovich, S. A. & Lowery, A. R. (2004). A Long-Term Follow-Up of Serious Adolescent Female Offenders.

Hellevik, O. (2009). Linear versus logistic regression when the dependent variable is a dichotomy. *Quality & Quantity*, 43(1), pp. 59–74.

von Hirsch, H., Bottoms, A. W., Burney, E. & Wikström, P.-O. H. (1999). *Criminal Deterrence and Sentencing Severity*. Oxford: Hart Publishing.

Hirschi, T. (1969). *Causes of delinquency*. Berkeley: University of California Press.

Hirschi, T. & Gottfredson, M. (1983). Age and the explanation of crime. *American journal of sociology*, 89(3), pp. 552–584.

von Hofer, H. & Tham, H. (2013). Punishment in Sweden: A changing penal landscape. In: *Punishment in Europe*. Springer, pp. 33–57.

Jaman, D. R., Dickover, R. M. & Bennett, L. A. (1972). Parole outcome as a function of time served. *Brit. J. Criminology*, 12, p. 5.

Jerre, K. & Tham, H. (2010). Svenskarnas syn på straff, p. 132.

Jonson, C. L. (2010). The impact of imprisonment on reoffending: A meta-analysis.

Kraus, J. (1981). The effects of committal to a special school for truants. *International Journal of Offender Therapy and Comparative Criminology*, 25(2), pp. 130–138.

Kriminalvården (2017). Kriminalvård och Statistik 2017.

Kriminalvården (2019). Årsredovisning 2018.

Kuziemko, I. (2012). How should inmates be released from prison? An assessment of parole versus fixed-sentence regimes. *The Quarterly Journal of Economics*, 128(1), pp. 371–424.

Lappi-Seppälä, T. & Tonry, M. (2011). Crime, criminal justice, and criminology in the Nordic countries. *Crime and justice*, 40(1), pp. 1–32.

Loughran, T. A., Mulvey, E. P., Schubert, C. A., Fagan, J., Piquero, A. R. & Losoya, S. H. (2009). Estimating a dose-response relationship between length of stay and future recidivism in serious juvenile offenders. *Criminology*, 47(3), pp. 699–740.

Meade, B., Steiner, B., Makarios, M. & Travis, L. (2013). Estimating a dose–response relationship between time served in prison and recidivism. *Journal of research in crime and delinquency*, 50(4), pp. 525–550.

Mears, D. P., Cochran, J. C. & Bales, W. D. (2012). Gender differences in the effects of prison on recidivism. *Journal of Criminal Justice*, 40(5), pp. 370–378.

Mears, D. P., Cochran, J. C., Bales, W. D. & Bhati, A. S. (2016). Recidivism and Time Served in Prison. *The journal of criminal law and criminology*, 106(1), pp. 83–124.

Mears, D. P., Cochran, J. C. & Cullen, F. T. (2015). Incarceration heterogeneity and its implications for assessing the effectiveness of imprisonment on recidivism. *Criminal Justice Policy Review*, 26(7), pp. 691–712.

Mears, D. P., Wang, X., Hay, C. & Bales, W. D. (2008). Social ecology and recidivism: Implications for prisoner reentry. *Criminology*, 46(2), pp. 301–340.

Merton, R. K. (1938). Social Structure and Anomie. *American Sociological Review*, 3(5), pp. 672–682.

Moffitt, T. (1993). Adolescence-limited and life-course-persistent antisocial behavior: a developmental taxonomy. *Psychological review*, 100(4), p. 674.

Mood, C. (2010). Logistic regression: Why we cannot do what we think we can do, and what we can do about it. *European sociological review*, 26(1), pp. 67–82.

Morgan, S. L. & Winship, C. (2015). *Counterfactuals and causal inference*. Cambridge University Press.

Nagin, D. S. (1978). General deterrance: A review of the empirical evidence. In: Blumstein, A., Cohen, J., & Nagin, D. (eds.) *Deterrence and incapacitation : estimating the effects of criminal sanctions on crime rates*. Washington: National Academy of Sciences.

Nagin, D. S., Cullen, F. T. & Jonson, C. L. (2009). Imprisonment and reoffending. *Crime and justice*, 38(1), pp. 115–200.

Nilsson, A. & Estrada, F. (2003). Victimization, inequality and welfare during an economic recession: a study of self-reported victimization in Sweden 1988–99. *British Journal of Criminology*, 43(4), pp. 655–672.

O'Malley, P. (1999). Volatile and contradictory punishment. *Theoretical criminology*, 3(2), pp. 175–196.

Orsagh, T. & Chen, J.-R. (1988). The effect of time served on recidivism: An interdisciplinary theory. *Journal of Quantitative Criminology*, 4(2), pp. 155–171.

Porta, M. (2015). A Dictionary of Epidemiology, Sixth Edition. *American Journal of Epidemiology*, 181(8), pp. 633–634.

Pratt, J. (2007). *Penal populism*. Routledge.

Pratt, J. (2008). Scandinavian exceptionalism in an era of penal excess - Part I: The nature and roots of scandinavian exceptionalism. *The British journal of criminology*, 48(2), pp. 119–137.

Ramakers, A., Apel, R., Nieuwbeerta, P., Dirkzwager, A. & Van Wilsem, J. (2014). Imprisonment length and post-prison employment prospects. *Criminology*, 52(3), pp. 399–427.

Reisig, M. D., Bales, W. D., Hay, C. & Wang, X. (2007). The effect of racial inequality on black male recidivism. *Justice Quarterly*, 24(3), pp. 408–434.

Rhodes, W., Gaes, G. G., Kling, R. & Cutler, C. (2018). Relationship between prison length of stay and recidivism: A study using regression discontinuity and instrumental variables with multiple break points. *Criminology & Public Policy*, 17(3), pp. 731–769.

Roach, M. A. & Schanzenbach, M. M. (2015). The effect of prison sentence length on recidivism: Evidence from random judicial assignment. *Northwestern Law & Econ Research Paper*, (16–08).

Rydberg, J. & Clark, K. (2016). Variation in the incarceration length-recidivism dose–response relationship. *Journal of Criminal Justice*, 46, pp. 118–128.

Sampson, R. & Laub, J. (1993). *Crime in the Making: Pathways and Turning Points Through Life*. Harvard University Press.

Sampson, R. & Laub, J. (1997). *Developmental Theories of Crime and Delinquency*. New Brunswick, N.J.: Transaction Publishers.

Shadish, W. R., Cook, T. D. & Campbell, D. T. (2002). *Experimental and quasi-experimental designs for generalized causal inference*. Boston: Houghton Mifflin.

Sivertsson, F. (2018a). Adulthood-limited offending: How much is there to explain? *Journal of Criminal Justice*, 55, pp. 58–70.

Sivertsson, F. (2018b). Criminal Careers in the Long Run: Patterns and Predictions of Criminal Convictions across Age, Time, and Gender.

Smith, P., Gendreau, P. & Goggin, C. (2002). *The effects of prison sentences and intermediate sanctions on recidivism: General effects and individual differences*. Solicitor General Canada Ottawa, ON.

Snodgrass, G. M., Blokland, A. a. J., Haviland, A., Nieuwbeerta, P. & Nagin, D. S. (2011). Does the Time Cause the Crime? An Examination of the Relationship Between Time Served and Reoffending in the Netherlands*. *Criminology*, 49(4), pp. 1149–1194.

SOU (1981). *Villkorlig frigivning samt nämnder och lekmannamedverkan inom kriminalvården : delbetänkande*. Stockholm: LiberFörlag/Allmänna förl.

SOU (1985). *Inriktningen av polisverksamheten: Slutbetänkande*. Stockholm: Liber/Allmänna förl.

SOU (1986). *Påföljd för brott : Om straffskalor, påföljdsval, straffmätning och villkorlig frigivning m.m.* Stockholm: Liber/Allmänna förl.

SOU (2017). *Villkorlig frigivning – förstärkta åtgärder mot återfall i brott*. Stockholm: Wolters Kluwer.

SOU (2018). *Slopad straffrabatt för unga myndiga*. Stockholm: Norstedts juridik.

Stafford, M. C. & Warr, M. (1993). A reconceptualization of general and specific deterrence. *Journal of research in crime and delinquency*, 30(2), pp. 123–135.

SCB (2004). Offer för våld och egendomsbrott 1978-2002. Statistiska centralbyrån (SCB). Stockholm.

Steffensmeier, D. & Allan, E. (1996). Gender and Crime: Toward a Gendered Theory of Female Offending. *Annual Review of Sociology*, 22(1), pp. 459–487.

Sutherland, E. (1947). Principles of Criminology.

Sykes, G. M. (1958). The society of captives: A study of a maximum security prison.

Tannenbaum, F. (1938). *Crime and the community*. Columbia University Press.

Tham, H. (2001). Law and order as a leftist project? The case of Sweden. *Punishment & Society*, 3(3), pp. 409–426.

Tham, H. (2006). Brottsoffret, kriminalpolitiken och kriminologin. *Nordisk tidsskrift for Kriminalvidenskab*, 93(2).

Tham, H. (2018). *Kriminalpolitik: brott och straff i Sverige sedan 1965*. Norstedts Juridik AB.

Tham, H., Rönneling, A. & Rytterbro, L.-L. (2011). The emergence of the crime victim: Sweden in a Scandinavian context. *Crime and justice*, 40(1), pp. 555–611.

Vetenskapsrådet (2017). *Good research practice*. Stockholm.

Villettaz, P., Gillieron, G. & Killias, M. (2015). *The effects on re-offending of custodial vs non-custodial sanctions*. The Campbell Collaboration.

Villettaz, P., Killias, M. & Zoder, I. (2006). The effects of custodial vs non-custodial sentences on re-offending. *A systematic review of the state of knowledge. Campbell Collaboration Crime and Justice Group, Lausanne*.

Von Hofer, H. (2005). Crime and punishment in Scandinavia. *Crime and justice in Scandinavia*.

Wacquant, L. (2001). The penalisation of poverty and the rise of neo-liberalism. *European Journal on Criminal Policy and Research*, 9(4), pp. 401–412.

Wermink, H., Nieuwbeerta, P., Ramakers, A. A., de Keijser, J. W. & Dirkzwager, A. J. (2018). Short-term effects of imprisonment length on recidivism in the Netherlands. *Crime & Delinquency*, 64(8), pp. 1057–1093.

Williams, R. (2011). Fitting heterogeneous choice models with oglm. *The Stata Journal*, 10(4), pp. 540–567.

Williams, R. (2012). Using the Margins Command to Estimate and Interpret Adjusted Predictions and Marginal Effects. *The Stata Journal*, 12(2), pp. 308–331.

Yang, C. S. (2017). Does Public Assistance Reduce Recidivism? *American Economic Review*, 107(5), pp. 551–55.

Young, J. (1999). *The exclusive society: Social exclusion, crime and difference in late modernity*. Sage.

Other Sources

SVT (2013). Få ungdomar döms till fängelse. *Sveriges Radio*, [2019-04-26]

SVT (2018a). Politisk majoritet för att avskaffa villkorlig frigivning, [2019-05-01]

SVT (2018b). Förslaget: Slopad straffrabatt för unga brottslingar, [2019-05-01]

SVT (2019). Mer än vart tredje svenskt fängelse är överfullt, [2019-05-01]

Appendix

Appendix A. Categorisation of Offence Types

Table A.1. Categorisation of Offence Types

Offence type	Transgression of law
Violent offence	Assault
	Aggravated assault
	Violence against a public servant
	Homicide
	Robbery
	Unlawful threat
	Unlawful compulsion
	Gross Violation of Integrity
	Unlawful persecution
	Molestation
Sex offence	Rape
	Sexual coercion
	Sexual exploitation
Property offence	Theft
	Grand theft auto
	Shoplifting
Drug offence	Production of drugs
	Possession of drugs
	Distribution of drugs
	Use of drugs
Traffic offence	Recklessness in Traffic
	Driving without a license
	Unauthorized Deviation from Traffic Accident
	Driving Under the Influence
Other offences	All other offences

Appendix B. Variance Inflation Factor (VIF)

Table B.1. Reform/Treatment

Variable	VIF	1/VIF
crime_tren~1	18.14	0.055123
crime_tren~2	15.43	0.064815
reform_dummy	3.97	0.251916
Mean VIF	12.51	

Table B.2. Female/Male

Variable	VIF	1/VIF
crime_tren~1	18.18	0.055006
crime_tren~2	15.44	0.064776
reform_dummy	3.96	0.252235
Mean VIF	12.53	

Table B.3. Dose-response x Reform/Treatment

Variable	VIF	1/VIF
crime_tren~1	18.16	0.055078
crime_tren~2	15.44	0.064746
dose_respo~e		
2	3.48	0.287638
3	2.37	0.421974
4	2.76	0.362240
5	2.29	0.435782
6	2.08	0.480241
1.reform_d~y	8.07	0.123868
dose_respo~e# reform_dummy		
2 1	5.16	0.193675
3 1	2.42	0.413306
4 1	2.92	0.342246
5 1	2.34	0.427875
6 1	2.07	0.482476
Mean VIF	5.35	

Table B.4. Age groups x Reform/Treatment

Variable	VIF	1/VIF
crime_tren~1	18.15	0.055085
crime_tren~2	15.44	0.064756
age_groups		
1	2.07	0.484186
2	2.08	0.480154
3	2.13	0.468447
5	2.02	0.494678
1.reform_d~y	4.89	0.204546
age_groups# reform_dummy		
1 1	2.15	0.465982
2 1	2.27	0.440070
3 1	2.45	0.408347
5 1	2.11	0.474251
Mean VIF	5.07	

Table B.5. Immigrant background x Reform/Treatment

Variable	VIF	1/VIF
crime_tren~1	18.17	0.055045
crime_tren~2	15.45	0.064734
etno4kat		
2	2.02	0.494307
3	1.84	0.544461
4	2.02	0.495066
1.reform_d~y	4.36	0.229336
etno4kat# reform_dummy		
2 1	2.09	0.479373
3 1	1.97	0.507861
4 1	2.12	0.471155
Mean VIF	5.56	

Table B.6. Offence types x Reform/Treatment

Variable	VIF	1/VIF
crime_tren~1	18.18	0.055008
crime_tren~2	15.46	0.064687
convictions		
1	2.53	0.394709
2	2.12	0.470888
3	2.65	0.377380
4	2.49	0.401589
5	2.57	0.388895
1.reform_d~y	6.45	0.154963
convictions# reform_dummy		
1 1	2.78	0.359314
2 1	2.16	0.463626
3 1	3.08	0.324750
4 1	2.72	0.367012
5 1	2.83	0.353066
Mean VIF	5.08	

Appendix C. Descriptive Statistics

Table C.1. Descriptive statistics of recidivism.

	Follow-up period: 1 year[1]			**Follow-up period: 3 years**			**Follow-up period: 5 years**		
	Control group	Treatment group	Difference	Control group	Treatment group	Difference	Control group	Treatment group	Difference
Incarceration time (months)									
Whole period (N=5 514, N=4 949)	42.42	41.72	-0.7	67.06	66.31	-0.75	73.50	73.03	-0.74
3-4 (N=1 048, N=1 000)	48.00	45.90	-2.1	68.88	67.98	-0.9	74.54	73.63	-0.91
5-8 (N= 2 670, N=2 477)	47.15	47.23	0.08	69.75	70.58	0.83	75.44	76.72	1.28
9-12 (N=418, N=348)	41.87	40.23	-1.64	70.05	66.17	-3.88	77.44	72.05	-5.39
13-16 (N=747, N=626)	34.00	30.35	-3.65	62.15	60.10	-2.05	69.97	68.52	-1.45
17-20 (N=333, N=280)	28.53	23.21	-5.32	61.30	50.74	-10.56*	68.37	60.75	-7.62
21-24 (N=298, N=209)	17.79	17.70	-0.09	51.06	47.50	-3.56	61.40	58.55	-2.85
Gender									
Female (N=315, N=264)	40.00	48.11	8.11*	60.13	68.63	8.5*	64.65	75.81	11.16**
Male (N=5 199, N= 4 676)	42.57	41.36	-1.21	67.48	66.18	-1.3	74.05	72.87	-1.18
Age-groups									
18-20 (N=311, N=309)	47.91	44.98	-2.93	75.50	70.37	-5.13	82.94	79.24	-3.7
21-24 (N=735, N=680)	43.27	40.44	-2.83	71.09	64.60	-6.49**	77.39	73.72	-3.67
25-29 (N=1 073, N=1 022)	44.83	43.15	-1.68	67.50	67.97	0.47	75.23	74.14	-1.09
30-49 (N=3 012, N=3 281)	43.36	43.51	0.15	68.57	68.95	0.38	25.39	24.77	-0.62
50> (N=383, N=348)	22.19	23.85	1.66	38.90	41.44	2.54	44.13	46.60	2.47
Immigrant Background									
Born in Sweden, at least one Swedish-born parent (N=3 872, N=3 520)	44.99	44.03	-0.96	69.41	67.86	-1.55	75.42	73.97	-1.45
Born in Sweden, no Swedish-born parent (N=284, N=274)	44.72	41.24	-3.48	74.63	70.19	-4.44	80.93	78.13	-2.8
Born in the rest of Europe, USA, Canada, Australia, or New Zealand (N=866, N=663)	37.41	42.08	4.67	62.94	65.91	2.97	70.86	73.43	2.57
Born in the rest of the world (N=464, N=449)	26.29	21.38	-4.91	47.95	50.47	2.52	55.34	59.75	4.41

Table C.1. Descriptive statistics on recidivism. Continued

	Follow-up period: 1 year			**Follow-up period: 3 years**			**Follow-up period: 5 years**		
	Control group	Treatment group	Difference	Control group	Treatment group	Difference	Control group	Treatment group	Difference
Offence types									
Violent offence (N=2 140, N=1 953)	39.49	36.00	-3.49*	67.95	64.40	-3.55*	75.04	73.28	-1.76
Sexual offence (N=160, N=151)	11.25	13.25	2	28.67	25.85	-2.82	35.17	34.48	-0.69
Property offence (N=3 461, N=2 964)	49.99	51.55	1.56	75.16	76.29	1.13	81.06	81.92	0.86
Traffic offence (N=1 588, N=1 425)	61.90	60.63	-1.27	84.95	84.64	-0.31	67.11	66.58	-0.53
Drug offence (N=1 093, N=1 107)	55.72	56.37	0.65	80.19	81.76	1.57	86.08	87.49	1.41
Other offence (N=1 551, N=1 377)	39.65	40.81	1.16	64.77	64.32	-0.45	70.84	70.77	-0.07

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

[1] The population size presented for each value is for the 1-year follow-up period. The population size drops with 751 observations (from 10 454 to 9 703) during the five-year follow-up period due to either decease, emigration, or deportation.

Appendix D. Regression Table. Doses of incarceration lengths. 1 Year Follow-up

Table D.1. Doses of incarceration lengths and interaction with two-thirds/reform dummy

Linear regression — Number of obs = 10,454; F(13, 10440) = 32.69; Prob > F = 0.0000; R-squared = 0.0309; Root MSE = .48633

reconv_one	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
crime_trend_control	.0000694	.0000967	0.72	0.473	-.0001201	.0002589
crime_trend_controlx2	2.74e-10	1.20e-07	0.00	0.998	-2.34e-07	2.35e-07
dose_response						
5-8	-.0080154	.0182201	-0.44	0.660	-.0437302	.0276994
9-12	-.0606099	.0286701	-2.11	0.035	-.1168089	-.0044109
13-16	-.1396562	.0232243	-6.01	0.000	-.1851803	-.094132
17-20	-.1945761	.0291682	-6.67	0.000	-.2517514	-.1374007
21-24	-.3020446	.0269908	-11.19	0.000	-.3549517	-.2491376
reform_dummy						
Two-thirds/Treatment	-.046251	.0275251	-1.68	0.093	-.1002055	.0077034
dose_response#reform_dummy						
5-8#Two-thirds/Treatment	.0217927	.0261047	0.83	0.404	-.0293774	.0729629
9-12#Two-thirds/Treatment	.0047733	.0419981	0.11	0.910	-.0775511	.0870976
13-16#Two-thirds/Treatment	-.015698	.0335493	-0.47	0.640	-.081461	.050065
17-20#Two-thirds/Treatment	-.0320043	.0416751	-0.77	0.443	-.1136955	.0496868
21-24#Two-thirds/Treatment	.0205861	.0409462	0.50	0.615	-.0596763	.1008486
_cons	.4671991	.0203271	22.98	0.000	.427354	.5070442

Table D.2. Predictive margins

Predictive margins — Number of obs = 10,454
Model VCE : Robust

Expression : Linear prediction, predict()

	Margin	Delta-method Std. Err.	t	P>|t|	[95% Conf.	Interval]
dose_response#reform_dummy						
3-4#Half-time/Control	.4915981	.0172011	28.58	0.000	.4578807	.5253155
3-4#Two-thirds/Treatment	.445347	.0180916	24.62	0.000	.4098841	.48081
5-8#Half-time/Control	.4835827	.0124452	38.86	0.000	.4591878	.5079775
5-8#Two-thirds/Treatment	.4591244	.0132111	34.75	0.000	.4332281	.4850207
9-12#Half-time/Control	.4309882	.0254473	16.94	0.000	.3811065	.4808698
9-12#Two-thirds/Treatment	.3895104	.0276309	14.10	0.000	.3353485	.4436722
13-16#Half-time/Control	.3519419	.0189968	18.53	0.000	.3147045	.3891793
13-16#Two-thirds/Treatment	.2899928	.0202672	14.31	0.000	.2502653	.3297204
17-20#Half-time/Control	.297022	.0259524	11.44	0.000	.2461504	.3478936
17-20#Two-thirds/Treatment	.2187666	.0266817	8.20	0.000	.1664654	.2710679
21-24#Half-time/Control	.1895534	.0235245	8.06	0.000	.1434408	.235666
21-24#Two-thirds/Treatment	.1638885	.0278756	5.88	0.000	.109247	.2185301

Appendix D. Regression Table. Doses of incarceration lengths. 3 Year Follow-up

Table D.3. Doses of incarceration lengths and interaction with two-thirds/reform dummy

```
Linear regression                               Number of obs     =     10,076
                                                F(13, 10062)      =      10.86
                                                Prob > F          =     0.0000
                                                R-squared         =     0.0151
                                                Root MSE          =     .46804

------------------------------------------------------------------------------
                             |               Robust
                reconv_three |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-----------------------------+------------------------------------------------
          crime_trend_control |  -.0000383   .0000932    -0.41   0.681    -.000221    .0001443
        crime_trend_controlx2 |   1.02e-07   1.16e-07     0.88   0.380    -1.26e-07   3.30e-07
                              |
                dose_response |
                        5-8   |   .0088205   .0171799     0.51   0.608    -.0248556   .0424965
                       9-12   |   .0116754    .027069     0.43   0.666    -.0413853   .0647361
                      13-16   |  -.0671457   .0231953    -2.89   0.004    -.1126131  -.0216783
                      17-20   |   -.075762   .0307874    -2.46   0.014    -.1361115  -.0154126
                      21-24   |  -.1780558   .0330715    -5.38   0.000    -.2428826   -.113229
                              |
              reform_dummy    |
      Two-thirds/Treatment    |  -.0222314   .0263794    -0.84   0.399    -.0739403   .0294776
                              |
 dose_response#reform_dummy   |
  5-8#Two-thirds/Treatment    |   .0177413   .0246619     0.72   0.472     -.030601   .0660837
 9-12#Two-thirds/Treatment    |  -.0288433   .0404003    -0.71   0.475     -.108036   .0503494
13-16#Two-thirds/Treatment    |  -.0114786   .0340199    -0.34   0.736    -.0781645   .0552073
17-20#Two-thirds/Treatment    |  -.0963907   .0457871    -2.11   0.035    -.1861425  -.0066388
21-24#Two-thirds/Treatment    |  -.0262014   .0506252    -0.52   0.605    -.1254369   .0730341
                              |
                        _cons |   .6912118   .0192079    35.99   0.000     .6535604   .7288632
------------------------------------------------------------------------------
```

Table D.4. Predictive margins

```
Predictive margins                              Number of obs     =     10,076
Model VCE    : Robust

Expression   : Linear prediction, predict()

------------------------------------------------------------------------------
                             |            Delta-method
                             |     Margin   Std. Err.      t    P>|t|     [95% Conf. Interval]
-----------------------------+------------------------------------------------
 dose_response#reform_dummy  |
     3-4#Half-time/Control   |   .6948397   .0164004    42.37   0.000     .6626917    .7269878
  3-4#Two-thirds/Treatment   |   .6726084   .0172897    38.90   0.000     .6387172    .7064995
     5-8#Half-time/Control   |   .7036602   .0117695    59.79   0.000     .6805896    .7267309
  5-8#Two-thirds/Treatment   |   .6991702   .0125089    55.89   0.000     .6746502    .7236901
    9-12#Half-time/Control   |   .7065151    .024058    29.37   0.000     .6593567    .7536735
 9-12#Two-thirds/Treatment   |   .6554404   .0270931    24.19   0.000     .6023325    .7085483
   13-16#Half-time/Control   |    .627694   .0195471    32.11   0.000     .5893778    .6660103
13-16#Two-thirds/Treatment   |   .5939841   .0216664    27.42   0.000     .5515137    .6364545
   17-20#Half-time/Control   |   .6190777   .0281527    21.99   0.000     .5638928    .6742626
17-20#Two-thirds/Treatment   |   .5004557   .0315202    15.88   0.000     .4386699    .5622414
   21-24#Half-time/Control   |   .5167839    .030587    16.90   0.000     .4568273    .5767405
21-24#Two-thirds/Treatment   |   .4683511    .036202    12.94   0.000      .397388    .5393143
------------------------------------------------------------------------------
```

Appendix D. Regression Table. Doses of incarceration lengths. 5 Year Follow-up

Table D.5. Doses of incarceration lengths and interaction with two-thirds/reform dummy

Linear regression — Number of obs = 9,703; F(13, 9689) = 7.19; Prob > F = 0.0000; R-squared = 0.0108; Root MSE = .44044

reconv_five	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
crime_trend_control	-.0000226	.0000883	-0.26	0.798	-.0001957	.0001505
crime_trend_controlx2	1.24e-07	1.11e-07	1.12	0.264	-9.35e-08	3.41e-07
dose_response						
5-8	.0092347	.0164417	0.56	0.574	-.0229945	.0414638
9-12	.0291462	.0253805	1.15	0.251	-.020605	.0788973
13-16	-.045497	.0224027	-2.03	0.042	-.0894109	-.001583
17-20	-.0616274	.0297857	-2.07	0.039	-.1200137	-.0032411
21-24	-.1312232	.0326765	-4.02	0.000	-.195276	-.0671704
reform_dummy						
Two-thirds/Treatment	-.0336727	.0251961	-1.34	0.181	-.0830624	.015717
dose_response#reform_dummy						
5-8#Two-thirds/Treatment	.0223949	.0235942	0.95	0.343	-.0238546	.0686445
9-12#Two-thirds/Treatment	-.0436078	.0384962	-1.13	0.257	-.1190683	.0318527
13-16#Two-thirds/Treatment	-.0055832	.0327693	-0.17	0.865	-.0698179	.0586514
17-20#Two-thirds/Treatment	-.0670475	.0446889	-1.50	0.134	-.154647	.0205519
21-24#Two-thirds/Treatment	-.0187636	.050195	-0.37	0.709	-.1171563	.0796291
_cons	.7439301	.0183329	40.58	0.000	.7079938	.7798664

Table D.6. Predictive margins

Predictive margins — Number of obs = 9,703
Model VCE : Robust

Expression : Linear prediction, predict()

	Margin	Delta-method Std. Err.	t	P>|t|	[95% Conf.	Interval]
dose_response#reform_dummy						
3-4#Half-time/Control	.7567297	.0156594	48.32	0.000	.7260341	.7874254
3-4#Two-thirds/Treatment	.7230571	.016572	43.63	0.000	.6905725	.7555416
5-8#Half-time/Control	.7659644	.0111235	68.86	0.000	.7441599	.7877689
5-8#Two-thirds/Treatment	.7546866	.0118359	63.76	0.000	.7314858	.7778875
9-12#Half-time/Control	.7858759	.0224798	34.96	0.000	.7418109	.8299409
9-12#Two-thirds/Treatment	.7085955	.0261357	27.11	0.000	.6573641	.7598269
13-16#Half-time/Control	.7112328	.0189202	37.59	0.000	.6741451	.7483204
13-16#Two-thirds/Treatment	.6719769	.0207699	32.35	0.000	.6312634	.7126903
17-20#Half-time/Control	.6951023	.0273173	25.45	0.000	.6415547	.74865
17-20#Two-thirds/Treatment	.5943821	.0310986	19.11	0.000	.5334224	.6553418
21-24#Half-time/Control	.6255065	.0303894	20.58	0.000	.5659369	.6850761
21-24#Two-thirds/Treatment	.5730703	.0362441	15.81	0.000	.5020243	.6441162

Appendix E. Regression Table. Age groups. 1 Year Follow-up

Table E.1. Age groups and interaction with two-thirds/reform dummy

Linear regression — Number of obs = 10,454; F(11, 10442) = 15.29; Prob > F = 0.0000; R-squared = 0.0121; Root MSE = .49099

reconv_one	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
crime_trend_control	.0000781	.0000977	0.80	0.424	-.0001134	.0002697
crime_trend_controlx2	-1.30e-08	1.21e-07	-0.11	0.914	-2.50e-07	2.24e-07
age_groups						
18-20	.0457134	.0297415	1.54	0.124	-.0125856	.1040124
21-24	-.0005835	.0204022	-0.03	0.977	-.0405757	.0394088
25-29	.0148786	.0176713	0.84	0.400	-.0197605	.0495177
>50	-.2120664	.0230379	-9.21	0.000	-.257225	-.1669079
reform_dummy						
Two-thirds/Treatment	-.0235695	.0213269	-1.11	0.269	-.0653743	.0182353
age_groups#reform_dummy						
18-20#Two-thirds/Treatment	-.0307279	.0422308	-0.73	0.467	-.1135084	.0520526
21-24#Two-thirds/Treatment	-.0298853	.0294288	-1.02	0.310	-.0875714	.0278007
25-29#Two-thirds/Treatment	-.0178805	.0254605	-0.70	0.483	-.0677878	.0320269
>50#Two-thirds/Treatment	.0158537	.0338838	0.47	0.640	-.050565	.0822724
_cons	.4200649	.0160108	26.24	0.000	.3886806	.4514493

Table E.2. Predictive margins

Predictive margins — Number of obs = 10,454
Model VCE : Robust

Expression : Linear prediction, predict()

	Margin	Delta-method Std. Err.	t	P>|t|	[95% Conf.	Interval]
age_groups#reform_dummy						
18-20#Half-time/Control	.49103	.0294275	16.69	0.000	.4333464	.5487136
18-20#Two-thirds/Treatment	.4367327	.0296786	14.72	0.000	.3785569	.4949084
21-24#Half-time/Control	.4447332	.019968	22.27	0.000	.405592	.4838743
21-24#Two-thirds/Treatment	.3912784	.0207379	18.87	0.000	.3506281	.4319286
25-29#Half-time/Control	.4601952	.0171406	26.85	0.000	.4265963	.4937942
25-29#Two-thirds/Treatment	.4187453	.0176565	23.72	0.000	.3841353	.4533554
30-49#Half-time/Control	.4453166	.011936	37.31	0.000	.4219198	.4687135
30-49#Two-thirds/Treatment	.4217472	.0131908	31.97	0.000	.3958906	.4476037
>50#Half-time/Control	.2332502	.0226843	10.28	0.000	.1887846	.2777158
>50#Two-thirds/Treatment	.2255344	.0243402	9.27	0.000	.177823	.2732459

Appendix E. Regression Table. Age groups. 3 Year Follow-up

Table E.3. Age groups and interaction with two-thirds/reform dummy

Linear regression — Number of obs = 10,076; F(11, 10064) = 21.92; Prob > F = 0.0000; R-squared = 0.0253; Root MSE = .46556

reconv_three	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
crime_trend_control	-.0000209	.0000927	-0.23	0.822	-.0002026	.0001608
crime_trend_controlx2	7.55e-08	1.16e-07	0.65	0.516	-1.52e-07	3.03e-07
age_groups						
18-20	.0692403	.0262233	2.64	0.008	.0178374	.1206432
21-24	.0252194	.0190264	1.33	0.185	-.0120762	.062515
25-29	-.0106718	.0169446	-0.63	0.529	-.0438867	.022543
>50	-.2966251	.0269538	-11.00	0.000	-.3494599	-.2437903
reform_dummy						
Two-thirds/Treatment	-.0086367	.020466	-0.42	0.673	-.0487541	.0314807
age_groups#reform_dummy						
18-20#Two-thirds/Treatment	-.0549119	.0384498	-1.43	0.153	-.1302812	.0204574
21-24#Two-thirds/Treatment	-.0684835	.0281882	-2.43	0.015	-.1237379	-.013229
25-29#Two-thirds/Treatment	.0014811	.0243416	0.06	0.951	-.0462334	.0491955
>50#Two-thirds/Treatment	.021947	.0392567	0.56	0.576	-.0550039	.098898
_cons	.6862107	.0151078	45.42	0.000	.6565963	.7158251

Table E.4. Predictive margins

Predictive margins — Number of obs = 10,076
Model VCE : Robust

Expression : Linear prediction, predict()

	Margin	Delta-method Std. Err.	t	P>|t|	[95% Conf.	Interval]
age_groups#reform_dummy						
18-20#Half-time/Control	.7607371	.0258176	29.47	0.000	.7101294	.8113448
18-20#Two-thirds/Treatment	.6971885	.0277683	25.11	0.000	.6427571	.7516199
21-24#Half-time/Control	.7167162	.0185467	38.64	0.000	.6803609	.7530715
21-24#Two-thirds/Treatment	.6395961	.0204028	31.35	0.000	.5996024	.6795897
25-29#Half-time/Control	.680825	.0164312	41.43	0.000	.6486165	.7130334
25-29#Two-thirds/Treatment	.6736694	.0168653	39.94	0.000	.64061	.7067288
30-49#Half-time/Control	.6914968	.0114661	60.31	0.000	.669021	.7139726
30-49#Two-thirds/Treatment	.6828601	.0126077	54.16	0.000	.6581465	.7075738
>50#Half-time/Control	.3948717	.0265941	14.85	0.000	.342742	.4470015
>50#Two-thirds/Treatment	.4081821	.0281749	14.49	0.000	.3529536	.4634105

Appendix E. Regression Table. Age groups. 5 Year Follow-up

Table E.5. Age groups and interaction with two-thirds/reform dummy

Linear regression — Number of obs = 9,703; F(11, 9691) = 23.30; Prob > F = 0.0000; R-squared = 0.0316; Root MSE = .43574

reconv_five	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
crime_trend_control	-2.56e-06	.0000876	-0.03	0.977	-.0001743	.0001692
crime_trend_controlx2	9.10e-08	1.10e-07	0.83	0.409	-1.25e-07	3.07e-07
age_groups						
18-20	.0833435	.023489	3.55	0.000	.0373001	.129387
21-24	.027948	.0179558	1.56	0.120	-.007249	.063145
25-29	.0061975	.0159888	0.39	0.698	-.0251439	.0375388
>50	-.3048578	.0278436	-10.95	0.000	-.3594371	-.2502785
reform_dummy						
Two-thirds/Treatment	-.0169385	.0194007	-0.87	0.383	-.0549678	.0210909
age_groups#reform_dummy						
18-20#Two-thirds/Treatment	-.0432107	.0346635	-1.25	0.213	-.1111585	.024737
21-24#Two-thirds/Treatment	-.0428139	.026495	-1.62	0.106	-.0947496	.0091217
25-29#Two-thirds/Treatment	-.0162236	.0230936	-0.70	0.482	-.0614919	.0290447
>50#Two-thirds/Treatment	.0192006	.0402609	0.48	0.633	-.0597191	.0981204
_cons	.7426169	.0143954	51.59	0.000	.7143989	.7708349

Table E.6. Predictive margins

Predictive margins — Number of obs = 9,703
Model VCE : Robust

Expression : Linear prediction, predict()

	Margin	Delta-method Std. Err.	t	P>|t|	[95% Conf.	Interval]
age_groups#reform_dummy						
18-20#Half-time/Control	.8403002	.0230826	36.40	0.000	.7950535	.8855469
18-20#Two-thirds/Treatment	.780151	.0251278	31.05	0.000	.7308952	.8294068
21-24#Half-time/Control	.7849047	.0174814	44.90	0.000	.7506374	.8191719
21-24#Two-thirds/Treatment	.7251523	.0191107	37.94	0.000	.6876913	.7626132
25-29#Half-time/Control	.7631542	.0154101	49.52	0.000	.7329471	.7933612
25-29#Two-thirds/Treatment	.7299921	.016062	45.45	0.000	.6985073	.7614769
30-49#Half-time/Control	.7569567	.0108745	69.61	0.000	.7356404	.7782729
30-49#Two-thirds/Treatment	.7400182	.0119918	61.71	0.000	.7165118	.7635247
>50#Half-time/Control	.4520988	.0274915	16.45	0.000	.3982097	.505988
>50#Two-thirds/Treatment	.454361	.028761	15.80	0.000	.3979835	.5107385

Appendix F. Regression Table. Immigrant Background. 1 Year Follow-up

Table F.1. Immigrant Background and interaction with two-thirds/reform dummy

Linear regression — Number of obs = 10,392; F(9, 10382) = 22.36; Prob > F = 0.0000; R-squared = 0.0150; Root MSE = .48989

reconv_one	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
crime_trend_control	.0000598	.0000979	0.61	0.542	-.0001322	.0002517
crime_trend_controlx2	1.42e-08	1.21e-07	0.12	0.907	-2.23e-07	2.51e-07
etno4kat						
Region 2	-.0027813	.0305818	-0.09	0.928	-.0627276	.057165
Region 3	-.0761887	.0182933	-4.16	0.000	-.1120472	-.0403303
Region 4	-.186811	.0219588	-8.51	0.000	-.2298546	-.1437675
reform_dummy						
Two-thirds/Treatment	-.0350338	.0202517	-1.73	0.084	-.074731	.0046634
etno4kat#reform_dummy						
Region 2#Two-thirds/Treatment	-.0253347	.0434935	-0.58	0.560	-.1105904	.0599209
Region 3#Two-thirds/Treatment	.0568866	.0277923	2.05	0.041	.0024083	.111365
Region 4#Two-thirds/Treatment	-.0401351	.030446	-1.32	0.187	-.0998151	.0195449
_cons	.4386234	.01535	28.57	0.000	.4085344	.4687124

Table F.2. Predictive margins

Predictive margins — Number of obs = 10,392
Model VCE : Robust

Expression : Linear prediction, predict()

	Margin	Delta-method Std. Err.	t	P>|t|	[95% Conf.	Interval]
etno4kat#reform_dummy						
Region 1#Half-time/Control	.4619636	.0112331	41.13	0.000	.4399444	.4839827
Region 1#Two-thirds/Treatment	.4269298	.0120934	35.30	0.000	.4032244	.4506351
Region 2#Half-time/Control	.4591823	.0305383	15.04	0.000	.3993214	.5190432
Region 2#Two-thirds/Treatment	.3988138	.0310761	12.83	0.000	.3378986	.459729
Region 3#Half-time/Control	.3857748	.0181298	21.28	0.000	.3502369	.4213128
Region 3#Two-thirds/Treatment	.4076277	.0209952	19.42	0.000	.3664731	.4487822
Region 4#Half-time/Control	.2751525	.0219436	12.54	0.000	.2321389	.3181662
Region 4#Two-thirds/Treatment	.1999837	.0213086	9.39	0.000	.1582147	.2417526

Appendix F. Regression Table. Immigrant Background. 3 Year Follow-up

Table F.3. Immigrant Background and interaction with two-thirds/reform dummy

Linear regression — Number of obs = 10,014; F(9, 10004) = 15.40; Prob > F = 0.0000; R-squared = 0.0150; Root MSE = .46845

reconv_three	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
crime_trend_control	-.0000473	.0000932	-0.51	0.612	-.0002301	.0001354
crime_trend_controlx2	1.10e-07	1.17e-07	0.95	0.344	-1.18e-07	3.39e-07
etno4kat						
Region 2	.0522404	.0274393	1.90	0.057	-.0015462	.106027
Region 3	-.0646367	.0187029	-3.46	0.001	-.1012981	-.0279753
Region 4	-.2145054	.0250347	-8.57	0.000	-.2635785	-.1654324
reform_dummy						
Two-thirds/Treatment	-.0272292	.0194078	-1.40	0.161	-.0652724	.010814
etno4kat#reform_dummy						
Region 2#Two-thirds/Treatment	-.029184	.0400986	-0.73	0.467	-.1077854	.0494174
Region 3#Two-thirds/Treatment	.0452455	.0279298	1.62	0.105	-.0095026	.0999937
Region 4#Two-thirds/Treatment	.0402698	.0357396	1.13	0.260	-.029787	.1103267
_cons	.6978012	.0145253	48.04	0.000	.6693287	.7262736

Table F.4. Predictive margins

Predictive margins — Number of obs = 10,014
Model VCE : Robust

Expression : Linear prediction, predict()

	Margin	Delta-method Std. Err.	t	P>|t|	[95% Conf.	Interval]
etno4kat#reform_dummy						
Region 1#Half-time/Control	.699644	.0106809	65.50	0.000	.6787073	.7205808
Region 1#Two-thirds/Treatment	.6724148	.0116004	57.97	0.000	.6496758	.6951538
Region 2#Half-time/Control	.7518844	.02751	27.33	0.000	.6979592	.8058095
Region 2#Two-thirds/Treatment	.6954712	.0293516	23.69	0.000	.6379362	.7530062
Region 3#Half-time/Control	.6350073	.0186268	34.09	0.000	.598495	.6715196
Region 3#Two-thirds/Treatment	.6530236	.0209198	31.22	0.000	.6120166	.6940307
Region 4#Half-time/Control	.4851386	.0251003	19.33	0.000	.435937	.5343402
Region 4#Two-thirds/Treatment	.4981792	.0257283	19.36	0.000	.4477466	.5486118

Appendix F. Regression Table. Immigrant Background. 5 Year Follow-up

Table F.5. Immigrant Background and interaction with two-thirds/reform dummy. FU 5 year

Linear regression — Number of obs = 9,641; F(9, 9631) = 12.53; Prob > F = 0.0000; R-squared = 0.0136; Root MSE = .44041

reconv_five	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
crime_trend_control	-.0000326	.0000882	-0.37	0.711	-.0002055	.0001402
crime_trend_controlx2	1.33e-07	1.11e-07	1.20	0.230	-8.42e-08	3.50e-07
etno4kat						
Region 2	.0550959	.0255346	2.16	0.031	.0050428	.1051491
Region 3	-.0457497	.0180962	-2.53	0.011	-.081222	-.0102773
Region 4	-.2005803	.0255147	-7.86	0.000	-.2505944	-.1505662
reform_dummy						
Two-thirds/Treatment	-.0374911	.0184693	-2.03	0.042	-.0736947	-.0012874
etno4kat#reform_dummy						
Region 2#Two-thirds/Treatment	-.0139638	.0371385	-0.38	0.707	-.0867631	.0588356
Region 3#Two-thirds/Treatment	.0402867	.0269599	1.49	0.135	-.0125604	.0931338
Region 4#Two-thirds/Treatment	.0578987	.0360776	1.60	0.109	-.0128211	.1286184
_cons	.7543063	.0137023	55.05	0.000	.7274469	.7811657

Table F.6. Predictive margins

Predictive margins — Number of obs = 9,641
Model VCE : Robust

Expression : Linear prediction, predict()

	Margin	Delta-method Std. Err.	t	P>|t|	[95% Conf.	Interval]
etno4kat#reform_dummy						
Region 1#Half-time/Control	.7651284	.010152	75.37	0.000	.7452284	.7850284
Region 1#Two-thirds/Treatment	.7276373	.0110483	65.86	0.000	.7059803	.7492943
Region 2#Half-time/Control	.8202243	.0256537	31.97	0.000	.7699376	.870511
Region 2#Two-thirds/Treatment	.7687695	.0270667	28.40	0.000	.7157131	.8218259
Region 3#Half-time/Control	.7193787	.0179311	40.12	0.000	.68423	.7545274
Region 3#Two-thirds/Treatment	.7221744	.0201314	35.87	0.000	.6827126	.7616362
Region 4#Half-time/Control	.5645481	.0255477	22.10	0.000	.5144692	.6146269
Region 4#Two-thirds/Treatment	.5849557	.0257416	22.72	0.000	.5344967	.6354147

Appendix G. Regression Table. Offence types. 1 Year Follow-up

Table G.1. Offence types and interaction with two-thirds/reform dummy

Linear regression — Number of obs = 10,454; F(13, 10440) = 61.17; Prob > F = 0.0000; R-squared = 0.0522; Root MSE = .48098

reconv_one	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
crime_trend_control	.0000675	.000096	0.70	0.482	-.0001206	.0002556
crime_trend_controlx2	-6.22e-09	1.19e-07	-0.05	0.958	-2.39e-07	2.27e-07
convictions						
Violence	-.1159933	.0207762	-5.58	0.000	-.1567185	-.075268
Sex	-.2939154	.0304609	-9.65	0.000	-.3536246	-.2342063
Property	.0018085	.0176341	0.10	0.918	-.0327577	.0363746
Traffic	.184778	.0204329	9.04	0.000	.1447257	.2248304
Narcotics	.1507496	.0230665	6.54	0.000	.1055349	.1959643
reform_dummy						
Two-thirds/Treatment	-.0109421	.0242462	-0.45	0.652	-.0584692	.036585
convictions#reform_dummy						
Violence#Two-thirds/Treatment	-.0674247	.0291348	-2.31	0.021	-.1245345	-.0103149
Sex#Two-thirds/Treatment	-.0350562	.0416139	-0.84	0.400	-.1166273	.046515
Property#Two-thirds/Treatment	-.0096492	.0261541	-0.37	0.712	-.0609162	.0416178
Traffic#Two-thirds/Treatment	-.0383688	.0299816	-1.28	0.201	-.0971386	.0204009
Narcotics#Two-thirds/Treatment	.0015383	.0325637	0.05	0.962	-.0622929	.0653694
_cons	.3844187	.0180474	21.30	0.000	.3490423	.4197952

Table G.2. Predictive margins

Predictive margins — Number of obs = 10,454; Model VCE : Robust; Expression : Linear prediction, predict()

	Margin	Delta-method Std. Err.	t	P>|t|	[95% Conf.	Interval]
convictions#reform_dummy						
Violence#Half-time/Control	.2910721	.018419	15.80	0.000	.2549674	.3271769
Violence#Two-thirds/Treatment	.2127053	.017926	11.87	0.000	.1775668	.2478438
Sex#Half-time/Control	.11315	.0291201	3.89	0.000	.0560689	.170231
Sex#Two-thirds/Treatment	.0671517	.0262451	2.56	0.011	.0157063	.1185971
Property#Half-time/Control	.4088739	.0147193	27.78	0.000	.3800213	.4377265
Property#Two-thirds/Treatment	.3882826	.0164474	23.61	0.000	.3560425	.4205227
Traffic#Half-time/Control	.5918434	.0180266	32.83	0.000	.5565079	.627179
Traffic#Two-thirds/Treatment	.5425325	.0194522	27.89	0.000	.5044024	.5806625
Narcotics#Half-time/Control	.557815	.0208311	26.78	0.000	.5169821	.5986479
Narcotics#Two-thirds/Treatment	.5484111	.0207666	26.41	0.000	.5077045	.5891177
Other#Half-time/Control	.4070654	.0145764	27.93	0.000	.3784929	.4356379
Other#Two-thirds/Treatment	.3961233	.0157064	25.22	0.000	.3653357	.4269108

Appendix G. Regression Table. Offence types. 3 Year Follow-up

Table G.3. Offence types and interaction with two-thirds/reform dummy

Linear regression — Number of obs = 10,076; F(13, 10062) = 63.23; Prob > F = 0.0000; R-squared = 0.0654; Root MSE = .45593

reconv_three	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
crime_trend_control	-.000043	.0000907	-0.47	0.636	-.0002208	.0001349
crime_trend_controlx2	9.28e-08	1.13e-07	0.82	0.413	-1.29e-07	3.15e-07
convictions						
Violence	-.1247119	.0226573	-5.50	0.000	-.1691247	-.0802991
Sex	-.386695	.0435624	-8.88	0.000	-.4720859	-.3013041
Property	.0075813	.017518	0.43	0.665	-.0267575	.0419201
Traffic	.1556374	.0182266	8.54	0.000	.1199096	.1913652
Narcotics	.157565	.019941	7.90	0.000	.1184766	.1966533
reform_dummy						
Two-thirds/Treatment	-.0132806	.0237032	-0.56	0.575	-.0597437	.0331824
convictions#reform_dummy						
Violence#Two-thirds/Treatment	-.0404745	.0323321	-1.25	0.211	-.103852	.0229029
Sex#Two-thirds/Treatment	-.0770979	.0581299	-1.33	0.185	-.191044	.0368483
Property#Two-thirds/Treatment	.0044833	.025883	0.17	0.862	-.0462526	.0552192
Traffic#Two-thirds/Treatment	.0035904	.0266705	0.13	0.893	-.0486891	.0558699
Narcotics#Two-thirds/Treatment	.0263426	.0279289	0.94	0.346	-.0284035	.0810888
_cons	.6515048	.0177451	36.71	0.000	.6167209	.6862888

Table G.4. Predictive margins

Predictive margins — Number of obs = 10,076
Model VCE : Robust

Expression : Linear prediction, predict()

	Margin	Delta-method Std. Err.	t	P>|t|	[95% Conf.	Interval]
convictions#reform_dummy						
Violence#Half-time/Control	.5272123	.0203517	25.91	0.000	.4873189	.5671058
Violence#Two-thirds/Treatment	.4734572	.0207823	22.78	0.000	.4327197	.5141947
Sex#Half-time/Control	.2652293	.042528	6.24	0.000	.181866	.3485926
Sex#Two-thirds/Treatment	.1748508	.0367918	4.75	0.000	.1027316	.24697
Property#Half-time/Control	.6595056	.0145592	45.30	0.000	.6309666	.6880445
Property#Two-thirds/Treatment	.6507083	.0159695	40.75	0.000	.6194048	.6820117
Traffic#Half-time/Control	.8075616	.0153364	52.66	0.000	.7774992	.8376241
Traffic#Two-thirds/Treatment	.7978714	.0163046	48.94	0.000	.7659111	.8298318
Narcotics#Half-time/Control	.8094892	.0172962	46.80	0.000	.7755852	.8433932
Narcotics#Two-thirds/Treatment	.8225512	.0168399	48.85	0.000	.7895416	.8555609
Other#Half-time/Control	.6519243	.0142046	45.90	0.000	.6240803	.6797682
Other#Two-thirds/Treatment	.6386436	.0155699	41.02	0.000	.6081235	.6691638

Appendix G. Regression Table. Offence types. 5 Year Follow-up

Table G.5. Offence types and interaction with two-thirds/reform dummy

Linear regression — Number of obs = 9,703; F(13, 9689) = 54.81; Prob > F = 0.0000; R-squared = 0.0638; Root MSE = .42848

reconv_five	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
crime_trend_control	-.0000293	.0000858	-0.34	0.732	-.0001975	.0001388
crime_trend_controlx2	1.16e-07	1.08e-07	1.08	0.281	-9.49e-08	3.27e-07
convictions						
Violence	-.1041475	.0224417	-4.64	0.000	-.148138	-.0601571
Sex	-.3749793	.0469789	-7.98	0.000	-.4670677	-.2828908
Property	.0153131	.016917	0.91	0.365	-.0178478	.048474
Traffic	.1496343	.0169737	8.82	0.000	.1163624	.1829062
Narcotics	.1564316	.0183143	8.54	0.000	.1205317	.1923315
reform_dummy						
Two-thirds/Treatment	-.0203857	.0227407	-0.90	0.370	-.0649622	.0241908
convictions#reform_dummy						
Violence#Two-thirds/Treatment	-.0316888	.0320925	-0.99	0.323	-.0945969	.0312192
Sex#Two-thirds/Treatment	-.0675891	.0642598	-1.05	0.293	-.1935517	.0583736
Property#Two-thirds/Treatment	.004513	.0248982	0.18	0.856	-.0442926	.0533186
Traffic#Two-thirds/Treatment	-.0024062	.0248567	-0.10	0.923	-.0511305	.0463181
Narcotics#Two-thirds/Treatment	.0151127	.0256211	0.59	0.555	-.0351101	.0653354
_cons	.7086573	.0170406	41.59	0.000	.6752541	.7420604

Table G.6. Predictive margins

Predictive margins — Number of obs = 9,703
Model VCE : Robust

Expression : Linear prediction, predict()

	Margin	Delta-method Std. Err.	t	P>|t|	[95% Conf.	Interval]
convictions#reform_dummy						
Violence#Half-time/Control	.6135966	.0201435	30.46	0.000	.5741112	.653082
Violence#Two-thirds/Treatment	.561522	.0207454	27.07	0.000	.5208568	.6021873
Sex#Half-time/Control	.3427649	.0460409	7.44	0.000	.252515	.4330147
Sex#Two-thirds/Treatment	.2547901	.0424567	6.00	0.000	.1715661	.338014
Property#Half-time/Control	.7330572	.013924	52.65	0.000	.7057632	.7603512
Property#Two-thirds/Treatment	.7171845	.0151836	47.23	0.000	.6874215	.7469475
Traffic#Half-time/Control	.8673784	.0138802	62.49	0.000	.8401703	.8945864
Traffic#Two-thirds/Treatment	.8445865	.0149696	56.42	0.000	.815243	.87393
Narcotics#Half-time/Control	.8741757	.0155307	56.29	0.000	.8437323	.9046192
Narcotics#Two-thirds/Treatment	.8689027	.015008	57.90	0.000	.8394839	.8983215
Other#Half-time/Control	.7177441	.0136965	52.40	0.000	.6908962	.744592
Other#Two-thirds/Treatment	.6973584	.0149697	46.58	0.000	.6680147	.7267021

Appendix H. Sensitivity Analysis

Table H.1. Results from linear probability models on the risk for recidivism after treatment. Treatment group one week from reform change and onward; control group one week prior to reform change.

	Follow-up period: 1 year	**Follow-up period: 3 years**	**Follow-up period: 5 years**
Two-thirds/Treatment	-0.0210	-0.0251	-0.0578
	(0.0685)	(0.0694)	(0.0641)
Constant	0.381***	0.639***	0.756***
	(0.0532)	(0.0530)	(0.0477)
Observations	209	202	198
R^2	0.000	0.001	0.004

Robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table H.2. Results from linear probability models on male's risk for recidivism after treatment. Drug offences excluded

	Follow-up period: 1 year	**Follow-up period: 3 years**	**Follow-up period: 5 years**
Two-thirds/Treatment	-0.0295	-0.0262	-0.0366
	(0.0205)	(0.0202)	(0.0194)
Constant	0.403***	0.653***	0.713***
	(0.0156)	(0.0153)	(0.0146)
Observations	9 099	8 756	8 432
R^2	0.000	0.000	0.000

Robust standard errors in parentheses. All estimates are after controlling for trend in entry crimes.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$