

The Best Ones Come Out First!

Early Release from Prison and Recidivism

A Regression Discontinuity Approach^{*}

Olivier Marie

Department of Economics, Royal Holloway University of London; Centre for Economic Performance (CEP) London School of Economics; and Research Centre for Education and the Labour Market (ROA) Maastricht University

This Version: September 2009

Abstract

There is strong evidence that incarceration has a general deterrent effect on individuals on the margin of crime. The impact of the experience of incarceration on future criminal behaviour, self deterrence, is more controversial. It is becoming a pressing issue in view of the large increases in the prison population of past decades. A main question is if harsher or more lenient sentences are more efficient in reducing future recidivism? The fact that offenders with different criminal profiles are treated differently makes it difficult to answer. The best behaved and least dangerous inmates are, for example, more likely to be selected for an early release programme. These characteristics will also influence their future offending behaviour making it difficult to identify the impact of time spent in prison. In this paper we exploit an administrative rule which makes offenders sentenced to less than three months in prison ineligible for the Home Detention Curfew (HDC) scheme in England and Wales to estimate the impact of early release on recidivism using a regression discontinuity (RD) approach. We have access to detailed data on all prisoners released between 2000 and 2006 and their past and future criminal history. We first obtain estimates controlling and matching on observable characteristics which find that the policy reduced recidivism by about 9 percent. The RD methodology takes into account the potential importance of unobservable characteristics. We find that the policy impacts remain relatively unchanged. However, when taking into account prison establishment unobserved characteristics, our results are weakened but still suggest that early release on electronic monitoring can reduce the likelihood of future arrest by 5 to 7 percent.

^{*} **Preliminary and using confidential data: do not quote without authors' permission!**

1 – Introduction

The prison population of England and Wales has almost doubled in the past twenty-five years, reaching over 80,000 inmates in 2007 (Ministry of Justice 2007). This represents the highest incarceration rate, 150 per 100,000 of population, of any comparable country in Western and Northern Europe (Walmsley 2007)¹. The large increase in the prison population should in theory have two impacts. First it will incapacitate offenders from committing crimes while incarcerated. Second it should have a general deterrent effect for individuals on the margin of crime. This second point is predicted in the Becker (1968) model of criminal behaviour which includes probability and severity of punishment as deciding factors. In a recent survey of the literature Levitt and Miles (2007) report that: “The new empirical evidence generally supports the deterrence model but shows that incapacitation influences crime rates, too”. Most of the studies they review focus on general rather than self deterrence. The latter effect is concerned with the individuals’ change in future criminal behaviour resulting from the experience of incarceration. Understanding the mechanisms of self deterrence is becoming ever more important as the proportion of the population with such an experience grows. There is an existing literature attempting to investigate if tougher or more lenient prison sentences are more or less likely to affect future criminal behaviour. Is it for example more efficient to impose longer or shorter custodial periods and in what conditions? The main problem in answering this question is that individuals who are more likely to re-offend, because of their criminal history and other unobserved characteristics, are usually given harsher sentences. Researchers have only recently begun to take this selection issue into account when investigating the impact of punishment on recidivism. We briefly review below the findings from this burgeoning literature.

¹ The ex-Soviet Republics of Estonia, Latvia, and Lithuania (all with about 300) , which are considered as part of Northern Europe, and the tiny Duchy of Luxembourg (with 167) which is part of Western Europe, are the only countries with higher incarceration rates. Note that the incarceration rate levels in Europe are all dwarfed by the 740 prisoners per 100,000 of population in the US.

On average one half of ex-prisoners in England and Wales are re-arrested within one year of their release (Cuppleditch and Evans 2005). This corresponds to a substantial number of crimes committed and it has recently become a government priority to see this recidivism rate decrease². This is the aim of the Home Detention Curfew (HDC) scheme which was introduced throughout the whole of England and Wales in January 1999. Prisoners sentenced to at least three months and to no more than four years are made eligible for early release on an electronic monitored curfew for up to half of their custodial sentence, provided they pass a risk assessment and are able to give a suitable residential address. Although the scheme has been ongoing for a decade, a recent House of Commons Committee of Public Accounts report concluded that:

“There is insufficient evidence available to determine whether electronic monitoring helps to reduce re-offending or promote rehabilitation. The Home Office should carry out further research to establish the role that electronic monitoring could play in minimising re-offending. It should make the results of the research available to courts and prisons, which make decisions on whether to place offenders on curfews.”³

The main reason for the lack of evidence of the impact of early release and electronic policies on recidivism is due to the problem of selection of prisoners into the program. Individuals with the lowest re-offending risk are the ones likely to be chosen for early release. Consequently it is very difficult to identify a policy impact on recidivism which is not biased by this selection. Not surprisingly simple models which do not consider this issue find that prisoners released on HDC are much less likely to re-offend. Results from models controlling and matching on observable characteristics of prisoners should improve the accuracy of our estimates since the selection into the programme will to a

² One of the Home Office Public Service Agreement targets set by the government in 1999 was “to reduce the rate of re-conviction of all offenders punished by imprisonment....by 5% by 2004...”

³ House of Commons Committee of Public Accounts (2006) “The Electronic Monitoring of Adult Offenders”, Conclusion 6, p.4

large extent depend on these observables. When we do this in what follows, we find that the impact of the policy is halved and that it reduces re-offending by between 9 and 10 percentage points. Still, if individuals released on HDC are selected on characteristics which cannot be observed statistically (e.g. general behaviour), then these models could still yield biased results. As this is very likely here we therefore consider other methodologies to try and solve the problem of selection on unobservables.

In this paper we exploit a rule making offenders receiving sentences inferior to three months ineligible for the program to obtain unbiased estimates using the regression discontinuity (RD) methodology. This technique makes simple assumption that the distribution of sentenced prisoners around this eligibility threshold is relatively random to get at the impact of HDC participation on re-offending rates. We first show how the individuals sentenced to three months \pm four weeks are extremely similar in their observable characteristics (i.e. no discontinuity). Following this we argue that there is also no discontinuity on the prisoner unobservables on either side of the threshold which enables us to identify an unbiased policy effect on recidivism.

At first the RD results appear in line to the ones estimated using OLS and PSM. However we find that adding prison establishment level fixed effects diminishes the impact of HDC which now decrease re-offending by between 5 and 7 percentage points. We therefore can conclude that early release on electronic is an efficient policy to improve future criminal behaviour although we must point out the local nature of these findings and can only speculate on the exact mechanism explaining them.

The remainder of the paper is structured as follows. The next section reviews the existing literature on the self deterrence effect of incarceration. Section 3 describes the HDC scheme and discusses the issues to do with eligibility for selection and the modelling approach we adopt to take into account this selection. Section 4 offers a

description of the data used and presents some descriptive statistics. Section 5 presents the empirical results from our various modelling approaches. The last section concludes

2 – Related Literature on Deterrence

Many studies have attempted to empirically identify the deterrent effect of the severity of punishment on criminal behaviour depicted by Becker (1968). One could in theory simply estimate the general impact of incarceration on crime activity. However the fact that areas with more offending are likely to have more prisoners - *ceteris paribus* - gives rise to the endogeneity problem faced by researchers. In a seminal paper, Levitt (1996) uses the changes in prison population sizes resulting from overcrowding litigations to deal with the endogeneity issue. He finds a very important crime reduction effect of incarceration although his estimates measure both deterrence and incapacitation. In a number of other solo and co-authored works, Levitt has subsequently attempted to better isolate the general deterrent effect of prison on criminal activity. He finds that the move from juvenile to adult criminal justice system (Levitt, 1998) has a substantial deterrence effects on youths experiencing this transition. Kessler and Levitt (1999) estimate that sentence enhancement laws in California were responsible for a 4 percent decrease in crime in the state. Finally Katz, Levitt and Shustorovitch (2003) investigate another aspect of general deterrence exploiting differences of the harshness of prison conditions using differences in inmate death rates across areas of the US. They find that the states with the worst penal establishments experienced larger decreases in crime between 1950 and 1990. Put together, all this evidence suggests that tougher punishment (length or conditions) have a deterrent effect on general criminal activity. However it does not answer the question of the impact of incarceration on future criminal behaviour or self deterrence. This is an important issue considering the

increasing number of released prisoners in society as a consequence of the enormous recent rise in the incarcerated population.

To investigate the self deterrence effect of incarceration it is necessary to have longitudinal micro data following the post-discharge offending activity of ex-prisoners. It is also essential to methodologically account for the difference in sentence and/or treatment received by those individual because of their past criminal profiles since this may explain future criminal activity. Only recently have a number of studies thought to measure the impact of self deterrence with serious consideration for the latter selection problematic. Lee and McCrary (2005) exploit the discontinuity of treatment of prisoners younger and older than 18 years old in the Florida criminal justice system to investigate the issue. They find no important deterrence effect of transition from youth to adult sentences and argue that studies using area level data – such as Levitt (1998) – mostly capture the incapacitation effect of incarceration. Chen and Shapiro (2007) also find a different impact of self rather than general deterrence of prison conditions on criminal behaviour. They exploit a discontinuity of assignment of federal prisoners to security levels on a supervision score each inmate receives and find that time spent in a harsher establishment leads to more post-release crime. In related research, Hjalmarsson (2009) capitalizes on discontinuities in punishment that arise from Washington State's juvenile sentencing guidelines to identify the effect of incarceration on the post-release criminal behaviour of juveniles. Her results show that incarcerated individuals have lower propensities to be reconvicted of a crime. Finally, Kuziemko (2007) exploits policy shocks (the over-crowding crisis of 1981) which resulted in the release of 900 prisoners in a single day, and institutional features (cut-offs in parole board guidelines) of the prison system in the state of Georgia to analyse the effect of time served on recidivism *and* the efficiency of a parole system versus a fixed-sentences regime. She finds that the abolition of the parole system has increased both per-prisoner costs and recidivism, and

that an additional month of time served has a large negative effect on the propensity to re-offend.

This last paper is closely related to the study of the impact of early release on future criminal behaviour we investigate here. The main reason is that it is concerned with optimal design of sentence length policy which could minimise recidivism. Although there are no parole boards as such in the UK, the decision to release a prisoner early on the HDC scheme is a discretionary one taken by a team in the inmate's holding establishment. The treatment received is however different since the individuals are still monitored in the outside world until the end of what would have been their custodial sentences as a result of the electronic monitoring. Consequently we will be testing the efficiency in changing future criminal behaviour of an original discharge package rather than just early release from prison.

The data and methodology we exploit for this research is we believe superior to what has been used in the previous literature. Firstly, as we will describe below, we have access to detailed information on *all* the prisoners released in England and Wales over a substantial seven year period (2000 to 2006). Secondly, our identification strategy relies on a very simple and clear discontinuity of sentencing length threshold, three months, which is perfectly observed and strictly enforced for probability of assignment to the scheme. The large sample size and wealth of information we have available also enables us to carry out essential robustness test of the validity of our results. These arguments highlight the innovative nature of this study in light of the existing literature. We now turn to describing the HDC policy, discussing prisoner selection issues into the scheme, and developing our modelling approach.

3 – The HDC Scheme, Issues of Selection and Modelling Approach

The Home Detention Curfew (HDC) Scheme

The Home Detention Curfew (HDC) scheme applies to prisoners who are serving sentences of between three months and under four years. It allows prisoners to live outside of prison providing they do not breach the rules of their curfew and is designed to help prisoners prepare for life after their release. Prisoners released on HDC have to sign a licence enforcing the times when they have to remain at their home address or hostel (this is normally 7pm – 7am). An electronic tag is fitted to the prisoner and monitoring equipment installed at the address by a private contractor⁴. Table 1 describes the salient features of the HDC policy in terms of sentence length, custodial period and the time spent on the scheme if selected. Typically most prisoners only serve around one half of their allotted sentence in custody. There is also a clear cut-off period for eligibility to HDC at three months which will be crucial for our research approach for estimating the impact of early release on recidivism.

The majority of offenders are, at least in principle, considered for early release. However, there are a number of statutory exclusions – in addition to the sentence time limit - which are that the scheme is i) only available to adults and ii) sex offenders and individuals who breached orders are disqualified⁵. Most other prisoners are theoretically eligible for early release on HDC, but must first be assessed according to the two following essential criteria: a) Pass a risk assessment conducted by the prison where the detainee is held. This takes into account previous offending history and other behavioural attributes which could indicate that the prisoner may be likely to breach trust (e.g. breach of bail conditions). The prison staffs also look at the general behaviour of the offender while incarcerated and participation in offending behaviour programs.

⁴ The scheme is explained in detail in the Home Office ‘Practitioners’ Guide’ by Dodgson et al (2000)

⁵ We do find that sex offenders are never considered but a proportion of individuals who have breached in the past (usually a non custodial sanction such as community orders) are selected for HDC. We expect this to be due to lack of information on past criminal history. Since we have what is believed to be superior therefore can control for past breaches.

All these elements are taken into account to ascertain low risk of re-offending for eligibility for early release on HDC. b) The need to have an appropriate address is required by the National Probation Services which provides a home circumstances report. This ensures that the proposed curfew address is suitable, that the risks of the prisoner to the public and of re-offending are acceptable at this address. This is then passed on to the prison which will make a final decision on HDC eligibility⁶.

If at any stage in this assessment it becomes apparent that the individual is not eligible for HDC, the process is stopped and the prisoner will serve the rest of his/her time in prison. After this selection 37 percent of eligible prisoners have been released early and spent part of their custodial sentence on electronic monitoring curfew since the introduction of the policy or more than 70,000 individuals. This represents a substantial number of ex-inmates going through the HDC scheme but there is no, to our knowledge, evaluation of the impact it has on future criminal behaviour. We believe it to be an important question to investigate as over half of discharged prisoners are arrested for another offence within one year of their release.

Issues of Selection and Modelling Approach

The main reason why the impact of HDC on recidivism has not yet been consistently estimated is mainly because there are important selection issues for participation in the scheme which are likely to bias estimation attempts that do not consider them. The brief description on eligibility above makes it clear that HDC is more likely to be granted to offenders with low risks of re-offending. This will depend for example on the past criminal history of the prisoner or his/her general behaviour while incarcerated. From a modelling perspective, this means that the selection process for the scheme is certainly

⁶ The final decision on early release is often left to a local discretionary authority such as parole boards in the US (Kuziemko, 2007) or the holding penal establishment in the case of HDC in the UK. There could exist important variations across prisons in this decision making process. We therefore believe it crucial to control for this possibility and will construct our modelling strategy accordingly.

going to influence the outcome variable we are interested in, namely the probability of re-offending.

Conceptually an ideal empirical comparison would look at the probability of a prisoner of committing a crime after release of an offender who was selected for early release on HDC to an *identical* prisoner who remained in custody until the end of his/her custodial sentence. However, in a practical sense, even with rich data that could control for a large number of observable characteristics of prisoners, it is very clear in the case of HDC that some of the decisions on eligibility to the scheme are discretionary (e.g. prison staff opinion of prisoner behaviour). We therefore need to carefully consider the best methodology to deal with this ‘selection on unobservables’ in our empirical analysis.

Methodology

The main modelling problem we face in estimating the impact of HDC on recidivism is that (both observable and unobservable) characteristics of offenders which are used to decide eligibility are also likely to influence re-offending. This is especially true in terms of unobservable characteristics during the selection process. With this in mind we consider two methodologies, the first being Ordinary Least Squares (OLS) regression methods coupled with Propensity Score Matching (PSM) where we can consider selection on observables, the second using Regression Discontinuity Design (RDD) which potentially can also deal with the selection on unobservables issue. We consider each of these in turn:

1). Controlling and Matching on Observables: OLS and PSM

For individual i a simple statistical model relating recidivism, our outcome of interest, to HDC, the policy treatment, participation can be written as:

$$REC_i = \alpha + \beta HDC_i + \delta Sent_i + u_i \quad (1.1)$$

where REC measures recidivism⁷, HDC is a dummy variable for program participation and u is an error term. We control for length of sentenced received, $Sent$, since this is a first raw indicator of the crime and offender profile for the criminal justice system at the time of judgment. The model also includes dummies for month and year of release to account for possible changes in the application of the policy or re-offending probabilities over time. If assignment to HDC treatment was random, then β would be an unbiased estimator of the impact of HDC on REC , recidivism.

However, as we have shown it is clear that HDC participation is non-random and so a regression estimate from equation (1.1) will be biased – overestimating the decreases in re-offending due to the policy. One possible means to deal with this is to augment (1.1) by adding observable characteristics of prisoners to amend the equation as:

$$REC_i = \alpha + \beta HDC_i + \delta Sent_i + \gamma X_{ki} + u_i \quad (1.2)$$

Where k individual characteristics are included in the vector of control variables, X . The Ordinary Least Squares (OLS) estimate of β is then the relationship between REC and HDC holding constant the X 's.

One aspect of the policy is that the final decision for HDC discharge is taken by the penal establishment. If certain prisons are more likely to decide in favour of early release than others this could affect our results. Also if there are also differences in prison level characteristics, such as harshness of conditions, then this would bias our estimates. We can address these issues by augmenting equation (1.2) as follows:

$$REC_{ia} = \alpha_a + \beta HDC_{ia} + \delta Sent_{ia} + \gamma X_{kia} + u_{ia} \quad (1.3)$$

⁷ We use two measures of recidivism: arrested within 12 and 24 months of release

The model now includes α_a , a set of prison establishment level fixed effects. Our interpretation of the estimated β should now be free of all penal institution factors which could influence the recidivism rate of ex-prisoners. However, if selection into HDC is dependent on factors not included in X or α_a , unobserved by the econometrician, the selection problems remain. One popular method to attempt improve estimates in program evaluation has been to resort to Propensity Score Matching (PSM). This can allow for selection on observables in X to occur in a more flexible manner than in (1.2). It is important to stress, however, that it cannot deal with selection on unobservables.

The PSM method gives a score of the probability of participation into the program based on a set of observable characteristics to each of the individuals. This is estimated using the following probit equation:

$$\Pr[HDC = 1]_i = \Phi(\alpha + \delta Sent_i + \delta X_{ki}) \quad (1.4)$$

where $\Phi(\cdot)$ is the standard normal cumulative distribution function.

Equation (1.4) is a probit estimation of HDC participation on the characteristics in X . From (1.4) one generates propensity scores for each individual. These scores can be used to match prisoners who have not been released on HDC to others who were selected for the scheme with a similar score or the ‘nearest neighbour’ (i.e. who are similar in terms of the X ’s). Once this is done we can again run a version of equation (1.2) to obtain an estimate of β but this time re-weighting each non-treated individual depending on how similar they are to their treated match depending on their propensity scores. We can expand this by also matching released inmates across penal establishment by including prison level fixed effects in the model. While again this should generate estimates of β more precise than the OLS models, they may still suffer from bias.

The reason is that even after matching on, and controlling for, observable individual characteristics, there remains the problem of program selection on

unobservable characteristics. Formally, we may still have that $E[u|HDC] \neq 0$ or that the unobserved part that remains in the error term u is still correlated with the participation decision HDC . A strong assumption of PSM is that matched individuals have relatively similar unobserved characteristics and thus this problem is addressed. This has recently been shown not to be the case when an important part of the selection process relies on discretionary decisions.⁸ As we have discussed this appears to be the case for HDC participation and although we will estimate the three described models, we do not want to conclude on the strength of a policy impact based on these. We therefore consider another methodology which should better address the discussed selection problem: Regression Discontinuity.

2) Regression Discontinuity Design

Regression Discontinuity (RD) design has had a long history in statistics, but has recently gained prominence among economists for its potential for dealing with the problem of unobservable characteristics alongside its conceptual simplicity⁹. This method can only be applied when there exists a cut-off point of an assignment variable Z above and below which there is a strong difference in treatment probability. As we clearly illustrate below, this is the case for HDC treatment depending on the length of sentence received (Z) due to the 3 months minimum selection rule.

A widely researched and very intuitive example of RDD occurs for the 50 percent cut-off rule for winning or losing an election. The argument is that different units (areas, firms) which have had very close votes around the cut-off are likely to be very similar observed and unobserved characteristics. Still they will have opposite outcomes whether they were above or below the assignment cut-off, making it very

⁸ For a discussion on the strengths and weaknesses of PSM see for example Morgan and Harding (2006)

⁹ For a clear and detailed discussion on the RD methodology, see for example Imbens and Lemieux (2008).

simple to compare the difference in impact of selection or not. In this case, an unbiased treatment effect on outcome, here Rec , with subscripts $+$ and $-$ indicating proximity to either side of the threshold can be written as:

$$\beta = Rec^+ - Rec^-$$

It is extremely simple to estimate β here since being above the cut-off guarantees treatment and we only have to compare the means of the outcome around that point. This is called a *sharp* RDD as the probability of treatment, or inclusion into a program, jumps from 0 to 1 on either side of the cut-off.

In the case of HDC treatment, as in many other programs, the change in the probability of treatment around the assignment variable threshold is not so sharp but does greatly increase. This type of set up is called a *fuzzy* RDD and it is still possible to exploit the discontinuity to identify a treatment effect¹⁰. In this case however the difference in outcomes around the cut-off will be a function of the difference in the jump in the proportion treated around this point. Mathematically and using average recidivism, Rec , the mean proportion released on electronic monitoring, HDC , and the subscript $+$ and $-$ as before, we can write $Rec^+ - Rec^- = \beta(HDC^+ - HDC^-)$. This can be re-written as the RDD estimator:

$$\beta = \frac{Rec^+ - Rec^-}{HDC^+ - HDC^-} \quad (2.1)$$

If it is the case that offenders just below and just above the cut-off do have similar characteristics (observable and unobservable) then the estimator in equation (2.1) can legitimately be used to estimate the causal impact of HDC on recidivism. This is because it simply compares the difference in re-offending rates of individuals which

¹⁰ In our case we are actually facing what has been referred to as a '*partially fuzzy*' by Battistin and Rettore (2008) or a '*simple special case*' by Blundell and Costa Dias (2009) version of the RD methodology. This is because treatment is only available but not mandatory on one side of the threshold. Both of these papers highlight the advantage of this approach relative to the standard *fuzzy* RDD.

have been randomly assigned around an assignment threshold and which should consequently have similar characteristics. Of course since not all prisoners released above the three months cut-off are discharged on HDC, this must be scaled by the difference in the jump in the proportion that are treated around this point.

One important point for the validity of this method is that the discontinuity around the threshold only occurs in the treatment variable. That is we need to show that other variables which could impact on selection, for example past criminal history, do not jump at this point. We must first show, to justify the RD method, that the observable characteristics of offenders above and below the sentence threshold are similar or continuous and we do this graphically. We describe below the use of matching techniques in an innovative way to choose the optimal sample size for our estimation. This is important here since it is likely that sentences given will be concentrated into certain fixed numbers of days, weeks or months. We find that this is the case and look at individuals sentenced to + and – 4 weeks around the three months threshold as a result.

We should not have selection on observables and unobservables with the RD methodology. Still to be cautious we will also estimate the necessary differences in mean outcome and mean treatment around the cut-off controlling for individual observable characteristics¹¹. Theoretically this should not change the impact of HDC on recidivism and this is what we find. However when we control for prison level fixed effects we see significant drops in the impact of early release on recidivism suggesting the importance of controlling for establishment level factors.

4 – Data and Descriptive Statistics

¹¹ See Appendix for details on how the RDD equation is adapted to control for observables.

In this Section we discuss the data we use and show some descriptive characteristics for the individuals in our sample. We also discuss the relevance of using Regression Discontinuity in view of the data.

Data

We have drawn on data from the Local Inmate Database System (LIDS) which contains detailed information on the sentence of every prisoner released offender in England and Wales between January 2000 and March 2007. This data contains more than 570,000 discharges for some 324,000 unique individuals due to multiple releases over this period. LIDS contains information on: sentence length; the type of crime offenders were sent to prison for; whether or not he/she was released on HDC, the date discharged, and the date convicted. This data was then matched, using the full name and date of birth of convicts, to the Police National Computer (PNC)¹². The resulting dataset contains information on arrests and convictions histories of all prisoners' pre and post-release in addition to all sentencing details.

We have dropped the subsequent discharges of individuals with multiple releases during this six year period because release on the scheme is only available after a first prison spell¹³. We also dropped all prisoners who were incarcerated for crimes which make them ineligible for HDC such as sexual offences. Under 18s are also dropped because they cannot be released on the scheme. A difficulty arose in matching the crime for which individuals were serving a sentence in the prison data among the multiple crimes recorded for these same individuals in the PNC. Using various dates (charged, sentenced, initial remand) available in this data, and windows of +/- 3 days to

¹² The matching had a 95 percent success rate and the non-matched individuals appeared randomly distributed.

¹³ We show in Table A4.1 of the Appendix the main differences in characteristics of prisoners with single and multiple discharges.

allow for imputing delay or error, and were left with a sample just under 260,000 discharged prisoners.

Eligibility to HDC is restricted to individuals sentenced from 3 months to 4 years. The first part of our analysis will therefore focus on this sample which represents 75 percent of prisoners released or almost 190,000 individuals. For the Regression Discontinuity we look at different samples before and after the three months eligibility limit. In the end we choose individuals who received sentences + and – 4 weeks of the threshold and discuss why below. This represents over 15 percent of all discharges, some 42,214 observations. We are interested in recidivism as our outcome variable of interest. We construct two re-offending dummies to measure one and two year recidivism. They are equal to 1 if the prisoner has a crime recorded in the PNC respectively 12 or 24 months after release and 0 otherwise¹⁴. It is important to note here that we measure re-offending from the day of release from prison or the time of completion of the HDC scheme. We find that only 1.1 percent of prisoners released on the scheme are arrested for a crime before finishing it. The inclusion of these ‘non-compliers’ in the analysis does not change any of our results.

Descriptive Statistics

Figure 1 below shows the distribution of sentences by non-HDC (plain line) and HDC (dotted line) discharge status. The first thing to note is that no prisoner is released early on electronic monitoring if he/she received a sentence less than 88 days (3 months). Second we can see that the majority of sentences given in England and Wales are relatively short, with over half of them shorter than eight months. We also see that there are large peaks in the sentences given at a certain number of days corresponding to

¹⁴ A crime is recorded in the PNC whenever an individual is arrested by the police. Although this does not guarantee a conviction, more than 85 percent of such arrests lead to one.

standard lengths available to judges¹⁵. The vertical lines on the graph show the threshold and the sample we will use for the RD analysis which includes the highest peaks of the distribution of sentences.

Table 2 reports the main descriptive characteristics for prisoners sentenced to between 3 months and 4 years by HDC discharge type. It also reports their recidivism rates and the difference in the characteristics across groups. The characteristics of prisoners selected for HDC make it clear the important selection which takes place when choosing the discharge type. For example there are more women and ethnic minorities released on the scheme. The prisoners selected are on relatively older and most strikingly have committed half as many crimes in the past than those who are not. Early release on HDC means that the offenders spend 14 percent less of their sentence in custody than other prisoners, on average. Looking at re-offending, we find very large differences between the two groups with both one and two years recidivism rates one-third higher for prisoners not released on the scheme. We cannot conclude here that these enormous decreases in recidivism are a result of HDC in view of the important differences in observable characteristics of our two groups of prisoners. The first step of our statistical analysis of the impact of the policy will thus have to focus on controlling for these characteristics. The OLS and PSM methodologies will do this. However they will not solve the selection problem if there exist unobserved differences between HDC and Non-HDC discharged prisoner.

Suitability of RD and the Threshold Sample Size

The essential premise to applying RD is to have a discontinuity in the treatment variable. In our case this is clearly shown in Figure 4.2 which plots the number of days

¹⁵ Judges in England and Wales must follow sentencing guidelines recommended by a panel of experts. These are not compulsory and in any case much looser than the sentencing 'grids' in place in many US states. We return to this issue when discussing the best sample size for our RD analysis below.

of sentenced against the proportion of HDC discharges. None of the prisoners sentenced to less than 88 days are released on the scheme while a quarter of those after the threshold are.

Figure 2 also features vertical lines at the threshold and 28 days before and after the three months HDC eligibility threshold. This is the sample we will use for our analysis. We have selected + and – 4 weeks as our sample based on an innovative technique which compares the best match on observable characteristics of individuals around the discontinuity. The simple method we apply is to create a propensity score of the probability of being sentenced of longer than the threshold for different samples. Carrying out this exercise we find that individuals four weeks around the three month cut-off are the most similarly distributed according to their propensity scores¹⁶. To explain this we first must note that judges are restricted to giving fixed length sentences (e.g. one or two months rather than 82 and 96 days). Still heterogeneity among judges – they can be tougher or more lenient – could mean that offenders with similar observable characteristics end up with very different sentences¹⁷. Consequently the randomness of assignment around the threshold would ‘jump’ from one sentence length to another. This is what we think explains the good match of individuals receiving three months + and – 4 weeks.

Table 3 reports the main characteristics of individuals who receive sentences 28 days above and below the threshold. It also reports their recidivism rates and the difference in the characteristics across groups. We see that the observable characteristics of our control (- 4 weeks) and treated (+ 4 weeks) groups of prisoners are very similar. If some differences are still significant, they are now very small relatively to those in Table 2. For example on average the same proportion of women, 10-11 percent, and

¹⁶ A simple explanation of this methodology can be found in the Appendix along with depiction of the distribution of propensity scores for +/- one week (Figure A1a) and +/- four weeks (Figure A1b)

¹⁷ This difference is well documented in the US. Kling (2006) exploits the difference in how tough judges are to identify the impact of sentence length on future employment.

ethnic minorities, 15 percent, are released on either side of the threshold. Offenders in both groups are the same age in years and have committed on average the same number of offences in the past. We also see that prisoners with post threshold sentences spend slightly less relative proportional time in custody (52 and 50 percent respectively), which could be expected as some of these inmates are released early on HDC. The recidivism rates of both groups are also very similar although somewhat inferior, 1 to 1.5 percentage points, for the + 4 weeks prisoners which is a first indication of an impact of the scheme on re-offending behaviour. Finally, we again see the large difference in HDC treatment of 24 percentage points between the two groups, our discontinuity.

We can also illustrate graphically that, although there exists a large change in the proportion treated around the threshold, there is no such discontinuity in prisoner characteristics. This is crucial to the use of the RD method as it will show that possible selection into HDC is due to sentence length received and not observables. Figure 3 shows the plot of mean number of previous offences by original sentence length. The distribution is U-shaped but clearly demonstrates that there is no discontinuity around the 88 days threshold . All other observed prisoner characteristics display the same continuity graphically around the cut-off sentence length¹⁸. This is to be expected from the mean levels reported in Table 3. What was also of interest is the difference in the outcome, recidivism, we noted between our two groups. Figure 4 plots the proportion of prisoners re-offending within a year of release by length of original sentence. We now see that there are signs of discontinuity around the 3 months threshold. Having that the treatment (HDC) and the outcome (recidivism) are not continuous, but that the observables are continuous, RD should generate a significant impact of the policy. We now turn to our statistical result to measure this impact using different methodologies.

¹⁸ We do not report here the figures of the distribution for all the prisoner observable characteristics considering the length of this paper. The figures are available from the author on request.

4.5 – Empirical Results

In this section we show estimates of the relationship between HDC participation and recidivism using the various methodological approaches we have described above. We first report results from the OLS and PSM models using the whole HDC eligibility sample. We then discuss RDD estimates using the +/- 4 weeks sample around the 88 days threshold. This will enable us to contrast the difference in results obtained considering that the latter methodology should generate more reliable estimates. We then put the estimated impact of the scheme in perspective and briefly discuss them from a policy point of view.

OLS and PSM Results

We start by estimating a simple OLS in the form of equation (1.1). This will measure the raw effect of HDC on recidivism only controlling for length of sentence but not for any other observed characteristics of prisoners selected into the scheme. We then augment the model as in equation (1.2) and control for a number of prisoner characteristics. These controls are: length of sentence in days, gender, age at release, number of previous offences, type of crime incarcerated for (burglary, drug offences, fraud and forgery, robbery, theft and handling, violence against the person, other offences, and offence not recorded), and month and year of discharge dummies. We then add the prison establishment level fixed effects as a final control as in equation (1.3).

The results are given in columns (1), (2), and (3) of Panels A (one year recidivism) and B (two years recidivism) of Table 4. The raw estimates without controls (column (1)) are very large and significant. They show decreases of almost one-third in recidivism rates for prisoners who were discharged on HDC. We know that

this is certainly an over-estimate of the impact of the scheme. This is because it does not take into account that HDC selection depends on being a low re-offending risk prisoner. The covariates we include as controls in column (2) are often used to measure this risk. It is therefore not surprising to find that the estimated impact of the policy on recidivism is almost halved. It remains large and significant with early release on electronic tagging cutting the chances of re-offending by around 12 percentage points. Controlling for prison establishment unobserved characteristics as in column (3) only slightly reduces the estimates.

PSM generates a probability of policy participation on individual characteristics. We do this for HDC selection using a probit $\Pr[HDC = 1]$ on the same controls used for the OLS above. We do this with and without the inclusion of prison establishment fixed effects and the results from the probit regressions are reported in Table A2 of the Appendix. The generated propensity scores assigned to each released prisoner allows us to match the HDC to non-HDC discharges on these scores. We then compare recidivism rates between the two groups and report the result in columns (3) and (4) of Panels A and B of Table 4. We find HDC now appears to reduce recidivism probability by about 9 percent when matching whether we add prison controls or not. Overall, these PSM estimates are relatively consistent with OLS ones although possibly more precise. They are still likely to be biased if the unobserved characteristics these models do not account for play a role in HDC participation. We therefore now turn to the RDD methodology to obtain impact estimates of the policy on recidivism which do not suffer from this bias.

RDD Results

Because of the rule that prisoners sentenced to less than 89 days are not eligible for HDC we can use the RD design to investigate the impact of the policy on recidivism. The main argument for using this method is that since the cut-off is arbitrary, it is very

likely that prisoners are randomly distributed on either side of it. We have shown graphically that there is indeed a strong discontinuity in the proportion of prisoners discharged on HDC around the 89 days threshold but the characteristics of prisoners which could impact on HDC selection and recidivism are continuous at this point. We finally saw that there is however a discontinuity in the outcome variable, re-offending, which is lower for those released 4 weeks after the cut-off compared to those released 4 weeks before.

If everyone was discharged on HDC after 88 days then we would have a Sharp RD and the estimated impact of the policy would simply be the observed lower recidivism rates. However since the increase is only of 24.3 percent we must accordingly scale the measure of the policy impact. The results from this exercise are reported in column (1) of Panels A and B of Table 5 below. For one year recidivism, the estimated effect is as in equation (2.1): the difference in mean outcome, $-.023$, divided by the difference in proportion treated, $.242$, around the threshold. This gives us a significant¹⁹ 9.4 percentage point decrease in re-offending one year after release from HDC participation. The estimated impact of the policy on two year recidivism in Panel B is now 7.9 percentage points but is not statistically significant.

These results confirm our previous findings that the policy does reduce re-offending probability. One important point here is that these estimates are very similar than for those obtained by OLS and PSM. This would suggest that the selection on unobservable prisoner characteristics for the scheme is relatively well captured by controlling for individual observable differences.

We can still see if the average treatment effects measured with RD are affected by controlling for observable characteristics of prisoners discharged around the threshold. Theoretically that should not change our RD estimates. We argue that, even if

¹⁹ As the estimate is similar to a local IV estimate of recidivism on HDC instrumented by being discharged after the cut-off, we are able to obtain standard errors.

they are randomly distributed on either side of the 88 days cut-off, it is still worth maximising the wealth of the data we have and control for prisoner characteristics which may affect HDC treatment and/or recidivism probability. The control variables used are the same as in the OLS models except that now we do not control for length of original sentence because it is collinear with being before or after the threshold. This exercise (column (2)) yields smaller but not statistically different coefficients to the previous estimates. This is a not a surprising result as controlling for observable characteristics should not change our results and therefore reassures us of the validity of the use of the RD methodology.

Another issue of interest we want to explore is the existence of penal establishment level characteristics which could influence our results. This is important since the prison makes the final decision for HDC release and recidivism could vary across establishments because of varying conditions of incarceration. Both these factors could change our estimates if distribution and selection of released prisoners is not similarly distributed around the threshold. To account for this possibility we include penal establishment fixed effects in the RD model. The estimates reported in column (3) show that the policy impacts remain significant but are now somewhat smaller at 6.6 and 5.3 percent for respectively one and two year recidivism. We believe this to be an important finding in view of the extensive use of discontinuity methods to investigate the self deterrence impact of incarceration. It suggests that even if individuals are well distributed around the threshold used for identification, other unobserved establishment or other institution/area level unobserved characteristics may still matter. Still after our model takes this possibility into account, we conclude that early release on electronic monitoring is a successful policy in reducing future criminal activity.

4.6 – Conclusion

The most reliable estimates from our evaluation of the impact of early discharge from prison on Home Detention Curfew, based upon a Regression Discontinuity design, produce evidence that participants were less likely to engage in criminal behaviour after release. These estimates are generally in line with the OLS and PSM results we generated except when we take into account possible prison establishment level unobserved characteristics. Still we obtain statistically significant 5 percentage points reductions in recidivism after release on HDC. In view of the results we argue that this is tentative evidence that this early release programme appears to have succeeded in affecting future criminal behaviour positively.

One should be still be careful not to conclude that extending the scheme to all prisoners discharged will have a similar impact on recidivism. We have estimated an local average treatment effect and HDC may not have the same effect on the behaviour of the 70 to 75 percent of prisoners not released on the scheme. More cautiously we can however say with more certainty that it should reduce changes of re-offending by about the estimated impact for prisoners serving sentences less than three months if HDC became available to them.

Of course, the comparison is only with other offenders and so one needs to be careful to observe that the early release could still have crime increasing consequences relative to keeping offenders in prison²⁰. At the same time it could, by reducing overcrowding, potentially also decrease the re-offending probability of prisoners who are not released on the scheme. This issue of the dynamics of recidivism is a very interesting area of research which has not received the attention it deserves. We therefore view our results as encouraging in the sense that, with a rigorous research approach, we can pin down a significant reduction in the probability of re-offending.

²⁰ Austin (1986) discusses these issues in the US context.

A natural question is the mechanism by which recidivism fell for HDC participants relative to their non-participating peers. One could suggest that the relatively small difference in the time spent in custody as a result of the scheme is crucial in avoiding 'prisonisation' or loss of contact with civil society while incarcerated. Another possibility is that reduced discharge on curfew orders are a form of 'social contract' with the released prisoner with strong positive effect for rehabilitation. Our overall conclusion is that HDC as an early release package – with a monitoring period outside prison - works in reducing re-offending although the exact mechanism to achieve remains uncertain.

References:

- Austin, J. (1986) Using Early Release to Reduce Prison Crowding: A Dilemma in Public Policy, *Crime and Delinquency*, 32, 404-502.
- Battistin E. and E. Rettore (2008) Ineligible and Eligible Non-Participants as a Double Comparison Group in Regression Discontinuity Design, *Journal of Econometrics*, 142, 715-730
- Becker, G. (1968) Crime and Punishment: An Economic Approach, *Journal of Political Economy*, 76, 175-209.
- Blundell, R. and M. Costa Dias (2009) Alternative Approaches to Evaluation in Empirical Microeconomics, *Journal of Human Resources*, forthcoming.
- Chen, M.K. and J.M. Shapiro (2007) Do Harsher Prison Conditions Reduce Recidivism? A Discontinuity-based Approach, *American Law and Economics Review*, 9, 1-30.
- Dodgson, K., E. Mortimer, and D. Sugg (2000) Assessing Prisoners for Home Detention Curfew: A Guide for Practitioners, *RDS Practitioners Guide 1*, Home Office RDS
- Hahn, J., P. Todd, and W Van der Klaauw (2001) Identification and Estimation of Treatment Effects with Regression-Discontinuity Design, *Econometrica*, 69, 201-209.
- Heckman, J., H. Ichimura and P. Todd (1997) Matching as an Econometric Evaluation Estimator, *Review of Economic Studies*, 65, 261-294.
- Hjalmarsson, Randi (2009) "Juvenile Jails: A Path to the Straight and Narrow or Hardened Criminality?" *Journal of Law and Economics*, forthcoming.

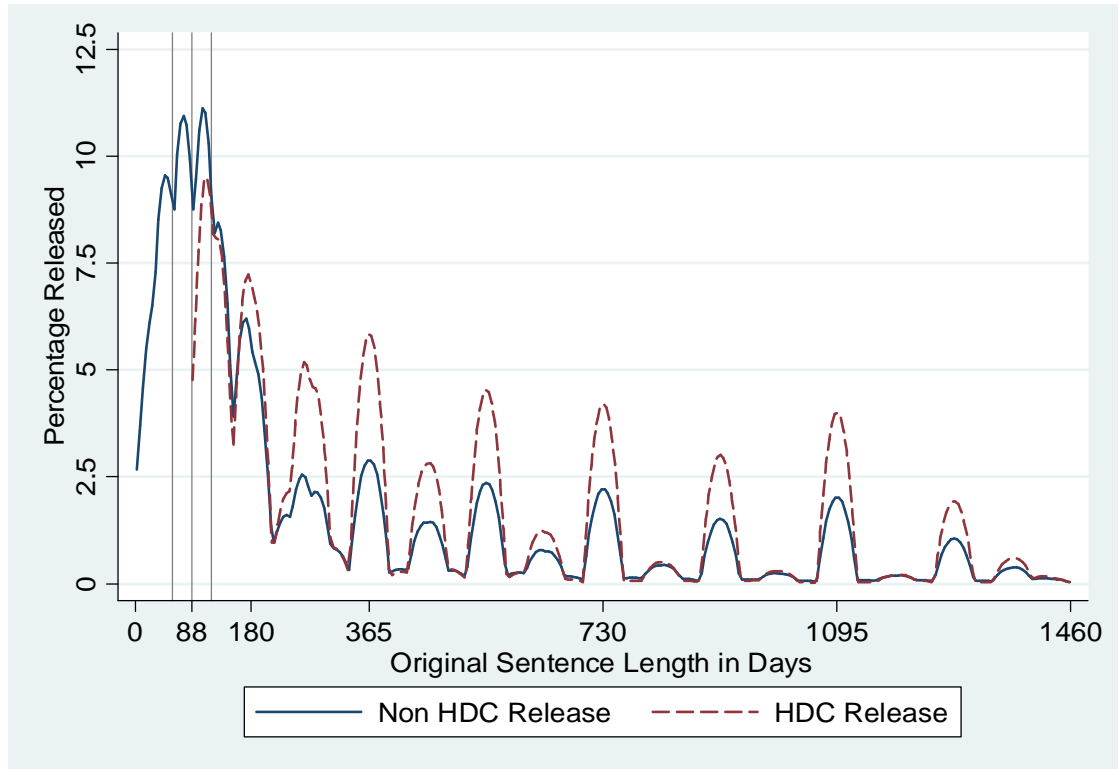
- Imbens, G. W. and T. Lemieux (2008) Regression Discontinuity Design: A Guide to Practice, *Journal of Econometrics*, 142, 615-635.
- Katz, L., Levitt, S.D. and E. Shustorovich (2003) Prison Conditions, Capital Punishment, and Deterrence, *American Law and Economic Review* 52. 318–43.
- Kessler, D.P. and S.D. Levitt (1999) Using Sentence Enhancements to Distinguish between Deterrence and Incapacitation, *Journal of Law and Economics*, 17, 343-363.
- Kling, Jeffrey R. (2006) Incarceration Length, Employment and Earnings, *American Economic Review*, 96:3, 863-876.
- Kuziemko, A. (2007) Going Off Parole: How the Elimination of Discretionary Prison Release Affects the Social Cost of Crime, *NBER Working Paper No. 13380*.
- Lee, D.S. and J. McCrary (2005) Crime, Punishment and Myopia, *NBER Working Paper No. 11491*.
- Levitt, S.D. (1996) The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation, *Quarterly Journal of Economics*, 111, 319-325
- Levitt, S.D. (1998) Juvenile Crime and Punishment, *Journal of Political Economy*, 106, 1156-1185
- Ministry of Justice of England and Wales (2007) Population in Custody (Monthly), Available from <http://www.justice.gov.uk/publications/populationincustody.htm>
- Morgan, S. L. and D.J. Harding (2006) Matching Estimator of Causal Effects: Prospects and Pitfalls in Theory and Practice, *Sociological Methods and Research*, 35, 3-60
- Rosenbaum, P. and D. Rubin (1984) Reducing Bias in Observational Studies Using Subclassification on the Propensity Score, *Journal of the American Statistical Association*, 79, 516-524.

Sentencing Guideline Council of England and Wales (Various Years) *Sentencing Guidelines*, Available from <http://www.sentencing-guidelines.gov.uk/index.html>

Van der Klaauw, Wilbert (2002) Estimating the Effect of Financial Aid Offers on College Enrolment: A Regression Discontinuity Approach, *International Economic Review*, 43, 1249-1287

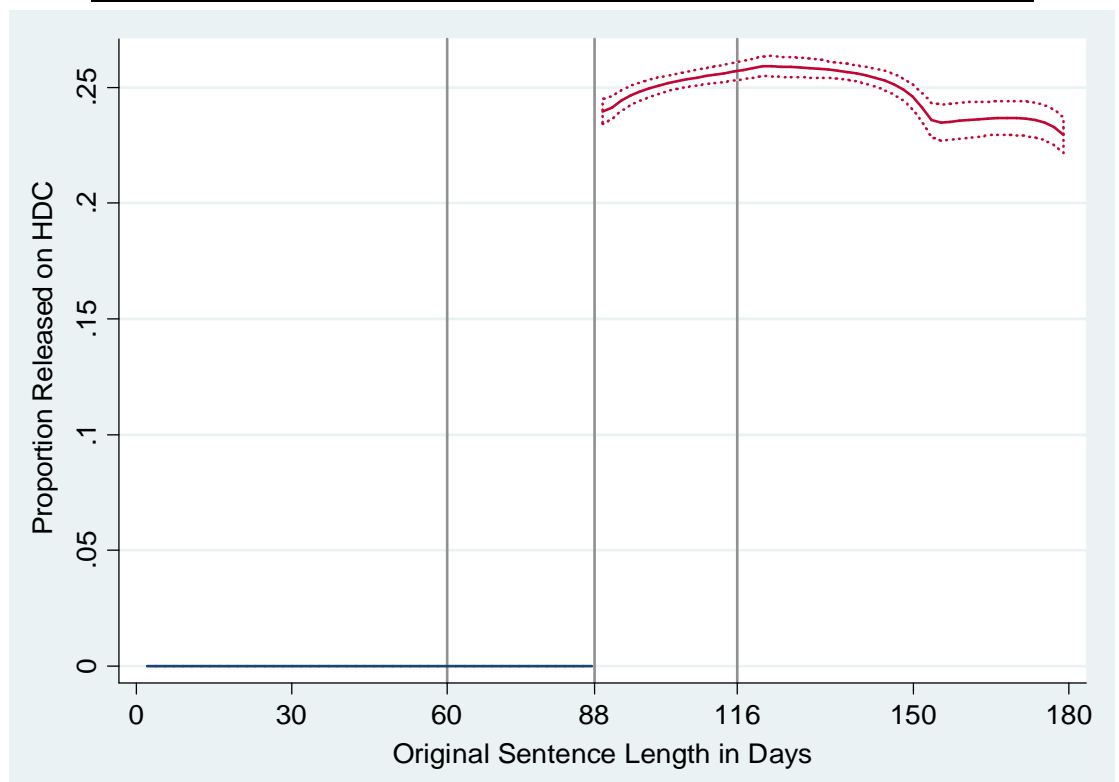
Walmsley, Roy (2007) Wold Prison Population List (Seventh Edition), *International Centre for Prison Studies*, King's College London

Figure 1: Distribution of Original Sentence Lengths in Days



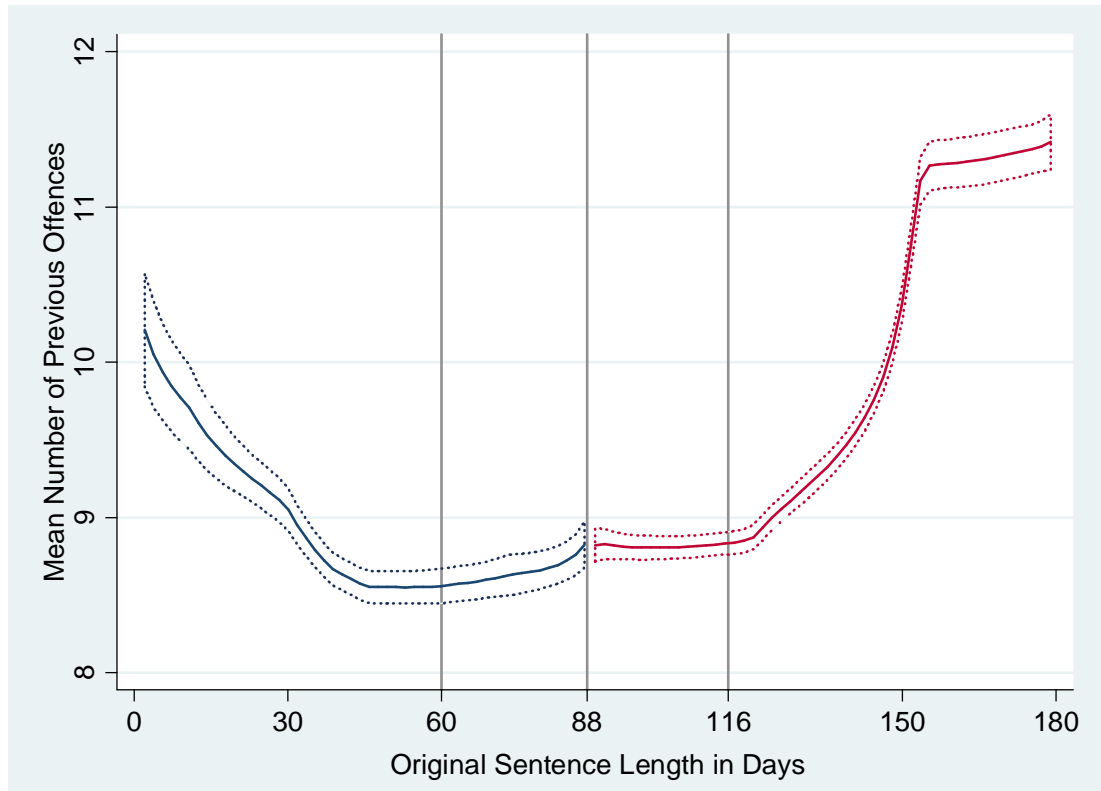
Note: Line smoothed with 14 days local averages.

Figure 2 : Proportion Released on HDC by Original Sentence Length



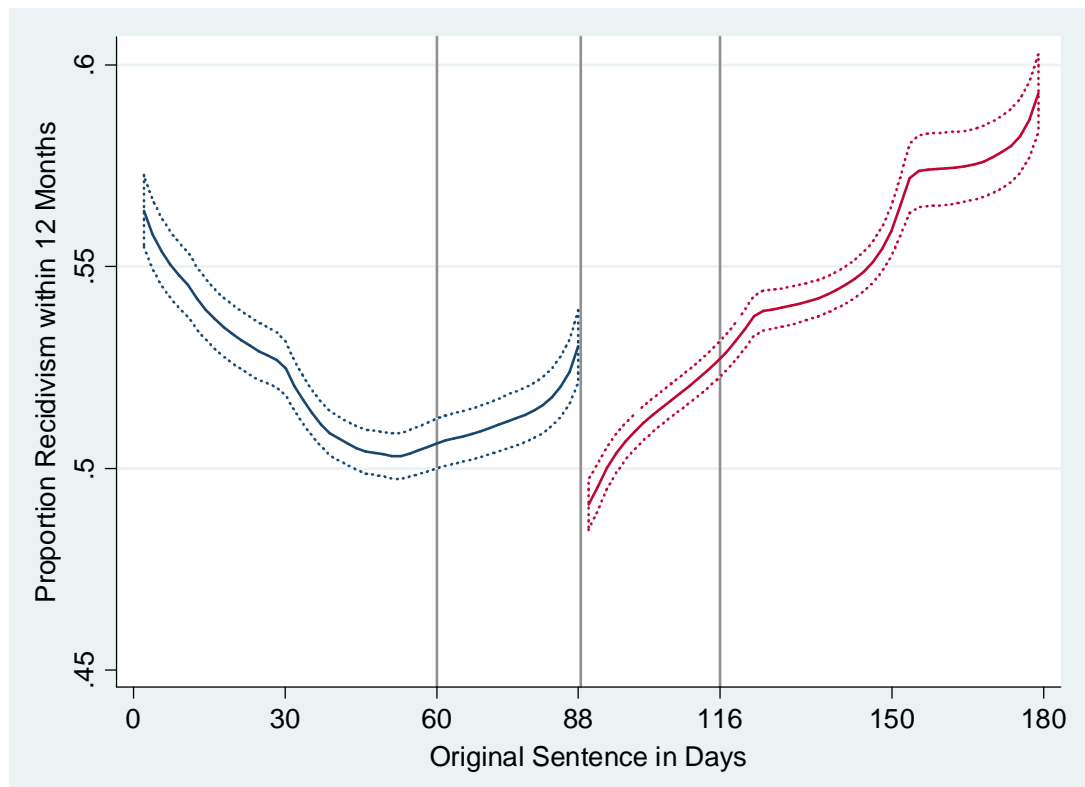
Note: Dotted lines show the confidence intervals. Line smoothed with 14 days local averages.

Figure 3 : Number of Previous Offences by Original Sentence Length



Note: Dotted lines show the confidence intervals. Line smoothed with 14 days local averages.

Figure 4: One Year Recidivism Rate by Original Sentence Length



Note: Dotted lines show the confidence intervals. Line smoothed with 14 days local averages.

**Table 1: Original Sentence Length,
Custodial Sentence Length, and Period on HDC**

Length of Sentence	Custodial Period of Sentence	Custodial Period to be Served if HDC Granted	Period on HDC
< 3 Months	< 6 Weeks	Not eligible	-
3 Months	6 Weeks	4 Weeks	2 Weeks
6 Months	3 Months	6 Weeks	6 Weeks
12 Months	6 Months	3 Months	3 Months
18 Months	9 Months	4.5 Months	4.5 Months
2 Years	1 Year	7.5 Months	4.5 Months
< 4 Years	< 2 Years	1 Year 7.5 Months	4.5 Months
> 4 Years	> 2 Years	Not eligible	-

Table 2: Descriptive Statistics of Prisoners by HDC Status of Release

Descriptive Characteristics of Prisoners Released on HDC or Otherwise who Receive Sentences Between 3 Months and 4 Years (HDC Eligibility Period)			
Discharge Type	Non HDC	HDC	Difference
Percentage Female	.078	.105	.027 (.001)
Percentage Ethnic Minority	.156	.177	.021 (.002)
Mean Age at Release	28.6	30.2	1.62 (.046)
Percentage Incarcerated for Violence	.241	.267	.026 (.002)
Percentage Breached in Past	.272	.133	-.138 (.002)
Mean Number Previous Offences	10.5	5.3	-5.07 (.033)
Proportion of Sentence Custodial	.527	.390	-.137 (.001)
Recidivism within 12 Months	.422	.190	-.231 (.002)
Recidivism within 24 Months	.566	.315	-.251 (.002)
Sample Size	118,494	70,908	-

Note: Robust standard errors in parenthesis.

Table 3: Descriptive Statistics of Prisoners by Original Sentence Length

Descriptive Characteristics of Prisoners Released Who Are Sentenced to 4 Weeks Before or After 3 Months Threshold for HDC Eligibility			
Discharge Type	- 4 Weeks	+ 4 Weeks	Difference
Percentage Female	.113	.102	-.011 (.004)
Percentage Ethnic Minority	.154	.154	-.000 (.004)
Mean Age at Release	29.3	29.5	.221 (.093)
Percentage Incarcerated for Violence	.186	.195	.009 (.004)
Percentage Breached in Past	.271	.269	-.000 (.004)
Mean Number Previous Offences	8.6	8.9	.301 (.087)
Proportion of Sentence Custodial	.521	.498	-.023 (.001)
Recidivism within 12 Months	.469	.446	-.014 (.005)
Recidivism within 24 Months	.596	.576	-.010 (.005)
Proportion Discharged on HDC	0	.242	.242 (.003)
Sample Size	17,706	24,055	-

Note: Robust standard errors in parenthesis.

Table 4: OLS and PSM Estimates of Impact of HDC on Recidivism

Panel A: Recidivism Within 12 Months of Release		Estimation on Individuals Sentenced to Between 3 Months and 4 Years: HDC Eligibility				
		OLS			PSM	
		(1)	(2)	(3)	(4)	(5)
HDC Discharge Dummy		-.218 (.002)	-.121 (.002)	-.114 (.002)	-.092 (.003)	-.088 (.004)
Sentence Length	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes	Yes
Prison Fixed Effects	No	No	Yes	No	Yes	Yes
PSM	No	No	No	Yes	Yes	Yes
Sample Size		189,402	189,402	189,402	189,402	189,402

Panel B: Recidivism Within 24 Months of Release		Estimation on Individuals Sentenced to Between 3 Months and 4 Years: HDC Eligibility				
		OLS			PSM	
		(1)	(2)	(3)	(4)	(5)
HDC Discharge Dummy		-.239 (.002)	-.127 (.002)	-.119 (.003)	-.098 (.004)	-.093 (.004)
Sentence Length	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes	Yes
Prison Fixed Effects	No	No	Yes	No	Yes	Yes
PSM	No	No	No	Yes	Yes	Yes
Sample Size		189,402	189,402	189,402	189,402	189,402

Note: Robust standard errors in parenthesis. The controls included in column (2) are: length of sentence in days, gender, age, number of previous offences, month and year of release dummies, and the type of crime incarcerated for (8 types). The same model with 140 prison establishment fixed effects is reported in column (3). The propensity score matching in columns (4) and (5) is based on the probit regressions reported in Table A2 of the Appendix.

Table 5: RD Estimates of HDC Impact on Recidivism

Panel A: Recidivism Within 12 Months of Release	Estimation on Individuals Sentenced to Between 58 and 118 Days: +/- 4 Weeks		
	(1)	(2)	(3)
Discontinuity of HDC Participation Around Threshold ($HDC^+ - HDC^-$)	.242 (.003)	.243 (.003)	.237 (.003)
Difference in Recidivism Around Threshold ($Rec^+ - Rec^-$)	-.023 (.005)	-.022 (.005)	-.016 (.005)
Estimated Effect of HDC on Recidivism Participation ($Rec^+ - Rec^-$) / ($HDC^+ - HDC^-$)	-.094 (.020)	-.090 (.018)	-.066 (.018)
Controls	No	Yes	Yes
Prison Fixed Effects	No	No	Yes
Sample Size	41,761	41,761	41,761

Panel B: Recidivism Within 24 Months of Release	Estimation on Individuals Sentenced to Between 58 and 118 Days: +/- 4 Weeks		
	(1)	(2)	(3)
Discontinuity of HDC Participation Around Threshold ($HDC^+ - HDC^-$)	.242 (.003)	.243 (.003)	.237 (.003)
Difference in Recidivism Around Threshold ($Rec^+ - Rec^-$)	-.019 (.005)	-.019 (.004)	-.013 (.005)
Estimated Effect of HDC on Recidivism Participation ($Rec^+ - Rec^-$) / ($HDC^+ - HDC^-$)	-.079 (.020)	-.077 (.018)	-.053 (.019)
Controls	No	Yes	Yes
Prison Fixed Effects	No	No	Yes
Sample Size	41,761	41,761	41,761

Note: Robust standard errors in parenthesis. The estimation is based on individuals sentenced to between 59 and 118 days. The controls included in column (2) are: gender, age, ethnic minority, breached in the past number previous offences, month and year of release dummies, and the type of crime incarcerated for (8 types). The same model with 126 prison establishment fixed effects is reported in column (3).

Appendix

RD with Controls

The formulae for estimation of β controlling for observable characteristics with Regression Discontinuity is as follows:

$$\beta = \frac{Rec^+ - Rec^-}{HDC^+ - HDC^-} = \frac{H}{\Gamma} \quad \text{from equations (A1.1) and (A1.2) below} \quad (A1)$$

$$HDC_i = \alpha + HAfter_i + \gamma X_{ki} + u_i \quad (A1.1)$$

$$Rec_i = \alpha + \Gamma After_i + \gamma X_{ki} + u_i \quad (A1.1)$$

The X s are the same k controls as before. It is not possible here to control for length of the original sentence, *Sent*, since it is orthogonal to being after the threshold, *After*.

We can however augment the above model by including prison level fixed effects to control for establishment specific unobservable characteristics.

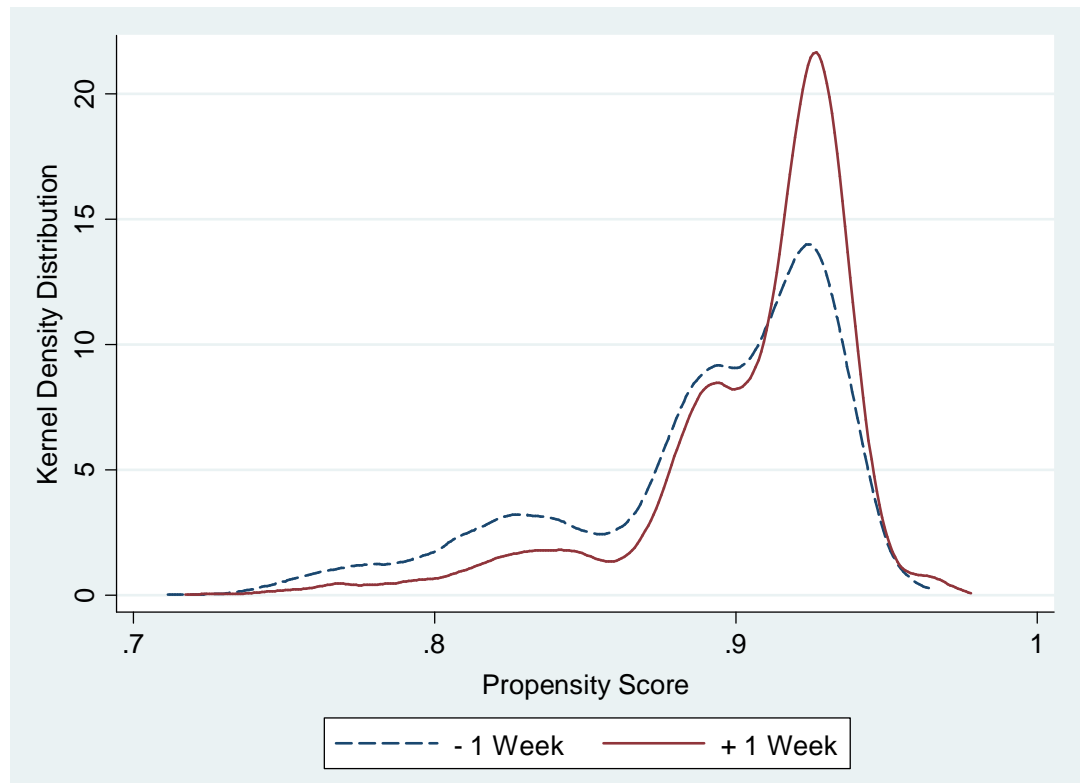
Choice of Sample around RDD Threshold

We run a simple probit model of the chances of a prisoner receiving a sentence above the threshold, *After*, on his observable characteristics as in this equation:

$$\Pr[After = 1]_i = \Phi(\alpha + \delta X_{ki}) \quad (A2)$$

We try this for different sample size and can plot the propensity scores generated to try and chose the best fitting sample. This is for example what we do in Figure A1.1 for + and -one week and Figure A1.2 for + and - four weeks below. We clearly see that the four weeks sample generates a far superior match on observables with the distribution of propensity scores of both groups impressively similar.

**Figure A1a: Propensity Scores for Individuals
Receiving Sentences +/- 1 Week of Three Months HDC Threshold**



**Figure A1b: Propensity Scores for Individuals
Receiving Sentences +/- 4 Weeks of Three Months HDC Threshold**

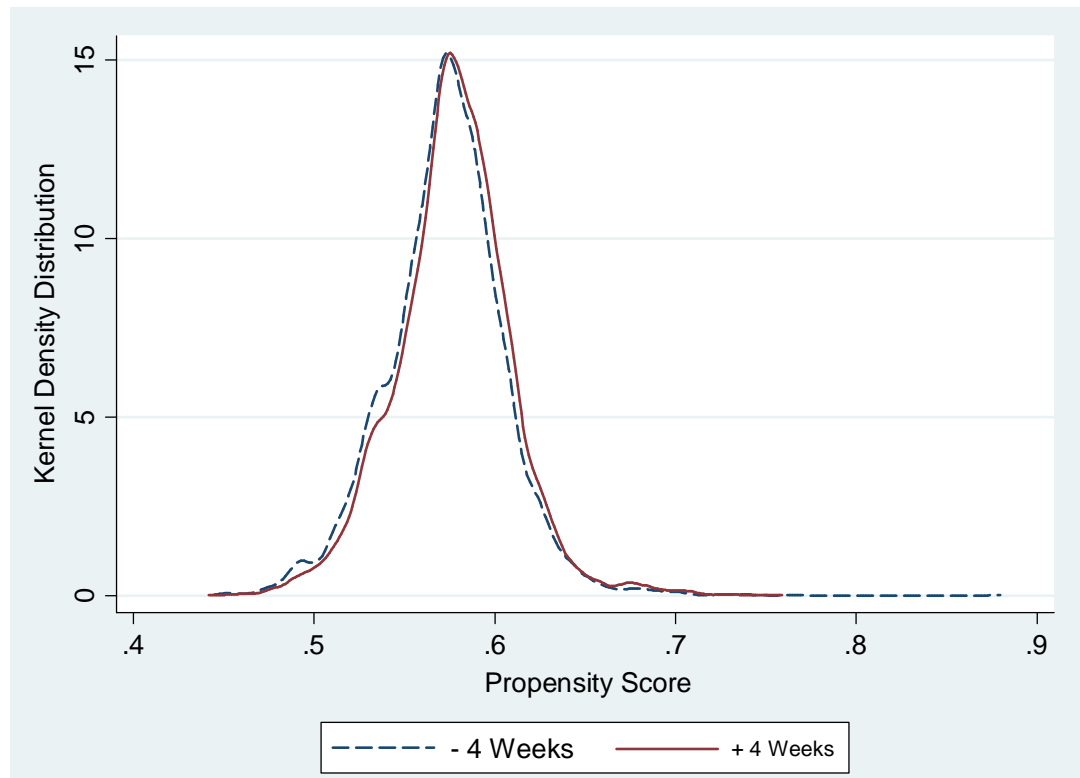


Table A1: Descriptive Statistics of Prisoners by HDC Status of Release

<i>Descriptive Characteristics of First Sentence of Prisoners Released with Single or Multiple Discharges</i>			
Discharge Type	Single Discharge	Multiple Discharges	Difference
Percentage Female	.098	.074	.024 (.001)
Percentage Ethnic Minority	.193	.136	-.058 (.002)
Mean Age at Release	30.8	27.3	3.5 (.037)
Percentage Incarcerated for Violence	.294	.182	-.111 (.002)
Percentage Breached in Past	.186	.298	.111 (.002)
Mean Number Previous Offences	6.9	12.3	5.4 (.039)
Proportion of Original Sentence Custodial	.479	.502	.024 (.001)
Recidivism within 12 Months	.211	.637	.426 (.002)
Recidivism within 24 Months	.337	.789	.452 (.002)
Proportion Discharged on HDC	.339	.126	. (.002)
Sample Size	180,374	76,908	-

Note: Robust standard errors in parenthesis.

**Table A2: Probit Models of HDC Participation as a Function of
Prisoner Observed Characteristics – Sentenced to 3 Month to 4 Years**

	Pr[HDC = 1]	
Prisoner Characteristics	(1)	(2)
Original Sentence Length	.000 (.000)	.000 (.000)
Gender	.015 (.004)	-.099 (.011)
Ethnic Minority	-.073 (.003)	-.057 (.003)
Age at Release	.006 (.000)	.005 (.000)
Breached in the Past	-.044 (.003)	-.051 (.003)
Number of Previous Offences	-.021 (.000)	-.018 (.000)
Offence Sentenced for Dummies	Yes	Yes
Month of Release Dummies	Yes	Yes
Year of Release Dummies	Yes	Yes
Prison Fixed Effects	No	Yes
Sample size	189,402	189,402

Notes: Marginal effects reported. Robust standard errors in parentheses. The model in column (2) includes 140 prison establishment fixed effects.