

A Policy Evaluation of Home Detention Sentencing:  
Evidence from New Zealand

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## Abstract

On 1 October 2007, New Zealand authorised home detention as a stand-alone sentence, permitting a judge to directly sentence offenders to home detention without requiring an initial imprisonment sentence. In this thesis, I estimate the causal effect of New Zealand's home detention sentence on first-time offenders' recidivism rates. Using population-wide linked administrative data from Statistics New Zealand's Integrated Data Infrastructure, I identify the local average treatment effect for first-time offenders sentenced around the home detention policy reform of 1 October 2007. By focusing on offenders sentenced within a narrow time window of the policy implementation, I construct a treatment and comparison group such that differences in recidivism rates can be used to recover the causal effect of home detention. In contrast to a policy evaluation of home detention carried out by the Ministry of Justice, I find no evidence that home detention affects the recidivism rate of first-time offenders, relative to short-term imprisonment, community detention or intensive supervision sentences. This conclusion is robust across a range of fuzzy regression discontinuity design and instrumental variables specifications. Additionally, I analyse the effect of a home detention sentence on offenders' attachment to the labour market. I run a difference-in-differences regression analysis of average monthly employment rates, earnings and benefit receipt, controlling non-parametrically for calendar time trends. The regression results provide no evidence that a home detention sentence increases offenders' labour market attachment relative to offenders at the margin of home detention eligibility. Overall, results in this thesis provide little justification for promoting home detention as a means for reducing crime or improving offenders' short-term or long-term labour market positions.

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## Attestation of Authorship

I hereby declare that this submission is my own work and that, to the best of my knowledge and belief, it contains no material previously published or written by another person (except where explicitly defined in the acknowledgements), nor material which to a substantial extent has been submitted for the award of any other degree or diploma of a university or other institution of higher learning.

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## Disclaimer

The results in this paper are not official statistics. They have been created for research purposes from the Integrated Data Infrastructure (IDI) managed by Statistics NZ.

The opinions, findings, recommendations, and conclusions expressed in this paper are those of the author(s), not Statistics NZ.

Access to the anonymised data used in this study was provided by Statistics NZ in accordance with security and confidentiality provisions of the Statistics Act 1975. Only people authorised by the Statistics Act 1975 are allowed to see data about a particular person, household, business, or organisation. The results in this paper have been confidentialised to protect these groups from identification.

Careful consideration has been given to the privacy, security, and confidentiality issues associated with using administrative and survey data in the IDI. Further detail can be found in the Privacy impact assessment for the Integrated Data Infrastructure available from [www.stats.govt.nz](http://www.stats.govt.nz).

The results are based in part on tax data supplied by Inland Revenue to Statistics NZ under the Tax Administration Act 1994. This tax data must be used only for statistical purposes, and no individual information may be published or disclosed in any other form, or provided to Inland Revenue for administrative or regulatory purposes. Any person who has had access to the unit record data has certified that they have been shown, have read, and have understood section 81 of the Tax Administration Act 1994, which relates to secrecy. Any discussion of data limitations or weaknesses is in the context of using the IDI for statistical purposes, and is not related to the data's ability to support Inland Revenue's core operational requirements.

## Section 1 Introduction

Prison overpopulation is an issue faced by many OECD countries (OECD, 2016). New Zealand's prison population has steadily increased since 1989 (Gluckman, 2018) and experienced a sharp rise between 2002 and 2006 when the incarceration rate jumped from 145 to 181 prisoners per 100,000 individuals (World Prison Brief, n.d.). Over the same period, the OECD average ranged from approximately 125 to 136 individuals per 100,000 (Australian Bureau of Statistics, 2009; OECD, 2006). In November 2006, the Criminal Justice Reform Bill was submitted to the New Zealand Government. The purpose of this Bill was to introduce financially and socially sustainable mechanisms to reduce New Zealand's incarceration rate. Amongst other things, the Bill suggested enacting home detention as a non-custodial alternative to imprisonment. Accordingly, on 1 October 2007, home detention was established as a sentence in its own right.

Home detention is an attractive criminal sanction for at least three reasons. First, home detention naturally eases prison overpopulation as offenders can serve their sentence at home. Second, there are fiscal cost-savings presented by administering home detention over short-term imprisonment. Offenders on home detention are estimated to cost New Zealand taxpayers \$58 per day compared to \$249 per day for imprisoned offenders (Ministry of Justice, 2011). The fiscal gains are further enhanced in New Zealand's jurisdiction because a 12-month home detention sentence is deemed equivalent to a 24-month imprisonment sentence (Sentencing Act 2002, S15B), reducing the number of days for which taxpayers are burdened by offenders. Third, a home detention sentence is thought to provide better outcomes for offenders by enabling them to preserve family and employment relationships (Gibbs & King, 2003). Similarly, offenders should be better prepared for reintegration back into society as they avoid the negative implications of imprisonment, namely: labour market discrimination against ex-inmates, loss of human capital and accumulation of criminal capital (Andersen & Andersen, 2014). The explanatory note of the Criminal Justice Reform Bill 2006 stated that the "advantages of home detention include low rates of reconviction and reimprisonment, high compliance rates, and positive support for offenders' reintegration and rehabilitation" (p. 5).

However, the empirical evidence on the effectiveness of home detention is mixed. For example, analysis of a 2001 Swedish electronic monitoring experiment showed no significant differences in the recidivism rates of offenders in the program compared to imprisoned offenders (Marklund & Holmberg, 2009; Wennerberg, Marklund & Nimeus, 2005). However, Henneguelle, Monnery and Kensey (2016) found that the incremental introduction of home detention in France

significantly reduced the recidivism rate for offenders on home detention relative to imprisonment. Similar contradictions are evident for the effect of home detention on offenders' attachment to the labour market. For example, Denmark's 2006 home detention reform resulted in significantly lower dependency on social welfare for offenders on home detention relative to imprisonment, while the 2008 expansion had no significant effect on the labour market outcomes of those affected by the policy (Andersen & Andersen, 2014).

While the effects of a home detention sentence on prison overpopulation and fiscal cost-savings are easy to quantify, the potential benefits (or costs) to offenders are more difficult to identify but are of key importance to policymakers weighing the pros and cons of introducing such sentencing regimes.

I seek to contribute to the existing evidence on the effects of a home detention sentence by answering the following question: *Did the 2007 expansion of New Zealand's home detention sentence reduce the recidivism rate and improve the labour market participation of first-time offenders?*

The contribution of my thesis is twofold. First, I add to the sentencing-based criminal deterrence literature as the first econometric policy evaluation of home detention using New Zealand population-wide administrative data from Statistics New Zealand's Integrated Data Infrastructure. Second, I contribute to the New Zealand policy debate about the effectiveness of home detention as a rehabilitative and corrective criminal justice sanction.

In contrast to a previous review by the Ministry of Justice (2011), my results from a fuzzy regression discontinuity design and an instrumental variables approach show no evidence that the introduction of home detention reduced recidivism for first-time offenders within one, two or five years from the date of sentencing. Further, difference-in-differences regression analysis of average monthly employment rates, earnings and benefit receipt, controlling non-parametrically for calendar time trends, provides no evidence that a home detention sentence increases offenders' labour market attachment relative to short-term imprisonment, community detention or intensive supervision sentences. These results provide little justification for promoting home detention as a means for reducing crime or improving offenders' short-term or long-term labour market positions. Rather, my thesis suggests that the only established benefits of expanding New Zealand's home detention scheme are the cost-savings to the taxpayers and the easing of prison overpopulation.

The remainder of my thesis proceeds as follows: Section 2 provides a comprehensive literature review, including a brief theoretical framework for examining an individual's propensity to commit a crime and a summary of relevant estimation strategies adopted in studies of sentencing-based criminal deterrence policies; Section 3 provides the institutional background of home detention in New Zealand; Section 4 describes the data and defines the population of interest; Section 5 details the empirical identification strategy; Section 6 presents the results and a sensitivity analysis; and Section 7 concludes.

## Section 2 Literature Review

Becker (1968) provides a basic utility maximisation framework describing the choice to commit a crime as the decision between two options. One, an individual may choose not to commit a crime, a relatively risk-free option. Two, an individual may choose to commit a crime, in which case their expected utility will depend on whether they are caught and, if so, what penalty they receive. The second option therefore takes the nature of a gamble (Chalfin & McCrary, 2017). Naturally, it is assumed that if an individual commits a crime without being caught, they are better off than if they did not commit a crime. In summary, the model posits three possible outcomes with different utility levels: (1) the utility associated with abstaining from crime; (2) the utility from committing a crime and not getting caught; and (3) the utility resulting from committing a crime, getting caught and being penalised. The difference between the utility in the risk-free option and the expected utility from committing a crime will determine whether an individual will engage in criminal activity. The latter is modelled as follows:

$$EU_i = P_i U_i(Y_i - f_i) + (1 - P_i) U_i(Y_i)$$

where  $EU_i$  denotes the expected utility of committing a crime for individual  $i$ ;  $P_i$  is individual  $i$ 's perceived probability of being caught;  $Y_i$  is individual  $i$ 's income and  $f_i$  is the penalty if individual  $i$  is caught. A large empirical literature examines the degree to which changes in  $P_i$ ,  $f_i$  and  $Y_i$  deter a potential offender from crime. A recent review of this criminal deterrence literature was conducted by Chalfin and McCrary (2017) who categorise deterrence studies into three general groups.<sup>1</sup> First, the responsiveness of crime to the probability of being apprehended ( $P_i$ ). This stream includes studies that examine the effect of police manpower and policing intensity on crime rates. Second, the sensitivity of crime to changes in the severity of criminal sanctions ( $f_i$ ). Studies in this group analyse sentencing reforms, police-induced discontinuities in the severity of sanctions, and the enactment of three-strike laws and capital punishment. Last, the responsiveness of crime to labour market conditions ( $Y_i$ ). This literature considers whether crime can be deterred through positive incentives such as unemployment levels or relevant market wages.

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<sup>1</sup> The guidelines of these authors are followed for two main reasons. First, they provide the most up-to-date research in this field. Second, they have a strong focus on marrying research carried out by economists and criminologists. In my view, the integration of these disciplines provides well-rounded context to my research question.

My thesis is categorised by the second strand of the criminal deterrence literature. Analyses of sentencing policies consider the differentiation of Becker's (1968) expected utility model with respect to the penalty ( $f_i$ ). Chafin and McCrary (2017) found that most studies in this field examine sentencing regimes that raise the probability of being imprisoned or increase the length of an imprisonment sentence. Such changes increase the expected cost of participating in criminal activity, which Becker's offending model suggests would decrease overall crime. Empirical evidence has supported this notion. For example, Ehrlich (1975) reported significant negative elasticities for the relationship between murder rates and the probability of execution by capital punishment in the United States. Using Italian data, Drago, Galbiati and Vertova (2009) found that as an offender's expected sentence length increases by one month, the propensity to recommit a crime reduces by 1.25 percent. Similarly, Abrams (2012) demonstrated that sentence enhancements for gun-related crime in the United States resulted in a 5 percent reduction in gun robberies.

### **Home detention**

The enactment of home detention is a specific type of sentencing policy that theoretically increases the expected utility of committing a crime. A home detention sentence is often shorter in length and commonly perceived as a lighter sanction than imprisonment. The availability of home detention therefore decreases an individual's perceived penalty ( $f_i$ ) of being caught for a crime, so following Becker's expected utility model, the choice to engage in criminal activity becomes more attractive from a utility-maximisation perspective.

Since electronic monitoring is a relatively modern way of serving a criminal sanction, empirical evaluations of home detention sentencing schemes did not surface until the late twentieth century. Henneguelle *et al.* (2016) suggest there is little causal evidence on effects of home detention on recidivism worldwide, and Chafin and McCrary (2017) believe the most econometrically-robust studies in this field only eventuated in recent years. As with any evaluation, the most common specification issue faced by researchers is the identification of appropriate treatment and control groups. Assessing the causal effect of home detention as an alternative to incarceration requires offenders in each sentence to be identical on average. However, offenders assigned to different sentences are not ex-ante comparable (Hinnerich, Pettersson-Lidbom & Priks, 2016), particularly regarding their level of risk. Low-risk offenders are more likely to be sentenced to home detention (Bonta, Wallace-Capretta & Rooney, 2000)

and hence comparing their outcomes to that of imprisoned offenders naturally biases the causal estimation.<sup>2</sup>

In addition to the basic differences between offenders in each sentence, selection bias can arise from two aspects of the sentencing process. First, manipulation of sentence length might induce selection bias if the enactment of home detention skews judges' sentencing behaviour (Andersen & Andersen, 2014). The introduction of a new criminal sanction may influence a judge's tendency to impose a certain type of sentence (Andersen & Andersen, 2014; Larsen, 2017). For example, a maximum 12-month home detention sentence may be available for offenders of 'non-serious' crime. For offenders who commit crime on the margin of 'serious', lenient judges might be motivated to impose a shorter sentence to permit home detention and avoid imprisonment. This would bias the estimated effect of home detention as the harder criminals become treated to home detention when they should have received imprisonment; making recidivism rates of the home detention group higher than expected. Second, a simple comparison of offenders sentenced to home detention versus imprisonment risks sample bias due to the strong inverse and non-linear association between age and crime (Hirschi & Gottfredson, 1983). Nagin, Cullen and Jonson (2009) discussed the issue of the age-crime relationship for empirical evaluations of criminal behaviour. If home detention sentences are shorter in length than imprisonment sentences, offenders sentenced to home detention will be younger upon release and could have a higher propensity to reoffend. Thus, the baseline difference in recidivism rates for offenders in each sentence cannot reliably be attributed to the sentence itself.

Randomised experiments alleviate selection bias entirely (Angrist, 2005; Sullivan, 2011). A properly executed randomised experiment (with perfect compliance – an issue discussed in Section 5) will mitigate selection bias through creating almost identical treatment and control

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<sup>2</sup> Marklund and Holmberg (2009) reiterate the difficulty in choosing an appropriate comparison group with their review of two meta-analyses of home detention across various jurisdictions. The authors first analysed The Campbell Collaboration study on the effects of electronic monitoring. This analysis reviewed 125 different identification methods for evaluating the effects of electronic monitoring on recidivism rates, but only 14 were regarded as having used a reasonable control group. However, none of these studies were able to statistically prove the hypothesised negative relationship between home detention and recidivism. The second meta-analysis reviewed by Marklund and Holmberg (2009) was limited to offenders whose risk for recidivism was considered moderate or high. After refining the sample to only the studies that had appropriate control groups, 54 were shortlisted. The authors then filtered out the papers that did not consider relevant effect measures in their data, such as convictions for new offences. Only three evaluations remained useful: one in the state of Georgia (US), a Canadian study and a study in the UK. Again, none of these three studies concluded a statistically significant negative effect of home detention on recidivism.

groups that differ only by treatment assignment. However, as demonstrated by the 2001 Swedish electronic monitoring experiment (Wennerberg *et al.*, 2005), randomisation of criminal sanctions is difficult to administer in practice. In Sweden, 260 offenders were accepted into an electronic monitoring trial and were released from prison to serve the remainder of their sentence at home. When analysing the experiment, Wennerberg *et al.* (2005) found that selection into electronic monitoring was non-random. Accordingly, the authors generated a credible comparison group using matching methods; a useful empirical technique to ensure comparability between treatment and control groups (Crown, 2014). Results highlighted that the electronic monitoring group reoffended 12 percent less than the control group within three years of completion, though this difference was statistically insignificant.

This Swedish experiment demonstrates the harsh reality of conducting randomised experiments in the criminal justice arena (Angrist, 2005). Later studies have explored the use of other econometric designs, besides matching methods, to evaluate changes to sentencing policies in the absence of randomisation (Angrist, 2005; Larsen, 2017). A regression discontinuity design (RDD) is widely recognised as a rigorous alternative to randomised experiments (Berk, 2010; Berk, Barnes, Alhman & Kurtz, 2010; Lee & Lemieux, 2010; Lee & McCrary, 2009). An RDD has recently become a favourable strategy for policy evaluations, especially attributable to the works of David Lee and Justin McCrary (Chalfin & McCrary, 2017), but is most commonly applied in non-criminal justice research (Rhodes, Gaes & Cutler, 2018). From the subset of studies that have utilised an RDD within the economics of crime, it has proven to be a valuable estimation strategy. In their examination of the relationship between imprisonment sentence length and recidivism, Rhodes *et al.* (2018) demonstrated that an RDD is a strong identification strategy in the context of sentencing guidelines. This conclusion is supported by Franco, Harding, Bushway and Morenoff (2018) in their study of recidivism also using discontinuities in prison sentencing thresholds. Rhodes and Jalbert (2013) used an RDD when evaluating an intensive supervision program for probationers and concluded that “[an RDD] should be used as an ongoing evaluation tool to inform restructuring of threshold values for offender assignment to risk categories” (p. 241). Marie (2015) used an RDD to evaluate the effect of home detention on recidivism in England. While results showed a significant reduction in the probability of re-arrest, the offenders on home detention were all ex-prisoners, and therefore the causal estimate was a combination of both incarceration and electronic monitoring effects. Moreover, despite an RDD being a second-best substitute to a randomised experiment, there are no academically published economic papers that have used an RDD to analyse the introduction of home detention as a stand-alone sentence.

Difference-in-differences and instrumental variables estimations are more commonly used for quasi-experimental criminology research (see Angrist (2005) and Landerso, Nielsen and Simonsen (2015)). Examples of economic papers that have employed these techniques for estimating the effect of home detention on recidivism and offenders' labour market outcomes are summarised below.

Di Tella and Schargrotsky (2013) used an instrumental variables model to analyse the effect of home detention on recidivism in Argentina. To deal with the non-random assignment of offenders on home detention, the authors used judges' ideology (i.e., the tendency to sentence offenders to home detention) as an instrument. Given random assignment of offenders to judges in each Argentinian district, the instrumental variables estimate had causal interpretation. Di Tella and Schargrotsky (2013) found significant negative effects of home detention on re-arrest rates between 11 and 16 percentage points. However, like Marie's (2015) English RDD paper, all home detention offenders were initially sentenced to prison, so the causal estimates included the impact of incarceration and did not consider home detention in isolation. Henneguelle *et al.* (2016) extended Marie (2015) and Di Tella and Schargrotsky's (2013) findings by estimating the effect of a stand-alone home detention sentence in France. Using variations in access to home detention as an instrument, the long-run effect of home detention was estimated to reduced recidivism by 6 to 7 percentage points.

Andersen and Andersen (2014) studied the effects of both the 2006 and 2008 electronic monitoring expansions in Denmark, with respect to an offender's dependency on social welfare.<sup>3</sup> The authors estimated the causal effects using a difference-in-differences estimator under the identifying assumption that the reforms are exogenous to offender characteristics. Acknowledging the selection bias between home detention and imprisoned offenders, date of conviction was used as an instrument of randomisation around the policy change. Negative and statistically significant results were found for the 2006 reform, suggesting that young offenders who received their sentence post-2006 experienced lower dependency rates than their pre-2006 counterparts. In contrast, results from the 2008 reform showed that offenders above 25 years old followed the same labour market trajectory regardless of their sentence.<sup>4</sup>

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<sup>3</sup> Denmark's original electronic monitoring sentence was introduced as a prison-alternative for offenders under the age of 25 who received a maximum imprisonment sentence of three months for traffic-related crime. The categorical restriction was lifted in 2006 and the age cap removed in 2008. These two reforms authorised offenders of all crimes and ages to serve their three-month sentence via electronic monitoring (Andersen & Andersen, 2014).

<sup>4</sup> Larsen (2017) also analysed the effect of Denmark's 2006 expansion of electronic monitoring but looked at offenders' educational outcomes. Using an instrumental variables two-staged least squares estimation, results showed significant differences between offenders sentenced before and after the reform,

Furthermore, the empirical evidence on the effect of home detention on recidivism and offenders' labour market outcomes, with the use of a credible and unbiased control group, is scarce (Henneguelle *et al.*, 2016). The causal effect of home detention depends on the choice of model specification and the researcher's definition of recidivism, as well as the jurisdiction-specific electronic monitoring system and the supervision and parole limitations of each home detention sentence (Gibbs & King, 2003). Thus, the empirical contributions to the criminal deterrence literature are mixed and difficult to compare. This pattern is also evident amongst criminology studies in this field. For example, Marklund and Holmberg (2009) learned that, for the most part, offenders in the Swedish electronic monitoring program felt the experience went 'very well' and appreciated spending time with their families outside the prison environment. Similar opinions were shared by offenders released from prison to home detention between 1999 and 2000 in New Zealand (Gibbs & King, 2002; Gibbs & King, 2003).<sup>5</sup> In contrast, Bonta *et al.* (2000) found that an offender's perception of the effectiveness of electronic monitoring depended on the type of state-enforced program.<sup>6</sup> Offenders in corrections-based programs were less likely to perceive electronic monitoring as a positive influence on their criminal behaviour compared to offenders in court-based programs. Staff at the corrections-based programs believed only 27 percent of offenders were deterred from reoffending, while workers at the court-based program believed the success rate to be 38 percent. Further, Budd and Mancini (2015) looked specifically at America's perception of electronic monitoring as a mechanism of reducing sexual recidivism. Using a 2010 public opinion survey, the authors identified that 32 percent (47 percent) of Americans perceived electronic monitoring to be very (somewhat) effective in reducing sexual recidivism. While these qualitative comments do not contribute to the economic debate about the effectiveness of a home detention sentence, they do provide additional context to home detention regimes.

My thesis provides two contributions to this sentencing-based criminal deterrence literature. First, it is the only study to utilise an RDD for the introduction of home detention sentencing. Second, it is the first econometric evaluation of home detention as a stand-alone sentence using population-wide linked administrative New Zealand data.

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whereby electronic monitoring, relative to imprisonment, increased upper-secondary education completion rates by 11.2 percentage points one year after release.

<sup>5</sup> The New Zealand studies by Anita Gibbs and Denise King are discussed in Section 3 as part of New Zealand's home detention history.

<sup>6</sup> The Canadian states of British Columbia and Newfoundland administered corrections-based electronic monitoring programs, whereas Saskatchewan had a court-based program.

## Section 3 Background

In this section, I first provide a brief history of the development of electronic monitoring and home detention in New Zealand. I then discuss the nature of New Zealand's 2007 sentencing reform, including the home detention legislation, the purpose of the sentence and how offenders were affected by its implementation.

Electronic monitoring was originally developed by a group of researchers at Harvard University in the 1960s. It was first judicially sanctioned in Albuquerque, New Mexico in 1983 after Arizona state district judge, Jack L. Love, received permission from the state's highest court to use the electronic device (Gable, 2015). Thereafter, many jurisdictions worldwide have integrated home detention via electronic monitoring into their justice system.

New Zealand's first legislative provision for home detention was introduced by section 46 of the Criminal Justice Amendment Act 1993 (1993 No 43), which authorised a home detention pilot in Auckland between 1995 and 1997. The primary objective of the pilot was to ease the transition of prisoners back into the community with a gradual process of release (Ministry of Justice, 2011). Over the two-year period, 37 prisoners were released from imprisonment to home detention.<sup>7</sup> A 1997 evaluation of the pilot reported mixed results in terms of offenders' reintegration abilities. While detainees and their families reportedly established a more positive lifestyle that persisted beyond the sentencing period (Church & Dunstan, 1997), over 30 percent of the participating cohort reoffended after the program concluded (Gibbs & King, 2002). The Ministry of Justice recommended further testing of the electronic monitoring component of home detention before the policy extended nationwide (Church & Dunstan, 1997).

Despite the somewhat unfavourable outcome of the pilot, the Bolger-Shipley National Government continued to push for the enactment of home detention due to prison overpopulation issues and the need for more cost-effective correction mechanisms (Gibbs & King, 2002). Consequently, on 1 October 1999, 'front-end' and 'back-end' home detention regimes were introduced through the Criminal Justice Amendment Act 1999 (1999 No 9). Front-end home detention was available for offenders sentenced to a maximum imprisonment term of two years, while back-end home detention was available for offenders who received a determinate imprisonment sentence of more than two years but were eligible for parole after

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<sup>7</sup> Participation in the pilot program was relatively low because offenders perceived home detention as more restrictive than standard parole (Gibbs & King, 2003).

serving one-third of their sentence. For both front-end and back-end schemes, the sentencing court would grant the offender leave to apply to a District Prisons Board for release to home detention (Criminal Justice Amendment Act 1999 (1999 No 9), s 21D). The decision to approve or decline an offender's application was governed by section 103B of the Criminal Justice Amendment Act 1999 (1999 No 9). If approved, the offender could serve some or all of their imprisonment sentence via home detention.<sup>8</sup> If declined, the offender was encouraged to address the factors that lead to their rejection and could apply again to the Prison Board at a later date (King & Gibbs, 2003).

As foreshadowed in Section 2, Anita Gibbs and Denise King surveyed the 897 New Zealanders that had been on home detention within 18 months of the 1999 legislative changes. Case records and statistical material were collected, and 70 interviews were conducted with home detention detainees and their key stakeholders. Across three main papers (Gibbs & King, 2002; Gibbs & King, 2003; King & Gibbs, 2003), the authors concluded that the introduction of home detention was relatively successful as most offenders abided by their home detention conditions. The authors found that the most suitable candidates for home detention are offenders who have appropriate accommodation with supportive residents; who have organised activities and regularly attend rehabilitative programs; and who do not have drug- or alcohol-related problems.<sup>9</sup>

### **The 2007 reform**

On 1 October 2007, New Zealand established home detention as a sentence in its own right. Section 15A and section 15B were inserted into the Sentencing Act 2002, permitting a judge to directly sentence an offender to home detention without requiring an initial imprisonment sentence. The Act only allows a judge to do so if (a) the court is satisfied that the offence cannot

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<sup>8</sup> The average processing time for an application for leave to home detention was 48 days (King & Gibbs, 2003). Thus, for offenders who received front-end home detention, their sentence would usually comprise of approximately 48 days in prison and the remainder on home detention. As stated above, offenders who received back-end home detention would serve the first third of their imprisonment sentence before being released to home detention.

<sup>9</sup> A second examination of New Zealand's 1999 home detention scheme was conducted by the Strategic Analysis Team of the Policy Development Group at the Department of Corrections. In June 2007, the New Zealand Government directed the Department of Corrections to investigate and explain any ethnic disparities in the number of offenders who were granted leave to apply for release to home detention and the approval rates of such applications. The authors sampled the offenders potentially eligible for front-end home detention during the 2004/2005 financial year. Logistic regression results suggested that Māori appeared to be disadvantaged by the front-end home detention scheme. However, ethnic disparities in the access to home detention were largely explained by the ethnic differences in the offence seriousness and criminal history of the sampled offenders (Department of Corrections, 2007). These results suggested that New Zealand's 1999 home detention scheme was enforced equitably.

be punishable by any less restrictive sentence or combination of sentences; and (b) the judge would have otherwise imposed a short-term imprisonment sentence of a maximum of two years. In New Zealand, a home detention sentence ranges from a minimum of 14 days to a maximum of 12 months (Department of Corrections, n.d.). For individuals aged 18 or older, there are no legislative restrictions for the type of offence that warrants a home detention sentence.<sup>10</sup> Judges use their discretion in deciding when and how a sentence is imposed depending on the characteristics of the offence, the circumstances and actions of the offender, and the sentences received by other offenders who have previously committed similar crimes (Ministry of Justice, 2011).

New Zealand's current home detention sentence is both punitive and rehabilitative (Department of Corrections, n.d.), with a strong focus on the reintegration needs of offenders. Through restricting an individual's location to an approved place of residence, offenders are given the opportunity to preserve family relations and avoid gang associations – two advantages that are unlikely when serving a prison term (Andersen & Andersen, 2014). Individuals are ordered to wear an electronically monitored ankle bracelet which signals an alarm if there is any tampering or unapproved movement. If the offender violates their sentencing conditions, they will face case-specific repercussions, including reconviction, depending on the severity of the infringement. A probation officer will monitor the offender's behaviour with regular visits and report any breaches. In addition, the probation officer will encourage the offender to seek or retain employment throughout their sentence and attend rehabilitative programs in the community. Upon completing a home detention sentence, most offenders are required to comply with post-detention conditions for a specified period (Ministry of Justice, 2011). This process is also followed by offenders released from short-term imprisonment.

The 2007 policy reform therefore induced substantial change for how offenders are penalised. If two individuals of identical circumstances committed the same crime, but one was sentenced before 1 October 2007 and the other sentenced after, the sentencing reform dictates that the first offender could serve a 24-month imprisonment sentence whereas the second offender could serve a 12-month home detention sentence. Not only did the legislative provision halve an offender's sentence for all crime warranting a maximum imprisonment sentence of two years, it also enabled offenders to engage with the community throughout their sentence. Broadly speaking, this opportunity could be approached in three ways: (a) the offender could

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<sup>10</sup> Section 15B of the Sentencing Act 2002 provides that home detention can only be imposed on an offender aged 17 or younger if the crime committed was a purely indictable offence, such as murder, manslaughter or sexual violation.

attend rehabilitative programs or retain employment to improve their reintegration prospects; (b) the offender might avoid community involvement and stay at their approved place of residence for the duration of their sentence; or (c) the offender could more easily re-engage in crime since home detention has less surveillance than 24-hour prison security. Nonetheless, while the enactment of home detention changed the way an offender can serve their sentence, all criminal offences are still registered on the offender's criminal record.

There have been no academically published economic papers that have studied the effect of the 1 October 2007 New Zealand home detention reform.<sup>11</sup> However, the Ministry of Justice conducted a review of the home detention sentence in 2011 and found that offenders who served home detention had lower recidivism rates than those who served short-term imprisonment sentences. For offenders sentenced to home detention, recidivism was measured from the date of starting their sentence; for offenders sentenced to short-term imprisonment, recidivism was measured from the date of release. This compares offenders at different points of their criminal event timeline, which is likely problematic given offenders on home detention are still bound by their sentencing conditions while offenders released from short-term imprisonment are not. However, the Ministry of Justice justifies this time difference through wanting to compare recidivism in time periods where both groups of offenders were in the community. The proportion of offenders that were sentenced to home detention in 2007/2008 and 2008/2009 that were reconvicted in the following 12 months were 21.5 percent and 23.0 percent respectively, whereas the proportion of offenders that were released from short-term imprisonment sentences in the same period faced recidivism rates of 50.3 percent and 52.6 percent respectively (Ministry of Justice, 2011).

Adding to the findings from the Ministry of Justice, I aim to estimate the causal effect of serving a home detention sentence on recidivism and attachment to the labour market. I further explain why, and under what conditions, these relationships can be interpreted as causal. To preface this econometric discussion, the next section describes the data and defines the offender population of interest.

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<sup>11</sup> Morris and Sullivan (2015), of the New Zealand Treasury, believe there is little evidence in general regarding the impact of different sentences on subsequent outcomes for offenders in New Zealand.

## Section 4 Data

### 4.1 Integrated Data Infrastructure

I use full-population administrative data supplied by Statistics New Zealand's Integrated Data Infrastructure (IDI). The IDI data are sourced from government agencies, Statistics New Zealand surveys and non-government organisations, and includes individual-level information on a range of characteristics, such as education, training, income and work, benefits and social services, population, health, justice and housing (Statistics New Zealand, n.d.a). A key advantage of the IDI is the ability to link individuals across all databases using a unique identifier. Individuals are also assigned a locally unique justice identifier to establish a link between the New Zealand Police, Ministry of Justice and Department of Corrections datasets. Neither the IDI identifier nor the justice identifier is available if an individual has received court-ordered name suppression at the time of their charge. Suppression orders forbid the linking of individuals to the IDI as the court prohibits the publishing of any identification details related to that person. Should there be information related to a person's charge that is not protected by the suppression order, such information is recorded in the independent dataset(s) but there is no link for that person to the IDI spine. This accounts for approximately 2 percent of all charges (Statistics New Zealand, 2018).

The primary dataset is the Court Charges data from the Ministry of Justice. These data contain records of all disposed charges filed in a New Zealand criminal court since 1992.<sup>12</sup> The Sector Group within the Ministry of Justice produces a refreshed dataset every six months to incorporate charges that were resolved at the end of the preceding calendar or financial year. From 2004 onward, information about an individual's offence type, charge, conviction, court date and location, sentencing/charge outcome date, sentence type and length and a range of demographic characteristics are typed by court staff into the Case Management System on a near-daily basis.<sup>13</sup>

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<sup>12</sup> 'Disposed charges' refers to finalised cases where an outcome was determined by the court and recorded in the Case Management System.

<sup>13</sup> Prior to 2004, data were stored on the Law Enforcement System. Due to the transition of computer apparatus between 2003 and 2004, the Ministry of Justice acknowledge that trend analysis commonly shows a discontinuity around this time (Statistics New Zealand, 2018). In addition, while Statistics New Zealand believe there are limited gaps in the Court Charges data, they conceded that some information is missing in records prior to 2004. Name and date of birth data are absent for approximately 90,000 individuals, which consequently detached them from the IDI spine. For these reasons, I only use Court Charges data from 2004 onward.

I link four additional datasets to the Court Charges data to provide a full description of the offender population. The Personal Details data within Statistics New Zealand's Core Data are used to identify individuals' demographic information. Tax data from Inland Revenue provide information about offenders' financial and labour market positions. From 1999 onward, the Employer Monthly Schedule contains, amongst other things, earnings from wages and salaries, benefit receipt, superannuation, paid parental leave and student allowances (Statistics New Zealand, 2015a). Tertiary education data provided by the Ministry of Education relates to formal enrolment in courses, programs and qualifications from all tertiary education organisations that receive government funding (Statistics New Zealand, 2015b). Data regarding a student's qualification or course code, start and end dates, fees information, completion rates, decile of the last attended secondary school and the highest attained secondary school qualification are available from 1994 onward. As part of Statistics New Zealand's Population Data, the 2013 Census provides demographic information on all individuals that were in New Zealand on census night (Tuesday 5 March 2013), including their residential address, religiosity status, ethnicity, age and birthplace (Statistics New Zealand, 2015c).

#### 4.2 Population of interest

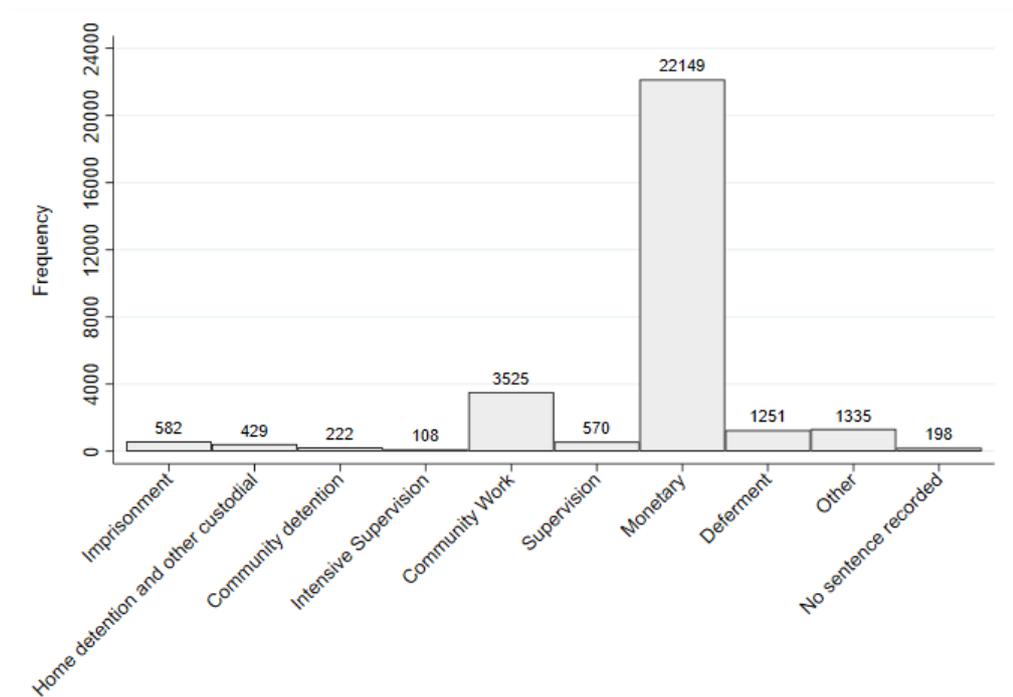
The population of interest comprises first-time offenders at the margin of home detention eligibility. These are the individuals whose most serious sentence received at their earliest sentencing date was either short-term imprisonment, home detention, community detention or intensive supervision. To arrive at this offender sample, I imposed four conditions on the Ministry of Justice's Court Charges data. First, I narrowed the data to offenders that were convicted and sentenced for their crime(s). This population concerns approximately 803,000 individuals between 1992 and 2017. Second, I situated offenders at their earliest sentencing date to avoid contamination across the treatment and control groups.<sup>14</sup> Approximately 47 percent of the 803,000 convicted offenders were sentenced on more than one occasion, so I only observe each individual at their first sentencing date so the causal effect of home detention for first-time offenders can be identified. Third, if an offender received more than one sentence at their earliest sentencing date, I only considered their most serious sentence received at this time. As above, this is to avoid treatment plurality but in a cross-sectional context. This condition is imposed using the *sentence ranking* variable in the Court Charges data. From most to least serious, this variable ranks the sentences as follows: (1) imprisonment; (2) home detention (which includes 'other custodial' for pre-2004 data); (3) community detention; (4) intensive supervision; (5) community work; (6) supervision; (7) monetary; (8) deferment; and (9) other.

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<sup>14</sup> An offender's sentencing date is referred to as their 'charge outcome' date in the Court Charges data.

To provide an example of the relative frequency of each of these sentences, Figure 1 illustrates the sentences received by first-time convicted offenders between 1 October 2007 and 1 October 2008, a population of 30,369 individuals. Monetary fines were clearly the most common sentence, followed by community work. The top four most serious sentences were administered the least.

Figure 1. Frequency of convicted offenders by sentence type



Notes: These sentences were administered to first-time offenders sentenced between 1 October 2007 and 1 October 2008.

Source: Own calculations based on data in Statistics New Zealand's IDI.

Fourth, my research question specifically targets offenders who were affected by the introduction of the home detention sentence. A direct interpretation of section 15B of the Sentencing Act 2002 indicates that substitution into home detention can only occur from a short-term imprisonment sentence. Thus, I restricted my sample to include first-time offenders with imprisonment sentences up to 730 days. Further, since home detention was enacted as the second most serious stand-alone sentence, it is possible (though not legislated) that substitution could also come from the third-ranked sentence.<sup>15</sup> The Ministry of Justice and the Department of Corrections are divided as to what the third most serious sentence is. The Ministry of Justice classifies community detention as the third-ranked sentence whereas the Department of

<sup>15</sup> While the possibility of both upper- and lower-threshold substitution is incorporated in the population of interest, a robustness test analyses recidivism for first-time offenders who received only short-term imprisonment or home detention sentences. As detailed in Section 6, the overall result is unchanged by the criteria of the offender sample.

Corrections grades intensive supervision as the third (Statistics New Zealand, 2018). To nullify this sentence categorisation dispute, both sentences are included as the lower threshold for home detention eligibility.<sup>16</sup> To summarise, the fourth condition imposed on the Court Charges data is refining the population of interest to only offenders who received short-term imprisonment, home detention, community detention or intensive supervision sentences.

#### 4.3 Estimation sample and key variables

The population of interest is observed in five windows either side of the sentencing reform. Specifically, 50 days, 100 days, 365 days, 730 days and 1,825 days before and after 1 October 2007. Within each observation window, the treatment group consists of those sentenced after 1 October 2007 and the control group consists of the population sentenced before 1 October 2007.<sup>17</sup> Table 1 shows the distribution of the offender sample by window size. Columns (1) and (2) show there are nearly 100 offenders in both the treatment and control groups within the 50-day observation window. The two groups are also relatively balanced when observing the 100-day window: the control group has 201 observations and the treatment group has 231. There are large differences in the number of observations across the two groups for the 365-day, 730-day and 1,825-day observation windows. These numbers lend support for the predictions of Becker's (1968) offending model (discussed in Section 2): more individuals chose to engage in criminal activity after 1 October 2007 as the introduction of home detention reduced the expected cost of committing a crime. This could explain why there are more observations in the treatment group than the control group for the wider observation windows; smaller windows are naturally less affected by crime trends.<sup>18</sup>

*Table 1. Population of interest by observation window*

	(1)	(2)	(3)
	<i>Control group</i>	<i>Treatment group</i>	<i>Total</i>
50 days	99	96	195
100 days	201	231	432
365 days	693	1,119	1,812
730 days	1,386	2,460	3,846
1,825 days	3,396	6,741	10,137

*Source:* Own calculations based on data in Statistics New Zealand's IDI.

<sup>16</sup> It is assumed that offenders who received community work, supervision, monetary or deferment sentences (ranked five to eight respectively by the Ministry of Justice) would have still received said sentences, irrespective of the home detention reform, as they are likely beyond the scope of home detention eligibility. Moreover, these offenders are excluded from the offender sample.

<sup>17</sup> The justification for defining the treatment and control groups in this way is explained in Section 5.

<sup>18</sup> Alternatively, the difference in observations between the treatment and control groups for the 365-day, 730-day and 1,825-day windows could be explained by crime rates responding to the record-high unemployment rate in 2008/2009 after the global financial crisis (Statistics New Zealand, 2012).

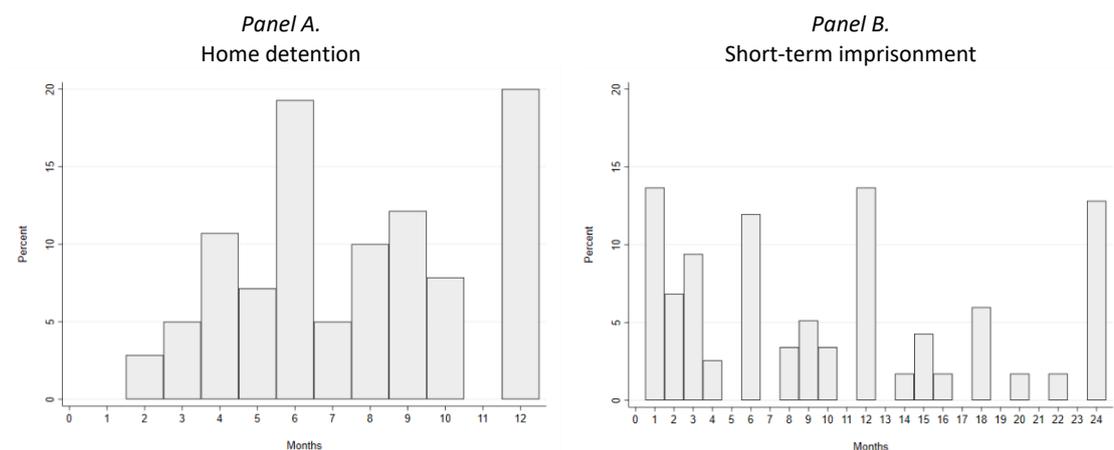
In evaluating the effect of a home detention sentence, I consider four outcomes of interest: recidivism rates, average wages and salaries earnings, employment rates and average benefit receipt. Recidivism is defined as the tendency of a convicted offender to reoffend. Recidivism is measured from an offender's sentencing date, rather than upon release, because it is a repercussion of home detention that the offender remains in society with (arguably) more of an opportunity to reoffend throughout their sentence. In contrast, reoffending possibilities are limited for imprisoned offenders due to the practical constraints of 24-hour prison security. To estimate the true effect of home detention on recidivism, I believe it is important that the definition of recidivism reflects all consequences of the sentence. I construct the recidivism variable as a dummy equal to one if the offender reoffends within 365 days after their first sentencing date, and again for recidivism within 730 days.<sup>19</sup> Further, recidivism is also considered in a long-term period, namely five years (1,825 days). Within this time frame, all offenders would have been released from their initial sentence, including offenders who received the maximum short-term imprisonment sentence of 24 months. Figure 2 provides visual evidence of the differences in sentence length between home detention and short-term imprisonment sentences administered to first-time offenders between 1 October 2007 and 1 October 2008. Panel A shows that a home detention sentence was most commonly six or twelve months, with similar six-monthly intervals also observed for imprisonment sentences in Panel B. Figure 2 suggests that almost one third of imprisoned offenders remained behind bars during the first 12 months that offenders are released from home detention. This extended incarceration period could affect the one- and two-year recidivism rates, so the inclusion of a five-year recidivism rate could add valuable insight.

The labour market outcomes are calculated from Inland Revenue's tax data. Individual monthly earnings from wages and salaries (measured in NZD) are sourced from the Employer Monthly Schedule between 2003 and 2010 and are censored to the 99<sup>th</sup> percentile to reduce the influence of outliers in my estimations. Employment rates are analysed with monthly employment indicators equal to one if an offender has positive earnings, and zero otherwise. Benefit receipt is defined by a binary indicator equal to one if an offender received positive benefit earnings, and zero otherwise.

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<sup>19</sup> A limitation of this variable is that it does not consider the curtailing of sentence length due to good behaviour and/or parole release. Roodman (2017) states that early release should be thought of as a reduction in the period of supervision, which in turn fast-tracks the opportunity to re-engage in crime. For the purposes of my thesis, it is assumed that the sentence assigned at the sentencing hearing is served in full by each offender. This assumption is made for every sentence type and all definitions of recidivism.

Figure 2. Sentence length by home detention versus short-term imprisonment



Notes: These sentences were administered to first-time offenders sentenced between 1 October 2007 and 1 October 2008. For confidentiality reasons, the percentage of home detention sentences of 1 or 11 months are suppressed. Likewise, suppression was required for imprisonment sentences that were 5, 7, 11, 13, 17, 19, 21 or 23 months. Source: Own calculations based on data in Statistics New Zealand’s IDI.

The independent variables used to show that the treatment and control groups are comparable are mostly represented by self-explanatory binary indicators. These include an offender’s gender, religiosity status, birthplace and educational attainment. An offender’s age at the time of the committing the offence is also included. I proxy an offender’s residential location by the location of their first court hearing, recorded in the Court Charges data. Ethnicity information is sourced from the Personal Details records in the Core Data.<sup>20</sup> Prioritisation rules are followed such that if an individual is affiliated with any ethnic group other than European, they will be categorised with that ethnicity. Following the Statistics New Zealand’s level one ethnicity classification, the ranking rules are: (1) Māori; (2) Pacific Peoples; (3) Asian; (4) Middle Eastern, Latin American or African (MELAA); (5) other; and (6) European. The offences committed by offenders are initially categorised using level one of the Australia and New Zealand Standard Offence Classification (Statistics New Zealand, n.d.b), which ranks offences into 16 groups.<sup>21</sup> Due to Statistics New Zealand’s IDI suppression rules for low observation counts, I further aggregate offences into the following six categories: (1) homicide, injury-causing and sexual offences; (2) dangerous acts, abduction and harassment offences; (3) robbery, extortion, burglary and theft;

<sup>20</sup> While an individual’s address and ethnicity information can be drawn from the 2013 Census, only about half the population of interest can be observed in the Census data. For this reason, variables that are sourced from the Court Charges or Core data are preferred.

<sup>21</sup> Specifically, these are: (1) homicide and related offences; (2) acts intended to cause injury; (3) sexual assault and related offences; (4) dangerous or negligent acts endangering persons; (5) abduction, harassment and other offences against the person; (6) robbery, extortion and related offences; (7) unlawful entry with intent/burglary, break and enter; (8) theft and related offences; (9) fraud, deception and related offences; (10) illicit drug offences; (11) prohibited and regulated weapons and explosives offences; (12) property damage and environmental pollution; (13) public order offences; (14) traffic and vehicle regulatory offences; (15) offences against justice procedures, government security and government operations; and (16) miscellaneous.

(4) fraud, deception, drugs and weapon crimes; (5) property offences, public order offences, traffic offences and offences against justice; and (6) miscellaneous offences and offences with inadequate information. This secondary aggregation preserves the ranking of the Australia and New Zealand Standard Offence Classification.

## Section 5 Identification Strategy

The objective of this thesis is to identify the causal effect of home detention on recidivism.<sup>22</sup> This research question can be expressed with the counterfactual outcome framework notation proposed by Rubin (1974). Equation 1 presents the individual causal effect estimation:

$$(1) \quad \Delta_i = Y_i(D_i = 1) - Y_i(D_i = 0)$$

where  $\Delta_i$  is the causal effect of treatment for individual  $i$ ; the first component is the potential outcome ( $Y_i$ ) when individual  $i$  is treated ( $D_i = 1$ ); and the second component is the potential outcome when individual  $i$  is not treated ( $D_i = 0$ ). Naturally, only one of these potential outcomes is observable for each individual: the potential outcome in the control state is not observed for an individual who receives treatment, and vice versa. The well-known solution to this problem is to derive an *average* treatment effect. Given my research aims to identify the causal effect of a home detention sentence on recidivism, I am interested in the average treatment effect of the sub-population of offenders that were ‘treated’ to home detention. This can be framed by Equation 2:

$$(2) \quad ATT = E(Y_i(1) | D_i = 1) - E(Y_i(0) | D_i = 1)$$

where the *ATT* parameter is the average treatment effect on the treated (where treatment is home detention receipt); the first component is the average recidivism rate for offenders sentenced to home detention ( $D_i = 1$ ) who in fact served home detention ( $Y_i(1)$ ); and the second component is the counterfactual average recidivism rate for offenders sentenced to home detention ( $D_i = 1$ ) had they not served home detention ( $Y_i(0)$ ). Again, only the former component is observable. In order to recover the ATT of home detention, a credible comparison group must be identified such that their observed outcomes are representative of the counterfactual potential outcomes of offenders sentenced to home detention. If the two groups are not statistically alike, the ATT estimation will suffer from selection bias.<sup>23</sup>

As mentioned in Section 2, randomised experiments are considered the ‘gold standard’ for combatting selection bias (Angrist, 2005; Sullivan, 2011). When done properly, random

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<sup>22</sup> For brevity, this section refers to the outcome of interest as recidivism, but the empirical analysis in Section 6 will also discuss the outcomes related to offenders’ labour market participation.

<sup>23</sup> See Appendix A for a discussion about selection bias in the Ministry of Justice’s 2011 review of home detention.

treatment assignment should remove concerns of selection bias. Further, a simple treatment-control mean comparison results in an unbiased ATT (Angrist, 2005). To estimate the true ATT in a non-experimental setting, however, econometric methods are required to identify treatment and control groups that are as good as randomly assigned (Crown, 2014).

One approach to achieve causal identification is a regression discontinuity design, exploiting discontinuities in treatment receipt arising from changes to, for instance, programs or policies. Such an approach requires a definitive cut-off point where some (or all) individuals receive or become eligible for treatment. If the eligibility rules are known, then it is possible to observe causal estimates by comparing the outcomes of individuals at the margin of the cut-off. The idea is that individuals who are sufficiently close to the cut-off are similar in all observable and unobservable characteristics. This approach only identifies an ATT parameter under strong assumptions and, in most cases, will instead identify a local average treatment effect (LATE).

The LATE measures the average treatment effect for those treated near the cut-off. Angrist, Imbens and Rubin (1996) state that “the average over the sub-population of those whose behaviour can be modified by assignment are likely to be informative about population averages of those who comply in the future, even if there is substantial heterogeneity in individual-level causal effects” (p. 450). The LATE parameter thus informs us about the behaviours of individuals most likely to be affected by rule changes in the future.

The LATE is computed locally around a policy cut-off or threshold ( $Z_0$ ), measured by a continuous running variable that determines treatment assignment. As the running variable reaches the cut-off, the probability of receiving treatment is induced to change. The LATE considers only the individuals for which their delivered treatment can be changed by the running variable. Specifically, those marginally above ( $Z_0^+$ ) and marginally below ( $Z_0^-$ ) the threshold (see Equation 3). These individuals are ‘compliers’ since  $D_i(0) = 0$  and  $D_i(1) = 1$  (Angrist, 2005). It is argued that, within a small observation window, compliers are almost identical in their observable and unobservable characteristics, with the only difference being whether they are on the left- or right-hand side of the cut-off (Lee & Lemieux, 2010). Treatment assignment can be assumed random within this window such that the LATE is recovered by comparing the average outcome ( $Y$ ) of those just above the threshold to those just below, as shown in Equation 3.

$$(3) \quad LATE = E(Y|Z_0^+) - E(Y|Z_0^-)$$

To estimate a LATE, consideration should be given to the type of treatment discontinuity caused by the policy change. If the probability of receiving treatment jumps from zero to one across the cut-off, then the discontinuity is considered ‘sharp’. If the change in the probability of treatment receipt is a factor less than one, then the discontinuity is ‘fuzzy’ since only some individuals are assigned into treatment. In the context of New Zealand’s home detention reform, the running variable that determines treatment assignment is an offender’s sentencing date. If an offender was sentenced after (before) 1 October 2007, they are (are not) eligible for the stand-alone home detention sentence. The reform therefore constitutes a fuzzy discontinuity since treatment is not guaranteed, but rather the probability of being sentenced to home detention increases across the cut-off. Moreover, the change in the recidivism rate before and after 1 October 2007 must be weighted by the discontinuity in treatment probability, formalised in Equation 4.

$$(4) \quad \widehat{Wald} = \frac{E(Y|Z = 1) - E(Y|Z = 0)}{E(D|Z = 1) - E(D|Z = 0)} = \frac{\textit{Reduced Form}}{\textit{First Stage}}$$

The Wald estimator provides a baseline manual estimate of the LATE. The numerator in Equation 4 is the difference in expected outcomes ( $Y$ ) for individuals whose sentencing date was after ( $Z = 1$ ) and before ( $Z = 0$ ) the 1 October 2007 cut-off and the denominator is the difference in the probability of being sentenced to home detention ( $D$ ) after and before the cut-off. In other words, the denominator adjusts the treatment effect estimation for the compliance rate. The numerator and denominator are known as the reduced form and first stage relationships, respectively. The Wald estimator is typically estimated using an instrumental variables estimation (see Appendix B for an explanation), where an offender’s sentencing date is used as an instrument for home detention receipt.

### **Fuzzy regression discontinuity design**

Though there are various estimation techniques that can recover a LATE, I employ a regression discontinuity design (RDD). As mentioned in Section 2, this is because an RDD is recognised as a second-best alternative to a randomised experiment and has successfully been used to recover unbiased causal effects of sentencing regimes using discontinuities in sentencing thresholds.

Estimates from an RDD identify whether small increments in eligibility rules are related to a specified outcome. Both the sharp and fuzzy RDD (FRDD) procedures represent different ways of taking the average outcomes for individuals who fall either side of a discontinuity, weighted by the change in treatment probability. An FRDD is estimated as follows:

$$(5) \quad \tau_{FRDD} = \frac{\lim_{\varepsilon \downarrow 0} E(Y_i | z_i = z_0 + \varepsilon) - \lim_{\varepsilon \uparrow 0} E(Y_i | z_i = z_0 + \varepsilon)}{\lim_{\varepsilon \downarrow 0} P(D_i = 1 | z_i = z_0 + \varepsilon) - \lim_{\varepsilon \uparrow 0} P(D_i = 1 | z_i = z_0 + \varepsilon)}$$

where the numerator captures the change in the average outcome ( $Y_i$ ) for offenders whose sentencing date ( $Z_i$ ) is marginally above ( $\varepsilon \downarrow 0$ ) and marginally below ( $\varepsilon \uparrow 0$ ) the 1 October 2007 threshold ( $Z_0$ ). Again, the denominator is the discontinuity adjustment whereby the treatment effect is scaled by the change in the probability of receiving a home detention sentence induced by the cut-off.

While Equation 5 is similar to Equation 4, the FRDD differs from the Wald estimator in three main ways. First, an FRDD produces a weighted LATE where observations closest to the threshold can be given more weight in the treatment estimation than those further away (Lee & Lemieux, 2010).<sup>24</sup> The relative contribution of each observation depends on the kernel function selected by the FRDD algorithm. An FRDD estimation is only equivalent to the Wald estimator when imposing a uniform kernel on a model without covariates (Angrist, 2005). Second, the FRDD estimation is an optimising procedure whereby the algorithm uses all available data to select the optimal bandwidth from which the LATE is recovered. In contrast, the Wald estimator requires an observation window to be imposed on the model, often determined arbitrarily by the researcher. Third, an FRDD can estimate the standard errors and statistical power of the LATE estimates, while these statistics from the manual Wald estimator require additional adjustment.

## 5.1 Assumptions

$\tau_{FRDD}$  in Equation 5 is causal under four assumptions: The Stable Unit Treatment Value Assumption; independence; the exclusion restriction; and monotonicity (Angrist *et al.*, 1996). Since these assumptions are also required for the instrumental variables model (see Appendix B), I refer to the sentencing date running variable as an instrument in this section. Three additional requirements justify the use of an RDD to estimate the LATE: (i) a first stage; (ii) no manipulation of the running variable; and (iii) covariate continuity. These three requirements are substantiated in Section 6.

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<sup>24</sup> FRDD is implemented using a triangular kernel in Section 6. While other weighting kernels could also be used, Lee and Lemieux (2010) showed that the choice of kernel typically has little impact on the resulting estimate. Sensitivity tests in Section 6 analyse whether there is a difference between a weighted LATE versus a non-weighted LATE estimation, or equivalently, an FRDD estimation with a triangular kernel compared to a uniform instrumental variables model or a manual Wald estimator.

The Stable Unit Treatment Value Assumption (SUTVA) requires an offender's potential outcomes to be unrelated to the treatment status of other individuals (Angrist *et al.*, 1996). This assumption can be referred to as a partial equilibrium assumption (Morgan & Winship, 2007) because an offender's propensity to reoffend depends solely on their own treatment status. Since only one potential outcome is observed for each offender,<sup>25</sup> SUTVA makes it possible to recover the average treatment effect for those who received treatment. In the absence of SUTVA, an offender's potential outcomes also depend on the treatment status of other offenders. Consequently, the number of potential outcomes increases exponentially and the task of defining an average treatment effect becomes challenging. The economic literature offers few techniques to deal with situations of SUTVA violations (Morgan & Winship, 2007), the nature of which are beyond the scope of this thesis.

The second assumption requires that the instrument is as good as randomly assigned: it must be independent of potential outcomes and potential treatment assignment. This assumption implies that the reduced form is the unbiased average causal effect of the instrument on the dependent variable ( $Z$  on  $Y$ ) and, as such, the first stage is the unbiased average causal effect of the instrument on treatment assignment ( $Z$  on  $D$ ), expressed as follows:

$$\text{Independence: } [Y_i(D_i, 1), Y_i(D_i, 0), D_{1i}, D_{0i}] \perp Z_i$$

where  $Y_i(D_i, 1)$  is the potential outcome for a treated individual;  $Y_i(D_i, 0)$  is the potential outcome for a non-treated individual;  $D_{1i}$  and  $D_{0i}$  are the potential treatment assignments; and  $Z_i$  is the instrument for individual  $i$ . This assumption is satisfied by the nature of the sentencing date instrument. As a sentencing date is simply a point in time, it cannot be correlated with an offender's potential outcomes or the potential sentences available to the offender. The only way that the instrument has a direct effect on potential outcomes is through delivered treatments (Angrist, 2005). Given this assumption, the inclusion of covariates, such as age, sex, race or education, is unnecessary for the treatment effect estimation (Rhodes *et al.*, 2018).<sup>26</sup>

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<sup>25</sup> That is, the outcome in the treated state is only observed for offenders that were in fact treated and the outcome in the untreated state is only observed for offenders that were not treated.

<sup>26</sup> Covariates could be included in the causal estimation if treatment is conditional on certain characteristics or if the covariates have predictive power in the model and thus could reduce residual variance (Angrist, 2005). Otherwise, covariates are only necessary for establishing comparability between the treatment and control groups (as shown in Table 3).

The exclusion restriction is that the instrument only affects the outcomes of interest through the change to home detention probability, more formally expressed as:

$$\text{Exclusion Restriction: } Y_i(D, 0) = Y_i(D, 1) \equiv Y_{Di} \text{ for } D = 0, 1$$

The sentencing date instrument satisfies the exclusion restriction by construction.

The monotonicity assumption is an interesting feature of this LATE setup. Monotonicity acknowledges that the instrument may not affect the treatment status of all individuals (Angrist, 2005), but everyone affected are affected in the same way, such that:

$$\text{Monotonicity: } D_{1i} \geq D_{0i} \text{ for all } i, \text{ or vice versa.}$$

That is, there are not allowed to be any defiers: the individuals for whom the instrument decreases the probability of treatment. Under this assumption, the treatment effects only refer to that of compliers: the individuals for which the instrument affects their treatment status. The LATE is not informative about the population that are always-takers (defined as  $D_i(0) = D_i(1) = 1$ ) or never-takers (defined as  $D_i(0) = D_i(1) = 0$ ) because by definition, treatment status for these groups is unaffected by the instrument (Angrist & Pischke, 2009). Monotonicity is implied by designs where the control group is prohibited from receiving treatment (Angrist *et al.*, 1996). Offenders sentenced before 1 October 2007 are legally prevented from receiving a stand-alone home detention sentence because penalties cannot be applied retrospectively (New Zealand Bill of Rights Act 1990, s 26(1)). If no one in the control group received treatment, then  $D_i(0) = 0$  for all offenders, and hence, always-takers cannot exist (Angrist, 2005).<sup>27</sup> In this situation, the LATE equals the ATT. This is because the ATT is the weighted average of the treatment effects of always-takers and the proportion of compliers assigned into treatment; formally stated as the ratio of the intention-to-treat (ITT) parameter to the compliance rate. However, if there are no always-takers, assigned treatment is equivalent to delivered treatment, so the ATT ratio becomes the reduced form divided by the first stage. In other words, without always-takers the ATT represents the average treatment effect for compliers: the definition of a LATE. This is known as the Bloom Result (Angrist & Pischke, 2009) shown in Equation 6.

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<sup>27</sup> The absence of always-takers is also known as one-sided non-compliance (Angrist, 2005).

$$(6) \quad \frac{E(Y_i|Z_i = 1) - E(Y_i|Z_i = 0)}{P(D_i = 1|Z_i = 1)} = \frac{ITT}{Compliance} = E(Y_{1i} - Y_{0i}|D_i = 1) = ATT = LATE$$

Furthermore, under the assumption that always-takers do not exist, FRDD can recover both a LATE and an ATT of home detention. The stronger the instrument (as determined by a high correlation with treatment status (Crown, 2014)) the smaller the likelihood of defiers and consequently, the less sensitive the estimates are to violations of the exclusion restriction and monotonicity assumptions (Angrist *et al.*, 1996). However, should always-takers exist, the ATT estimate would suffer from sample bias, the size of which depends on the proportion of always-takers and whether the causal effects are heterogeneous between compliers and always-takers (Angrist *et al.*, 1996; Yamamoto, 2016).

Testable assumptions

(i) A first stage

The requirement for a first stage is fundamental to an RDD estimation because there must be a discontinuity in the probability of receiving treatment (Lee & Lemieux, 2010). Put differently; there must exist a non-zero average causal effect of the instrument on treatment assignment ( $Z$  on  $D$ ), as shown below.

$$E(D_i(1) - D_i(0)) \neq 0$$

If a first-stage relationship is not identified, the running variable does not determine treatment assignment, and hence the policy change did not induce the exogenous variation required for an RDD analysis.

(ii) No manipulation of the running variable

A treatment effect can only be interpreted as causal if individuals have imperfect control over treatment assignment (Stevens, 2016). Self-selection into treatment can occur by manipulation of the running variable. In the present context, offenders can behave in a way that makes them a more attractive candidate for a home detention sentence in the post-reform regime, but there must remain an element of chance that determines their sentence (Lee & McCrary, 2009). This assumption safeguards against selection bias, enabling sentence assignment to be as good as random within the observation window around the cut-off (Stevens, 2016).

The Lawyers and Conveyancers Act (Lawyers: Conduct and Client Care) Rules 2008 supports this assumption by stating that lawyers have an absolute duty of honesty to not mislead or deceive

the court. Lawyers cannot manipulate court hearing or sentencing dates to attain a lesser penalty for their clients, so the frequency of sentencing hearings should be continuous immediately around the 1 October 2007 cut-off. This can be tested by a running variable density test around the threshold, as proposed by McCrary (2008). If court hearings are bunched just after the cut-off, this would indicate that offenders (or their legal representatives) orchestrated their sentencing date to receive treatment.

(iii) Covariate continuity

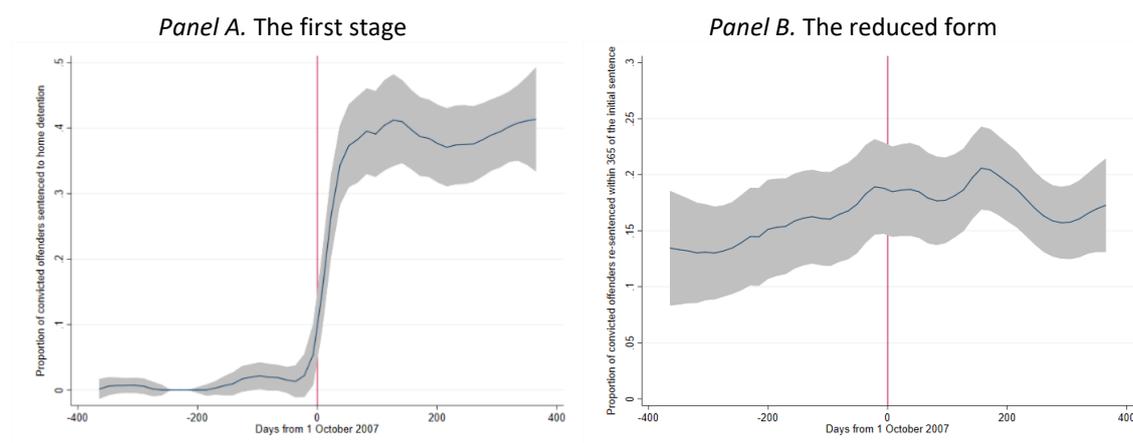
A notable feature of an RDD is that there is no value of the running variable ( $Z_i$ ) at which both the treatment ( $Z_i \geq Z_0$ ) and control ( $Z_i < Z_0$ ) states are observable for a given individual. The strength of an RDD estimate relies on the willingness to extrapolate outcomes across the threshold for the individuals that fall within the discontinuity window (Angrist & Pischke, 2009). Covariate continuity must be established to confirm that individuals marginally below the threshold are a good comparison group for those marginally above the threshold. The smaller the window, the more likely the two groups will be statistically comparable.

This requirement can be validated through tests of covariate balance (Lee & Lemieux, 2010). These mean-difference t-tests inspect whether the distribution of offender characteristics is continuous at the threshold and hence identifies whether the only variation across the cut-off is the probability of being sentenced to home detention. Agodini, Thornton, Kahn and Peikes (2002) state that a control group is well-matched to its respective treatment group if 95 percent of the covariate distribution tests fail to identify a statistically significant difference in the mean of each group.

## Section 6 Results

My primary analysis examines recidivism rates for first-time offenders sentenced to home detention within 365 days of 1 October 2007, relative to short-term imprisonment, community detention or intensive supervision sentences in the same period. As mentioned in Section 5, one method for estimating the LATE is a manual calculation of the Wald estimator (Equation 4); presented visually in Figure 3.

Figure 3. The Wald estimate of the LATE of home detention on a one-year recidivism rate



Notes: The x-axis on Panels A and B is offenders' sentencing dates measured on a calendar timeline with 1 October 2007 as day zero. The grey scale area represents the 95 percent confidence interval.

Source: Own calculations based on data in Statistics New Zealand's IDI.

The denominator of the Wald estimator is shown in Panel A. The probability of being sentenced to home detention increases by 37 percentage points as offenders' sentencing dates cross the 1 October 2007 cut-off. The numerator of the Wald estimator is shown in Panel B, demonstrating the relationship between offenders' sentencing dates and a one-year recidivism rate across the 1 October 2007 cut-off. This reduced-form effect equates to nearly 2.6 percentage points. Taking the ratio of the (unrounded) reduced form and first stage effects, the Wald estimate of the LATE of home detention on a one-year recidivism rate is approximately 6.8 percent. This estimate suggests offenders sentenced to home detention are 6.8 percent more likely to reoffend within one year than offenders sentenced to short-term imprisonment, community detention or intensive supervision.

This Wald estimate of 6.8 percent is the baseline parameter for the LATE of home detention on recidivism. In what follows, Section 6.1 compares the Wald estimate to the LATE parameters recovered from FRDD and instrumental variables procedures, and across two- and five-year recidivism periods. To validate these estimates, the existence of a first stage, covariate

continuity and no manipulation of the running variable are demonstrated in Section 6.2. Further, in Section 6.3, I explore a range of model specifications to assess the robustness of these estimates. These include the time window for which the population of interest is observed (6.3.1), the weighting kernel and bandwidth used in the FRDD (6.3.2), the home detention eligibility criteria (6.3.3), the definition of recidivism (6.3.4), the identification variable for the observed sentence (6.3.5) and the exclusion of reconvictions from breaches of home detention conditions (6.3.6). In Section 6.3.7, I run a group of placebo estimations using different policy introduction dates. Finally, in Section 6.3.8, I discuss the difference in the expectation of future sentences between the treatment and control groups and how future research could further explore this issue.

## 6.1 LATE estimation methods

LATE estimates from the FRDD and two-staged least squares (2SLS) instrumental variables estimations are provided in Table 2. The 2SLS LATE estimates in column (2) are similar to the Wald estimates in column (1) since both procedures put equal (uniform) weight on each observation within the 365-day window of 1 October 2007. For each FRDD specification in column (3), the algorithm optimised a bandwidth and weighting kernel using the full period of Court Charges data, ranging from 1992 to 2017.<sup>28</sup>

*Table 2. LATE estimates of home detention on recidivism by estimation method*

	(1) <i>Wald estimator</i>	(2) <i>2SLS</i>	(3) <i>FRDD</i>
365-day recidivism	0.068	0.069	0.059
730-day recidivism	0.092	0.084	0.101
1,825-day recidivism	0.056	0.038	0.075

*Notes:* The Wald estimator in column (1) and the 2SLS estimation in column (2) imposed uniform weighting on observations within the 365-day window. The FRDD estimations in column (3) optimised a triangular kernel weighting function for all specifications. Asterisks indicate the significance level of the LATE estimate where \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .

*Source:* Own calculations based on data in Statistics New Zealand's IDI.

Table 2 shows the LATE for a one-year recidivism rate lies between 5.9 and 6.9 percent across the three models. The LATE estimates are larger in magnitude when considering a two-year recidivism period: ranging from 8.4 to 10.1 percent. The greatest difference between the Wald, 2SLS and FRDD LATE estimates occur for the 5-year recidivism rate. However, all LATE estimates

<sup>28</sup> The inclusion of the full Court Charges data period is to reduce researcher degrees of freedom since economists are yet to conclusively determine the optimal bandwidth selection for a non-parametric estimation (Lee & Lemieux, 2010). For the one-year recidivism rate, the FRDD algorithm optimised a 1,310-day bandwidth. The bandwidths were slightly smaller for the two-year and five-year recidivism rates: 1,283 and 1,222 days, respectively. All FRDD models optimised a triangular kernel.

are notably positive and not significantly different from zero. Table 2 suggests that, across FRDD and 2SLS specifications, there is no statistically significant evidence that a home detention sentence reduces recidivism for offenders at the margin of home detention eligibility.

## 6.2 Model validity

### (i) A first stage

The first stage relationship is shown in Panel A of Figure 3, where the probability of home detention receipt increases by 37 percentage points as offenders' sentencing dates cross the 1 October 2007 implementation date. This clear exogenous change verifies that the 2007 legislative provisions were applied almost immediately in New Zealand's jurisdiction and directly affected the sentencing outcomes of the population of interest.

### (ii) No manipulation of the running variable

The McCrary test formally identifies whether there is a statistically significant difference in the density function of the sentencing date running variable around 1 October 2007. The null hypothesis is that the density of the running variable is continuous across the cut-off, i.e., the running variable discontinuity is zero. The distribution of sentencing hearings within each observation window is shown in Appendix Figure C1. In the 50-day, 100-day and 1,825-day windows, I cannot reject the null hypothesis, with respective p-values of 0.852, 0.267 and 0.409 (see Panels A, B and E of Appendix Figure C1). Within these observation windows, running variable manipulation is non-existent and sentence assignment is as good as random for the population of interest. The opposite is true for the 365-day and 730-day windows: there is a statistically significant difference in the density of sentencing hearings across the threshold. The null hypothesis of no discontinuity is rejected in Panels C and D of Appendix Figure C1, with respective p-values of 0.002 and 0.013.<sup>29</sup>

### (iii) Covariate continuity

The continuity assumption requires that the treatment and control groups are, on average, identical across observable characteristics. Covariates are compared under four categories: (i) demographic, (ii) crime, (iii) education; and (iv) earnings. Table 3 presents the covariate balance

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<sup>29</sup> When observing the number of observations in each of these time windows (shown in Table 1), there are almost double the number of offenders in the treatment group than the control group. This would likely explain why there is a discontinuous density function in the frequency of sentencing hearings across the thresholds for these observation windows: there are simply more people being sentenced from 2008 onward. However, this does not explain why the 1,825-day window does not reject the McCrary test null hypothesis.

for the population of interest within a 365-day observation window. For the covariate balance within the 50-day, 100-day, 730-day and 1,825-day windows, see Appendix Tables C1, C2, C3 and C4, respectively.

The differences in the composition of the treatment and control groups in Table 3 are economically trivial. Approximately 74 percent of each group are male, who are approximately 29 years old at the time of committing their crime. 57 percent of offenders are European, 26 percent are Māori and 10 percent are Pacific Peoples.<sup>30</sup> Using first-court location codes as a proxy for offender location, the distribution of offenders in the treatment and control groups across New Zealand's major cities is statistically equivalent.<sup>31</sup> NCEA Level 1 is the most common level of educational attainment for all offenders, with only about 17 percent having last attended a high-decile secondary school.<sup>32</sup> Nearly 30 percent of each group committed either homicide, injury-causing or sexual offences (Category 1), while 5 percent (19 percent) committed Category 2 (Category 3) offences.<sup>33</sup>

When comparing gross earnings and benefit receipt across the treatment and control groups, two methods are adopted. First, a direct comparison of the annual earnings in pre-treatment calendar years. The mean earnings in 2006 from wages and salaries are \$51,066 for the control group and \$62,598 for the treatment group, but the difference is insignificant. The second earnings comparison considers the age-to-earnings trajectory. As the treatment group are younger than the control group in the 2006 calendar year, the annual wage and salary earnings from the treatment group in 2006 is compared to that of the control group in 2005. This cross-over of calendar time eliminates any age effects on earnings. The difference between the two groups equates to \$7,874 and remains insignificant.

Furthermore, the t-statistics in Table 3 show no significant differences in any of the characteristics listed above. Table 3 verifies that the treatment and control groups are

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<sup>30</sup> These findings are consistent when using ethnicity indicators in the 2013 Census data, except there exists a significant difference in the Asian composition of the treatment and control groups. However, as mentioned in Section 4, less than half the population of interest is included in the 2013 Census dataset so comparisons of ethnicity using the Personal Details data are more representative.

<sup>31</sup> These results are somewhat consistent when using Statistics New Zealand's Address Notification (Full) dataset to identify an individual's region. Of the 16 location variables, there is a significant difference in the proportion of offenders located in the North Island, Wellington and in Southland. Nonetheless, the Address Notification (Full) dataset only links half the population of interest, so the court location proxies are more representative for offenders analysed in this thesis.

<sup>32</sup> Note that the Ministry of Education data account for only approximately one-third of the population of interest.

<sup>33</sup> Given offence type is continuous across the 1 October 2007 threshold, this suggests the level of criminality between the treatment and control groups is constant. Therefore, changes in New Zealand's overall crime rate between 2006 and 2008 does not distort the comparability of the two groups.

statistically comparable within a 365-day window either side of 1 October 2007. As shown in Appendix Tables C1 and C2, the treatment and control groups observed within a 50-day and 100-day window also satisfy the covariate balance test, but clear significant differences are observed within a 730-day and 1,825-day window (Appendix Tables C3 and C4).

*Table 3. Covariate balance for sampled offenders sentenced 365 days before and after 1 October 2007*

	<i>Control group</i>		<i>Treatment group</i>		<i>t-statistic</i>
	<i>N</i>	<i>Mean (SD)</i>	<i>N</i>	<i>Mean (SD)</i>	
<i>Demographic</i>					
Male	684	0.75 (0.43)	1,104	0.73 (0.44)	0.91
Age	669	29.9 (12.7)	1,104	29.0 (12.7)	1.46
Religious	246	0.47 (0.50)	432	0.51 (0.50)	-1.01
Born in NZ	276	0.87 (0.34)	474	0.83 (0.38)	1.43
European	687	0.58 (0.49)	1,116	0.56 (0.50)	1.24
Māori	687	0.26 (0.44)	1,116	0.26 (0.44)	0.09
Pacific Peoples	687	0.10 (0.29)	1,116	0.11 (0.31)	-0.83
Asian	687	0.04 (0.18)	1,116	0.04 (0.20)	-0.84
MEELA	687	0.02 (0.14)	1,116	0.03 (0.17)	-1.49
Auckland City	693	0.08 (0.27)	1,119	0.09 (0.28)	-0.84
Christchurch City	693	0.09 (0.29)	1,119	0.09 (0.28)	0.56
Wellington City	693	0.04 (0.19)	1,119	0.05 (0.21)	-0.58
<i>Crime</i>					
Category 1	693	0.28 (0.45)	1,119	0.28 (0.45)	0.35
Category 2	693	0.04 (0.19)	1,119	0.06 (0.23)	-1.64
Category 3	693	0.18 (0.38)	1,119	0.19 (0.40)	-0.98
Category 4	693	0.32 (0.47)	1,119	0.30 (0.46)	0.77
Category 5	693	0.16 (0.37)	1,119	0.15 (0.36)	0.38
Category 6	693	0.02 (0.14)	1,119	0.02 (0.13)	0.65
<i>Education</i>					
NCEA 1	252	0.18 (0.38)	423	0.20 (0.40)	-0.65
NCEA 2	252	0.06 (0.23)	423	0.09 (0.29)	-1.68
NCEA 3	252	0.13 (0.34)	423	0.16 (0.37)	-1.14
Low decile	222	0.47 (0.50)	381	0.43 (0.50)	0.97
Medium decile	222	0.35 (0.48)	381	0.42 (0.49)	-1.56
High decile	222	0.18 (0.39)	381	0.16 (0.37)	0.75
<i>Earnings</i>					
Gross 2006 WS	693	51,066 (161,004)	1,119	62,598 (258,002)	-1.06
Gross 2005 WS	693	54,724 (169,087)	1,119	55,338 (241,006)	-0.06
Gross WS year mix	693	54,724 (169,087)	1,119	62,598 (258,002)	-0.71
Gross 2006 BEN	693	22,814 (133,202)	1,119	16,770 (60,574)	1.31
Gross 2005 BEN	693	19,439 (129,979)	1,119	14,240 (55,074)	1.18

*Notes:* Offences are categorised as follows: (1) homicide, injury-causing and sexual offences; (2) dangerous acts, abduction and harassment offences; (3) robbery, extortion, burglary and theft; (4) fraud, deception, drugs and weapon crimes; (5) property offences, public order offences, traffic offences and offences against justice; and (6) miscellaneous offences and offences with inadequate information. Each NCEA level represents the highest attained secondary school qualification. School deciles are from the last attended secondary school, ranked as follows: 1-3 is low, 4-7 is medium and 8-10 is high. WS are earnings from wages and salaries. BEN are earnings from benefit receipt. Gross WS year mix observes wages and salaries earnings from 2006 for the treatment group and 2005 for the control group. All earnings variables are measured in NZD. Standard deviations are in parentheses. Equality of means are tested by the t-statistic where statistically significant differences are indicated with \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .

Source: Own calculations based on data in Statistics New Zealand's IDI.

Overall, there is not one observation window that satisfies all three of the testable RDD requirements while also being wide enough to account for seasonality (defined as having all 12 calendar months included at least once on both sides of the cut-off). Table 4 summarises the econometric trade-offs faced by each observation window. The population of interest within a 365-day observation window is my preferred specification because, despite rejecting the McCrary test (column (2)), there is a significant first stage (column (1)), there are zero significant differences in the mean covariates of the treatment and control groups (column (2)), and the treatment and control groups are both observed for all 12 calendar months.

Table 4. Econometric trade-off for the population of interest by observation window

	(1) <i>A first stage</i>	(2) <i>McCrary test</i>	(3) <i>Covariate continuity</i>	(4) <i>Accounting for seasonality</i>
50 days	Yes	0.852	2	No
100 days	Yes	0.267	4	No
365 days	Yes	0.002	0	Yes
730 days	Yes	0.013	7	Yes
1,825 days	Yes	0.409	10	Yes

Notes: 'Yes' in column (1) indicates that there is a statistically significant discontinuity in treatment receipt across the 1 October 2007 cut-off. Column (2) provides the p-values for the McCrary density test for which the null hypothesis is no discontinuity in the density of the running variable. Column (3) states the number of covariates from the mean-difference t-test tables that are significantly different across the treatment and control groups. In total, 29 covariates are tested. Column (4) indicates whether the observation window accounts for seasonality. Seasonality is defined as having all 12 calendar months included at least once on both sides of the cut-off.

Source: Own calculations based on data in Statistics New Zealand's IDI.

### 6.3 Robustness tests

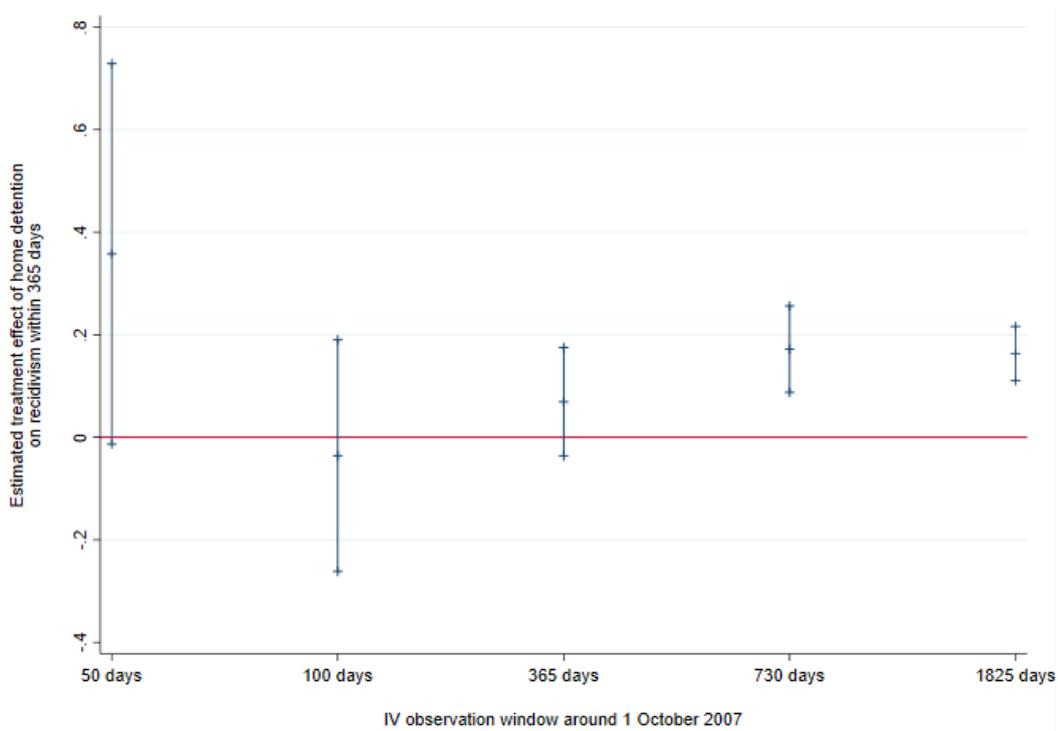
In this robustness section, I examine how the LATE estimates in Table 2 behave when the empirical design is modified. Results from all robustness tests suggest the overall conclusion, that home detention has no significant effect on recidivism for first-time offenders at the margin of home detention eligibility, is not sensitive to variations of the model specification.

#### 6.3.1 2SLS observation window selection

Here, LATE parameters from 2SLS estimations are computed using a variety of observation windows. In addition to the econometric trade-off faced by each window (summarised in Table 4), the window choice is important for two other reasons. First, data must be observed in a small enough window that the likelihood of model misspecification is minimal, especially since the plausibility of an RDD relies on the ability to distinguish a polynomial trend from a discontinuous treatment function (Angrist & Pischke, 2009). Second, there must be enough data within the window to yield precise estimates; a choice that presents a bias-variance trade-off (Angrist & Pischke, 2015). Therefore, 2SLS estimates for the effect of serving home detention on one-year

recidivism rates are computed for the population of interest observed within 50, 100, 365, 730 and 1,825 days of 1 October 2007. Each LATE and the respective 95 percent confidence interval are shown in Figure 4.

Figure 4. Instrumental variables LATE estimates of home detention on a one-year recidivism rate



Notes: The upper and lower limits represent the 95 percent confidence interval for the estimated LATE within each observation window. The LATE estimates for the 50-day, 100-day and 365-day windows are insignificant ( $p > 0.05$ ). The LATE estimates for the 730-day and 1,825-day windows are statistically significant at the 1 percent level ( $p = 0.00$ ).

Source: Own calculations based on data in Statistics New Zealand's IDI.

Each instrumental variables specification confirms there is no statistically significant evidence that home detention reduces recidivism for offenders at the margin of a home detention sentence. In fact, the LATE estimates within the 730-day and 1,825-day observation windows are positive and statistically significant at the 5 percent level. Figure 4 suggests that the wider the observation window, the more likely that home detention has a significant, positive effect on recidivism for first-time offenders. This conclusion is also evident when analysing a two-year recidivism rate (see Appendix Figure C2).

### 6.3.2 FRDD specification

As reported in Table 2, the primary FRDD treatment effect for a one-year recidivism rate is 5.9 percent. This LATE parameter was estimated by an unrestricted FRDD model that optimised the bandwidth and weighting kernel from the full period of Court Charges data, ranging from 1992

to 2017. Table 5 shows how this estimate changes when alternative observation windows and weighting functions are imposed on the FRDD model.

*Table 5. LATE estimates of home detention on a one-year recidivism rate by FRDD model*

	(1) <i>Uniform kernel</i>	(2) <i>Optimised weight</i>
Unrestricted observation window	- 0.001	0.059
365-day observation window	0.455	0.042

*Notes:* The optimised weighting function selected by the FRDD estimation in column (2) was a triangular kernel for both specifications. Asterisks indicate the significance level of the LATE estimate where \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .  
*Source:* Own calculations based on data in Statistics New Zealand's IDI.

When imposing a uniform kernel on the estimation, the treatment effect becomes negative but remains insignificantly different from zero. Limiting the FRDD optimal weighting function to a 365-day observation window produces a similar estimate to the unrestricted model (4.2 versus 5.9 percent). Yet, when both a uniform kernel and a 365-day observation window are enforced, the treatment effect is a massive 45 percent.<sup>34</sup> Nonetheless, Table 5 verifies that there is no indication of a systematic decrease in the one-year recidivism rate for offenders who served home detention relative to short-term imprisonment, community detention or intensive supervision sentences, irrespective of the FRDD specification.

### 6.3.3 Criteria of the offender sample

The population of interest comprises first-time offenders that were, or would have been, directly affected by the enactment of home detention as a stand-alone sentence. For reasons explained in Section 4, these are the offenders who were sentenced to short-term imprisonment, home detention, community detention or intensive supervision. To further test the sensitivity of the main result, 2SLS and FRDD estimates are computed with different definitions these marginal offenders, as shown in Table 6.

First, treatment effects for one- and two-year recidivism rates are analysed for first-time offenders who received imprisonment, home detention, community detention or intensive supervision sentences within 365 days of 1 October 2007. This broader specification includes prison sentences of all lengths, not just those considered short-term. The 2SLS and FRDD estimates (columns (1) and (3) of Table 6) both show that serving a home detention sentence increases the one-year recidivism rate by approximately 10.5 percent relative to the other three sentences. When inspecting a two-year recidivism rate, the 2SLS estimate in column (2)

<sup>34</sup> This could be due to a weak first stage such that the denominator of the LATE is almost zero, amplifying the causal estimate.

increases to 13.7 percent and the FRDD estimate in column (4) becomes 16.8 percent, both statistically significant. Furthermore, when long-term imprisoned offenders are included in the population of interest, the recidivism rates of offenders on home detention is significantly higher than offenders at the margin of the sentence.<sup>35</sup> This is likely because more offenders are incarcerated for the entire recidivism period, making the recidivism rates of home detention offenders appear relatively higher.

Second, treatment effects for one- and two-year recidivism rates are estimated by comparing only offenders who received short-term imprisonment or home detention sentences within 365 days of the policy cut-off. This narrowed specification assumes that substitution into home detention came solely from offenders who would have otherwise received a short-term imprisonment sentence; a direct interpretation of section 15B of the Sentencing Act 2002. The 2SLS estimate for the one-year recidivism rate in column (1) is 2.2 percent, while the two-year recidivism rate in column (2) is negative 1.4 percent. Neither is significantly different from zero. Both the one- and two-year recidivism FRDD estimates produce a positive LATE of approximately 1 percent. Table 6 demonstrates that even with a broader or narrower definition of the population of interest, there remains no statistically significant evidence that a home detention sentence reduces recidivism within one or two years of an offender’s sentencing date.

*Table 6. LATE estimates of home detention on recidivism by definition of offender sample*

	<i>2SLS</i>		<i>FRDD</i>	
	(1) <i>One-year recidivism</i>	(2) <i>Two-year recidivism</i>	(3) <i>One-year recidivism</i>	(4) <i>Two-year recidivism</i>
Imprisonment, home detention, community detention and intensive supervision sentences	0.109 *	0.137 *	0.104	0.168 **
Short-term imprisonment and home detention sentences	0.022	-0.014	0.012	0.0098

*Notes:* The 2SLS estimation in columns (1) and (2) imposed uniform weighting on observations within a 365-day observation window. The optimised weighting function selected by the FRDD estimations in columns (3) and (4) were triangular kernels. Asterisks indicate the significance level of the LATE estimate where \*\* if  $p < 0.01$  and \* if  $p < 0.05$ . *Source:* Own calculations based on data in Statistics New Zealand’s IDI.

<sup>35</sup> With the exception of the one-year recidivism rate estimated by the FRDD specification, which is positive but insignificant.

### 6.3.4 Definition of recidivism

As with all outcome variables, the predicted effectiveness of a home detention sentence depends on the researcher's definition of recidivism. To address this concern, I construct two alternative measures of recidivism. Panel A of Table 7 provides the LATE estimates of recidivism in discrete intervals following an offender's sentencing date and Panel B shows the LATE estimates of recidivism measured from an offender's release date. Both panels observe the population of interest within a 365-day observation window.

Across estimation methods, Panel A shows that recidivism increases during the second year (between 365 and 730 days) for offenders sentenced to home detention relative to short-term imprisonment, community detention and intensive supervision. Reoffending propensities decrease between the third and fifth years (730 to 1,825 days) of offenders' sentencing dates, perhaps a reflection of incapacitation effects given all short-term imprisoned offenders would have been released by this time. Although, these estimates are not significantly different from zero. There is also no evidence that a home detention sentence reduces the propensity to reoffend when recidivism is measured from offenders' release dates.<sup>36</sup> All three models in Panel B produce positive treatment effects for one- and two-year post-release recidivism rates for offenders sentenced to home detention relative to their offender counterparts.

Table 7. LATE estimates of home detention on recidivism by definition of recidivism

	(1) <i>Wald estimator</i>	(2) <i>2SLS</i>	(3) <i>FRDD</i>
<i>Panel A.</i>			
Between 365 and 730 days	0.024	0.014	0.044
Between 730 and 1,825 days	- 0.036	- 0.046	- 0.016
<i>Panel B.</i>			
365-day post-release recidivism	0.041	0.017	0.066
730-day post-release recidivism	0.056	0.040	0.083

Notes: The Wald estimator in column (1) and the 2SLS estimation in column (2) imposed uniform weighting on observations within the 365-day observation window. The FRDD estimations in column (3) optimised a triangular kernel weighting function for all specifications. Asterisks indicate the significance level of the LATE estimate where \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .

Source: Own calculations based on data in Statistics New Zealand's IDI.

### 6.3.5 Identification of initial sentence

When defining the population of interest in Section 4, first-time offenders are only observed for the most serious sentence received at their initial sentencing hearing. This is to avoid treatment

<sup>36</sup> As the Court Charges data does not contain records of an offender's release date, this variable is proxied by adding the length of an offender's sentence to their sentencing date, under the assumption that all sentences are served in full.

plurality in situations where an offender received more than one sentence at a given point in time. An offender’s most serious sentence is identified with the *sentence ranking* variable in the Ministry of Justice’s Court Charges data. To test whether the composition of the offender sample is sensitive to the categorisation rules set by the *sentence ranking* variable, sentence assignment is compared using the Ministry of Justice’s *charge ranking* variable. This variable ranks all charges faced by an individual in a given calendar year. A range of information is used to determine the seriousness of a charge, including the charge outcome, sentence type, sentence length or amount, remands in custody, bail eligibility and maximum offence penalties (Statistics New Zealand, 2018). However, the algorithm that weights this information is unknown.<sup>37</sup>

Identifying a single sentence for each offender requires two steps when using the *charge ranking* variable. First, I observe the *charge ranking* variable for the calendar year that the offender was first sentenced. Second, I identify the most serious charge faced by the offender at the date of their first sentencing. The sentence that corresponds to the most serious charge becomes the sentence that the offender is observed serving. The resulting categorisation is almost identical. Table 8 shows the frequencies of all sentences received by first-time offenders between 1992 and 2017. Columns (1) and (2) provide the sentence categorisation using the *sentence ranking* variable while columns (3) and (4) show the equivalent using the *charge ranking* variable.

Table 8. The categorisation of offenders by ranking variable

	<i>Sentence ranking variable</i>		<i>Charge ranking variable</i>	
	(1) <i>Frequency</i>	(2) <i>Percent</i>	(3) <i>Frequency</i>	(4) <i>Percent</i>
Imprisonment	23,784	2.96	23,613	2.94
Home detention and other custodial	5,838	0.73	5,838	0.73
Community detention	4,476	0.57	4,476	0.56
Intensive supervision	1,560	0.19	1,557	0.19
Community work	131,970	16.4	13,1976	16.4
Supervision	23,541	2.93	23,610	2.94
Monetary	551,634	68.7	551,586	68.6
Deferment	30,009	3.73	29,871	3.72
Other	22,512	2.80	22,794	2.84
No sentence recorded	8,154	1.01	81,60	1.02
Total	803,478	100	803,481	100

Source: Own calculations based on data in Statistics New Zealand’s IDI.

<sup>37</sup> This ambiguity is the reason why the *sentence ranking* variable is my preferred sentence-identification method for this thesis.

Results in Table 8 confirm that ranking rules do not affect the categorisation of offenders into their observed sentence. The percentage of convicted offenders in each of the top four sentences is the same across the ranking variables. Approximately 2.95 percent of convicted offenders received imprisonment sentences at their first sentencing date, while 0.73 percent received home detention and other custodial sentences. The percentage of offenders who received community detention (intensive supervision) sentences is approximately 0.57 (0.19) percent under each ranking variable. Thus, as the composition of the first-time offender population is identical across the *sentence ranking* and *charge ranking* variables, the population of interest will also be identical, and so too will the LATE estimates. Furthermore, Table 8 verifies that there is no statistically significant evidence that a home detention sentence reduces recidivism, even when an offender's sentence assignment is determined by their most serious charge.

#### 6.3.6 Reconviction offence type

If an offender breaches their home detention conditions, their non-compliance could result in a further conviction (Department of Corrections, n.d.). A formal breach action is classified as an 'offence against justice', which forms part of Category 5 offences. If offenders serving home detention were reconvicted for breaching their conditions, this could explain why the one-year recidivism LATE estimate was not negative or statistically significant for offenders on home detention. To test this theory, reoffence categories are compared across the population of interest within a 365-day observation window for a one-year recidivism rate. Panel A of Figure 5 provides the types of reoffences committed by the control group, and Panel B of Figure 5 illustrates the equivalent for the treatment group.<sup>38</sup>

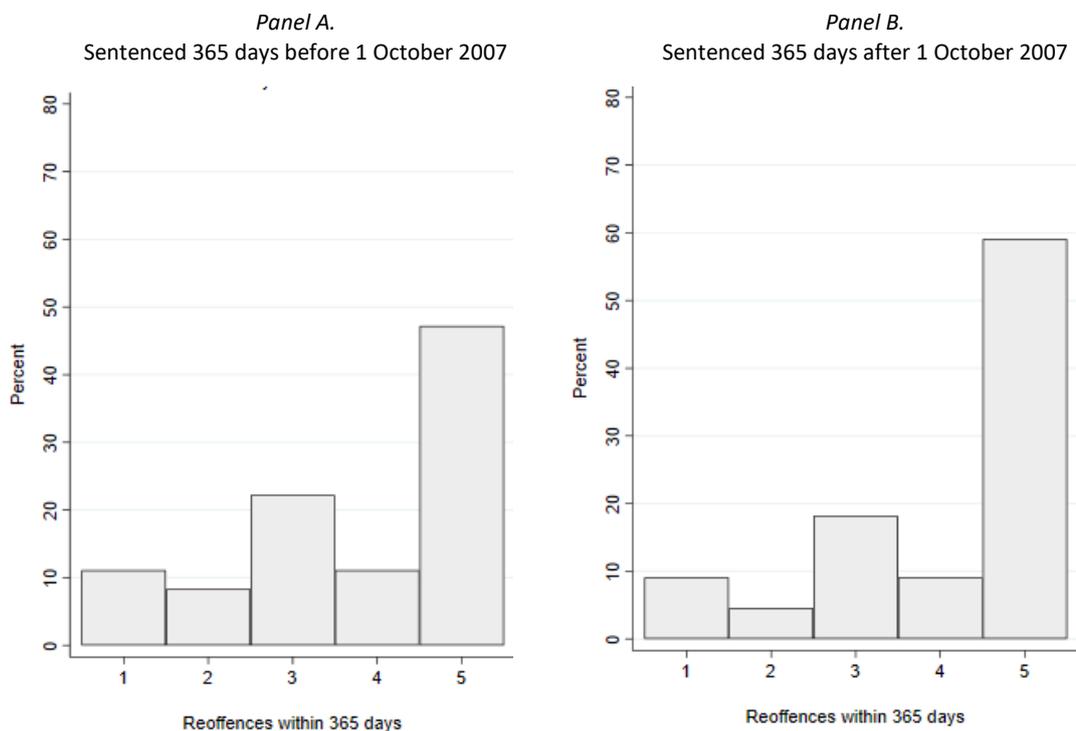
Figure 5 shows that reconviction commonly resulted from Category 5 offences for both the treatment and control groups. To formally test the difference in Category 5 reoffences by treatment status, I conduct mean-difference t-tests. The difference between the treatment and control groups is insignificant for one-year and five-year recidivism rates. However, for a two-year recidivism rate, the number of Category 5 reoffences is significantly higher for offenders in the treatment group ( $p = 0.03$ ). To address the possibility that this significant difference in reoffence categories biased the LATE estimations in Table 2, recidivism is re-defined to include only reconvictions that resulted from Category 1, 2, 3 or 4 offences. 2SLS and FRDD models are estimated to test whether a home detention sentence reduced the probability of being reconvicted of crimes other than offences against justice. Results are shown in Table 9. Both

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<sup>38</sup> See Appendix Figures C3 and C4 for the reoffence categories committed by the treatment and control groups in a two- and five-year recidivism period, respectively.

specifications produce negative but statistically insignificant LATE estimates across all three recidivism periods. This verifies that a home detention sentence has no significant impact on recidivism, irrespective of the reconvictions that resulted from breaches of home detention conditions.

Figure 5. Category of reoffence by sentencing date for a one-year recidivism period



Notes: Offences are categorised as follows: (1) homicide, injury-causing and sexual offences; (2) dangerous acts, abduction and harassment offences; (3) robbery, extortion, burglary and theft; (4) fraud, deception, drugs and weapon crimes; and (5) property offences, public order offences, traffic offences and offences against justice. For confidentiality reasons, Category 6 offences (miscellaneous offences and offences with inadequate information) are suppressed in this analysis.

Source: Own calculations based on data in Statistics New Zealand's IDI.

Table 9. LATE estimates of home detention on recidivism for reoffences excluding breaches of home detention conditions

	(1) 2SLS	(2) FRDD
365-day recidivism	-0.011	-0.025
730-day recidivism	-0.037	-0.034
1,825-day recidivism	-0.025	-0.016

Notes: The 2SLS estimation in column (1) imposed uniform weighting on observations within the 365-day observation window. The FRDD estimations in column (2) optimised a triangular kernel weighting function for all specifications. Asterisks indicate the significance level of the LATE estimate where \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .

Source: Own calculations based on data in Statistics New Zealand's IDI.

### 6.3.7 Placebo reform dates

A final robustness test examines whether the LATE estimates are driven by general macroeconomic trends. Placebo tests are used to identify whether the treatment effects vary over time, biasing the primary estimates (Hausman & Rapson, 2017). In the present context, placebo tests identify whether one-year recidivism rates are discontinuous across points in time other than the 1 October 2007 sentencing reform. By definition, there should be no significant discontinuity in home detention receipt across cut-off dates prior to the actual reform, making the first stage equal to zero. Thus, a placebo test recovers an intention-to-treat (ITT) parameter rather than a LATE. The expectation is that the ITT estimates are zero in the absence of time-varying outcomes. To test this hypothesis and validate my RDD empirical design, I observe the population of interest in a 365-day observation window around the placebo reforms dates of 1 October 2006, 2005 and 2004. For each placebo date, the treatment group are the offenders sentenced within 365 days after the reform and the control group are those sentenced within 365 days prior. The first stage, reduced form and ITT estimates for each placebo reform are presented in Table 10.

*Table 10. ITT estimates of a one-year recidivism rate by placebo reform date*

	(1) <i>First stage</i>	(2) <i>Reduced form</i>	(3) <i>ITT</i>
1 October 2006	0.000**	-0.002	0.000 **
1 October 2005	0.000**	0.012	0.000 **
1 October 2004	0.000**	0.045	0.000 **

*Notes:* The first stage estimations in column (1) and ITT estimations in column (3) imposed uniform weighting on observations within the 365-day observation window. The ITT estimates are recovered from an OLS regression. The reduced form estimates in column (2) are from a sharp RDD estimation that optimised a triangular kernel weighting function. 95 percent confidence intervals are in parentheses. Asterisks indicate the significance level of the estimates where \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .

*Source:* Own calculations based on data in Statistics New Zealand's IDI.

As expected, column (1) shows the zero first stage effect for each placebo reform, all of which are statistically significant at the 1 percent level. The key observations are in column (3): the ITT estimates are all zero and statistically significant at the 1 percent level. Column (3) implies that, prior to the 2007 home detention reform, the reoffending tendency of the population of interest was continuous across time.<sup>39</sup> This implies any differences in recidivism rates between the treatment and control groups across 1 October 2007, adjusting for the discontinuity in treatment receipt, can be attributed to the home detention reform itself. Therefore, Table 10

<sup>39</sup> This result is supported by the insignificant reduced form estimates in column (2). The reduced form and ITT estimates differ because the sharp RDD estimation optimises a triangular kernel while the OLS regression imposes uniform weighting. Note that a sharp RDD is preferred over an FRDD estimation for placebo tests due to the absence of a first stage.

verifies that the primary LATE estimates in Table 2 represent the causal effect of home detention on recidivism for the offender population of interest.

#### 6.3.8 Constant release environments

In this subsection, I address the issue of constant release environments and identify the differences in future sentencing expectations between the treatment and control groups. Results provide direction for future research in this regard.<sup>40</sup>

In my thesis, the treatment and control groups received their first sentence in different sentencing environments. The control group were sentenced in an environment where their expected sentence was short-term imprisonment, community detention or intensive supervision; the treatment group were sentenced in an environment where home detention was an additional sentencing option. This initial difference in sentencing environments is the foundation of my research question: *Did the 2007 home detention sentencing reform reduce the recidivism rate of first-time offenders?* To answer this question, it is important that the treatment and control groups have constant sentencing environments in the observed recidivism period. This requires all offenders in the control group to be released into the home detention environment, i.e., after 1 October 2007. This is because the choice to recommit a crime (post-release) must only be affected by the sentence the offender initially served, not due to differences in the expectation of future sentences. Thus, to recover the causal effect of home detention on recidivism, reoffending rates of the treatment and control groups should be compared in an environment where home detention is anticipated for all offenders.

To test the possible extent of this issue, I observe the control group (defined by a 365-day observation window) for 365 days post-release and take the weighted average of the number of days spent in the home detention environment. Results suggest that 89.3 percent of the 365-day post-release period was spent in the new environment. The weighted average is larger when observing the control group in narrower windows: for both the 50-day and 100-day observation windows, approximately 99 percent of the 365-day post-release period was after 1 October 2007. Furthermore, while almost all the control group were released into the home detention environment for the smaller observation windows, future research could analyse the extent to

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<sup>40</sup> I discuss the differences in sentencing environments with respect to recidivism measured from the date of release. However, given Table 7 showed that the LATE estimates do not differ across definitions of recidivism, this extension could equally be applied to recidivism periods measured from the date of sentencing.

which the differences in release environments possibly biased the treatment effect estimation for the control group observed within the 365-day window.

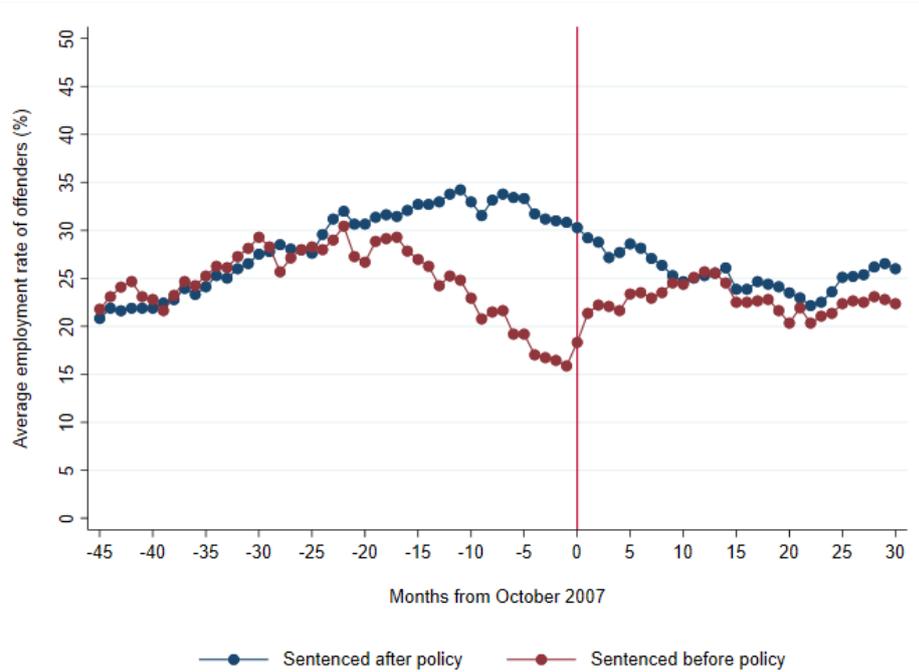
#### 6.4 Labour market attachment: A difference-in-differences analysis

In this section, I present the effect of a home detention sentence on offenders' labour market attachment. The empirical strategy is explained in the context of average employment rates; however, results are also provided for average earnings and benefit receipt. Graphical outputs for these latter labour market indicators can be found in Appendix Figures A5 and A6.

The average monthly employment rates for the population of interest within a 365-day observation window are shown in Figure 6. These trends are measured across a *calendar* timeline where October 2007 is month zero. Approximately two years prior to the reform, the average monthly employment rates of the treatment and control groups followed similar trajectories. In the year preceding the reform, there is a clear disparity in the employment rates of the two groups. Naturally, this is explained by the fact that the control group would have started their short-imprisonment, community detention or intensive supervision sentences while the treatment group are still sentence-free. This inconsistency across sentencing timelines inevitably causes differences in monthly employment rates. A comparison of the average monthly employment trends of the treatment and control groups over calendar time is thus not informative of the causal effect of home detention on employment.

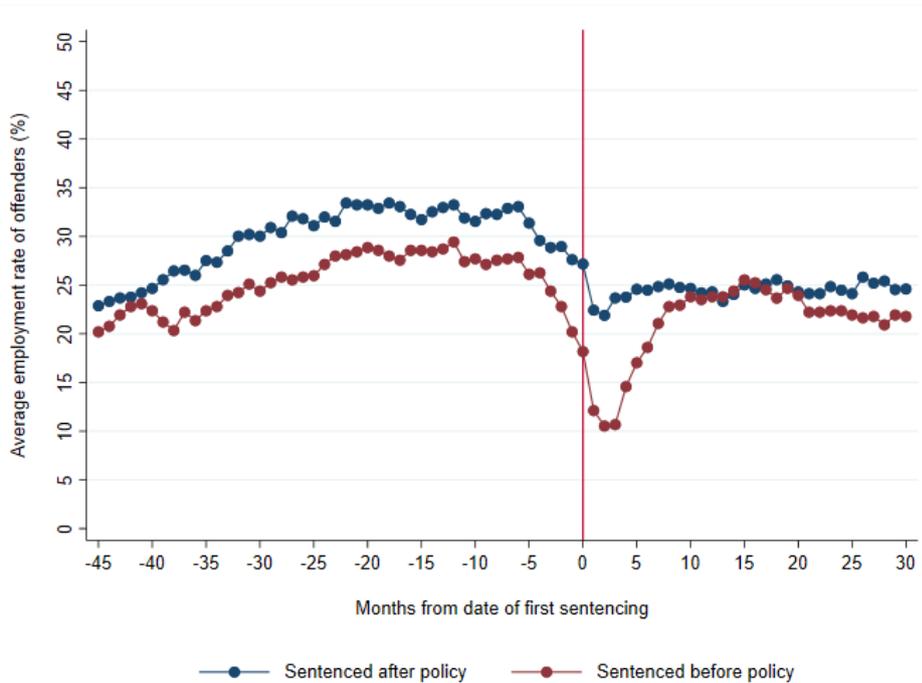
Instead, employment rates should be observed on an *event* timeline where offenders' sentencing dates are normalised to month zero, as shown in Figure 7. In the months approaching offenders' sentencing dates, the treatment group have a higher employment rate than the control group. This gap widens during the first three months post-sentencing but converges after one year. While the event timeline can identify distinct differences in the labour market attachment of the treatment and control groups, it also observes the two groups at different points in the calendar year. Specifically, the average sentencing date for the control group is April 2007, whereas the average sentencing date is April 2008 for the treatment group. This time difference could affect the employment opportunities for each group, especially since 2007/2008 was the onset of the global financial crisis. To mitigate this issue, the event timeline can be corrected for calendar time fixed effects. This can be achieved with the following difference-in-differences (DD) regression:

Figure 6. Average monthly employment rate of offender sample by calendar time



Notes: Employment is a binary indicator equal to one if the offender had positive earnings from wages and salaries in a given month. Month zero, shown by the red line, is October 2007.  
 Source: Own calculations based on data in Statistics New Zealand's IDI.

Figure 7. Average monthly employment rate of offender sample by event time



Notes: Employment is a binary indicator equal to one if the offender had positive earnings from wages and salaries in a given month. Offenders' sentencing dates are normalised to month zero, shown by the red line.  
 Source: Own calculations based on data in Statistics New Zealand's IDI.

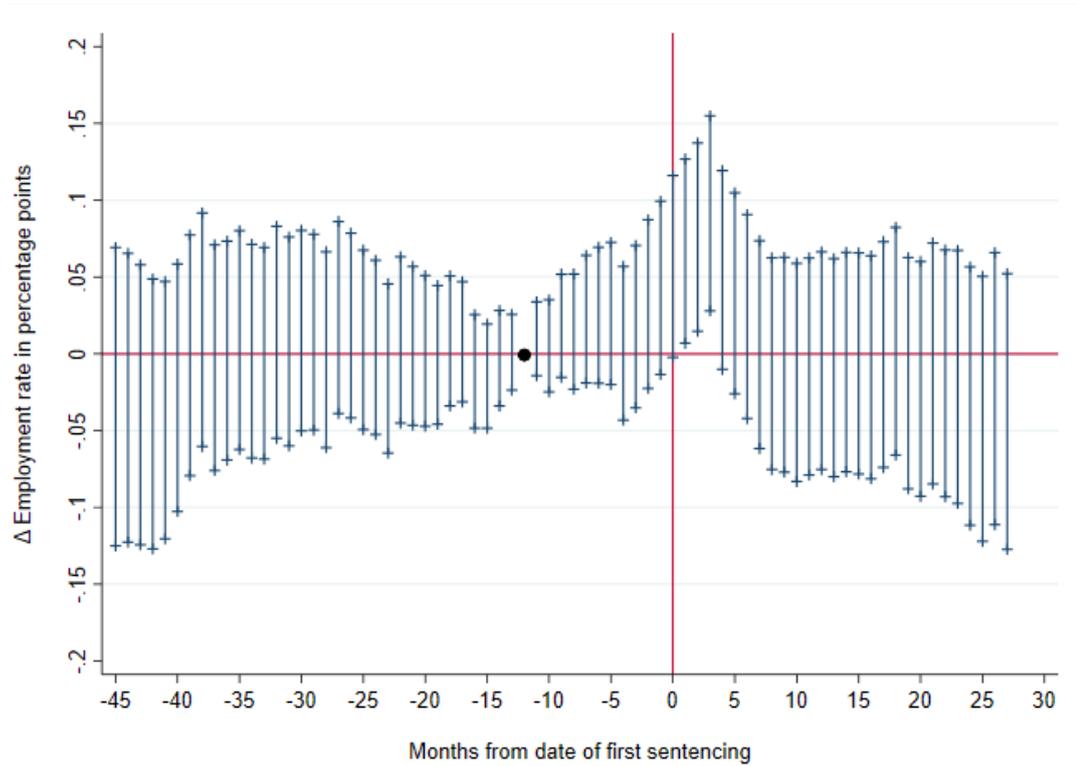
(7)

$$Y_{iet} = \alpha + \beta TR_i + \sum_{\substack{j=-45 \\ j \neq -12}}^{27} \delta_j \cdot 1[j = e] + \sum_{\substack{j=-45 \\ j \neq -12}}^{27} \gamma_j \cdot (1[j = e] \cdot TR_i) + \sum_y \tau_y \cdot 1[y = t] + u_{iet}$$

where  $Y_{iet}$  is a binary indicator that equals one if individual  $i$  is employed in event time  $e$  and calendar month  $t$ , and zero otherwise;  $\alpha$  is a constant;  $TR_i$  is a binary indicator that equals one if individual  $i$  is sentenced after 1 October 2007 and zero otherwise; the third term is a full set of event time dummies ranging from 45 months prior to sentencing, to 27 months after; the fourth component is an interacted term of the former two components; the fifth term is a full set of calendar month indicators; and  $u_{iet}$  is the error term. Month -12 of the event timeline is the base category such that the coefficient of the interacted variable ( $\gamma_j$ ) is interpreted relative to the employment rate one year prior to sentencing. The inclusion of the calendar time dummies allows  $\gamma_j$  to recover the causal effect of the home detention reform on average employment rates controlling for general labour market trends. In this DD framework,  $\gamma_j$  identifies a causal effect if there are no other treatment-specific shocks to labour market attachment and if the common trends assumption holds. That is, in the absence of the sentencing reform and conditional on calendar month fixed effects, the difference in average employment rates between the treatment and control groups would have followed the same trajectory over time. The 95 percent confidence intervals for the  $\gamma_j$  estimates over an event timeline are shown in Figure 8.

Each confidence interval indicates the percentage point change in average employment between the treatment and control groups relative to event month -12. Figure 8 provides three noteworthy observations. First, leading up to offenders' sentencing dates ( $e < 0$ ), the change in employment levels between the two groups is not significantly different from zero. This pattern lends strong support to the common trends assumption. Second, in the first three months after being sentenced ( $0 < e < 3$ ), the treatment group experiences a significantly higher employment rate relative to the control group. Third, from four months post-sentencing ( $4 < e < 27$ ), all 95 percent confidence intervals contain zero. While offenders on home detention are allowed to seek or retain employment, this is only reflected in the first three months of their sentence; perhaps by virtue of incapacitation effects. Figure 8 therefore provides no evidence that offenders on home detention have a significantly higher employment rate than offenders sentenced to short-term imprisonment, community detention or intensive supervision, for the duration of their sentence or the months after.

Figure 8. The average effect of the home detention reform on monthly employment rates



Notes: Offenders' sentencing dates are normalised to month zero. This figure shows the effect of the home detention reform on average monthly employment rates, measured by the percentage point change in the employment rate between the treatment and control groups relative to month -12, shown by the black marker. The vertical lines represent the 95 percent confidence interval of each estimate, as calculated by Equation 7.

Source: Own calculations based on data in Statistics New Zealand's IDI.

This result is reinforced when looking at the average monthly earnings from wages and salaries and average benefit receipt across the treatment and control groups.<sup>41</sup> The DD regression results depicted in Appendix Figure C5 show no statistically significant difference in the average monthly wages and salaries earnings between the treatment and control groups from two years pre-sentencing to three years post-sentencing.<sup>42</sup> This inference is also apparent for the average benefit uptake of the two groups (Appendix Figure C6).<sup>43</sup> Moreover, analysis of mean monthly wages and salaries and benefit receipt reiterate the conclusion that a home detention sentence has no significant impact on offenders' labour market participation relative to short-term imprisonment, community detention or intensive supervision sentences.

<sup>41</sup> The calendar timeline and event timeline graphs (like that of Figures 6 and 7) for the wages and salaries and benefit receipt labour market outcomes are available upon request.

<sup>42</sup> With the exception of the third month post-sentencing.

<sup>43</sup> The only significant difference in average benefit receipt between the treatment and control groups, relative to event month -12, is during the second month post-sentencing.

## Section 7 Conclusion

I estimated the local average treatment effect of serving a home detention sentence for first-time offenders sentenced around 1 October 2007 in New Zealand. Utilising population-wide linked administrative data from Statistics New Zealand, I identified a credible comparison group by observing offenders within a narrow time window either side of the policy cut-off. In contrast to the report by the Ministry of Justice (2011), I found no statistically significant evidence that a home detention sentence reduces recidivism compared to short-term imprisonment, community detention or intensive supervision sentences. This conclusion is robust to all FRDD and 2SLS specifications and is not sensitive to changes in the offender sample, recidivism period or definition, sentence categorisation rules or reoffence type. Placebo tests further validated my empirical design. While different observation windows presented econometric trade-offs for the population of interest, the window choice was trivial for the overall result since all specifications failed to identify any significant effect of home detention on recidivism.

In addition, analysis of offenders' labour market participation suggests the theorised employment benefits of home detention are not realised in practice. First-time offenders who serve home detention sentences cannot be said to gain better labour market positions than offenders at the margin of home detention eligibility. Once controlling, non-parametrically, for calendar time labour market trends, DD regression estimates showed no statistically significant differences in the average monthly employment rates, wages and salaries earnings, and benefit receipt across the treatment and control groups. This trend is evident before sentencing, throughout the sentencing period and post-release.

Overall, I do not question the fiscal cost-savings of a home detention sentence relative to short-term imprisonment. Given the enactment of home detention was motivated by the need for more cost-effective correction methods and prison overpopulation issues (Criminal Justice Reform Bill 2006; Gibbs & King, 2003), perhaps it is serving its purpose. However, evidence from my thesis provides little justification for promoting home detention as a criminal justice sanction that advances offenders' reintegration abilities.

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## Appendices

### Appendix A. Ministry of Justice's 2011 Report

The Ministry of Justice (2011) quantified the observed effect of a home detention sentence by comparing the average recidivism rates of offenders sentenced to home detention versus short-term imprisonment. This comparison can be expressed in Rubin's (1974) counterfactual framework notation:

$$(A1) \quad NE = E(Y_i(1)|D_i = 1) - E(Y_i(0)|D_i = 0)$$

where the first component represents the average recidivism rate of offenders who served home detention ( $Y_i(1)$ ), given they were sentenced to home detention ( $D_i = 1$ ). The second component is the average recidivism rate of offenders who served short-term imprisonment ( $Y_i(0)$ ), given they were sentenced to short-term imprisonment ( $D_i = 0$ ). Taking the differences between these two produces the Naïve Estimator (NE).

The NE estimate can only be interpreted as the average treatment effect for offenders sentenced to home detention (i.e., the ATT) under the assumption of no selection bias. That is, offenders sentenced to home detention and offenders sentenced to short-term imprisonment are exactly alike across all observable and unobservable dimensions. If this were the case, the average recidivism rate of offenders sentenced to short-term imprisonment would appropriately represent the recidivism rate of offenders sentenced to home detention had they not served home detention. This equalisation is framed by Equation A2 such that the NE recovers an unbiased ATT in Equation A3.

$$(A2) \quad E(Y_i(0)|D_i = 0) = E(Y_i(0)|D_i = 1)$$

$$(A3) \quad \begin{aligned} ATT &= E(Y_i(1)|D_i = 1) - E(Y_i(0)|D_i = 1) \\ &= E(Y_i(1)|D_i = 1) - E(Y_i(0)|D_i = 0) \\ &= NE \end{aligned}$$

The assumption imposed by Equation A2 infers that judges assign sentences randomly without considering the case-specific characteristics of the offender or the offence. Equation A2 proposes that the only difference between offenders sentenced to home detention and offenders sentenced to short-term imprisonment is their sentence assignment. To test this assumption, offenders sentenced between 1 October 2007 and 1 October 2008 are compared across a range of characteristics. Mean-difference t-tests are presented in Table A1. As shown

by the t-statistic column, there are significant differences in the composition of offenders in short-term imprisonment and home detention sentences across all four categories of covariates. Such differences are also evident when comparing the two groups between 1 October 2008 and 1 October 2008 (Table A2). Based on these results, it is unreasonable to assume that the observed outcomes of offenders sentenced to short-term imprisonment are representative of the counterfactual outcomes of offenders sentenced to home detention. Moreover, I contribute to the Ministry of Justice's report by identifying a credible comparison group for recovering the causal effect of home detention on recidivism in New Zealand, but for first-time offenders.

Table A1. Covariate balance for offenders sentenced to short-term imprisonment versus home detention between 1 October 2007 and 1 October 2008

	<i>Short-term imprisonment</i>		<i>Home detention</i>		<i>t-statistic</i>
	<i>N</i>	<i>Mean (SD)</i>	<i>N</i>	<i>Mean (SD)</i>	
<i>Demographic</i>					
Male	363	0.86 (0.35)	426	0.62 (0.49)	7.59 **
Age	357	29.8 (12.5)	420	30.5 (12.7)	-0.81
Religious	81	0.49 (0.50)	198	0.51 (0.50)	-0.28
Born in NZ	90	0.84 (0.37)	219	0.83 (0.38)	0.34
European	363	0.63 (0.48)	429	0.53 (0.50)	2.89 **
Māori	363	0.19 (0.40)	429	0.30 (0.46)	-3.34 **
Pacific Peoples	363	0.10 (0.30)	429	0.10 (0.30)	-0.14
Asian	363	0.04 (0.21)	429	0.05 (0.22)	-0.31
MEELA	363	0.03 (0.16)	429	0.02 (0.14)	0.84
Auckland City	363	0.13 (0.33)	429	0.09 (0.28)	1.84
Christchurch City	363	0.09 (0.29)	429	0.09 (0.29)	-0.12
Wellington City	363	0.03 (0.16)	429	0.05 (0.21)	-1.41
<i>Crime</i>					
Category 1	363	0.32 (0.47)	429	0.25 (0.44)	2.18 *
Category 2	363	0.04 (0.19)	429	0.04 (0.21)	-0.41
Category 3	363	0.20 (0.40)	429	0.15 (0.36)	1.62
Category 4	363	0.23 (0.42)	429	0.42 (0.49)	-5.82 **
Category 5	363	0.21 (0.41)	429	0.10 (0.30)	4.41 **
Category 6	363	0.00 (0.05)	429	0.03 (0.17)	-2.95 **
<i>Education</i>					
NCEA 1	96	0.14 (0.35)	171	0.24 (0.43)	-1.98 *
NCEA 2	96	0.07 (0.26)	171	0.09 (0.28)	-0.38
NCEA 3	96	0.26 (0.44)	171	0.16 (0.37)	2.11 *
Low decile	78	0.42 (0.50)	165	0.45 (0.50)	-0.49
Medium decile	78	0.44 (0.50)	165	0.40 (0.49)	0.69
High decile	78	0.14 (0.35)	165	0.15 (0.36)	-0.27
<i>Earnings</i>					
Gross 2007 WS	363	40,589 (199,865)	429	74,095 (205,410)	-2.31 *
Gross 2008 WS	363	20,180 (82,420)	429	83,688 (454,981)	-2.62 **

Notes: Offences are categorised as follows: (1) homicide, injury-causing and sexual offences; (2) dangerous acts, abduction and harassment offences; (3) robbery, extortion, burglary and theft; (4) fraud, deception, drugs and weapon crimes; (5) property offences, public order offences, traffic offences and offences against justice; and (6) miscellaneous offences and offences with inadequate information. Each NCEA level represents the highest attained secondary school qualification. School deciles are from the last attended secondary school, ranked as follows: 1-3 is low, 4-7 is medium and 8-10 is high. WS are earnings from wages and salaries, measured in NZD. Standard deviations are in parentheses. Equality of means are tested by the t-statistic where statistically significant differences are indicated with \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .

Source: Own calculations based on data in Statistics New Zealand's IDI.

Table A2. Covariate balance for offenders sentenced to short-term imprisonment versus home detention between 1 October 2008 and 1 October 2009

	<i>Short-term imprisonment</i>		<i>Home detention</i>		<i>t-statistic</i>
	<i>N</i>	<i>Mean (SD)</i>	<i>N</i>	<i>Mean (SD)</i>	
<i>Demographic</i>					
Male	369	0.86 (0.35)	468	0.6 (0.5)	8.85 **
Age	366	30.2 (12.7)	462	32.2 (13.9)	-2.05 *
Religious	102	0.47 (0.50)	228	0.52 (0.50)	-0.86
Born in NZ	117	0.84 (0.37)	246	0.79 (0.41)	1.01
European	372	0.61 (0.49)	468	0.51 (0.50)	2.87 **
Māori	372	0.24 (0.43)	468	0.29 (0.45)	-1.62
Pacific Peoples	372	0.08 (0.28)	468	0.14 (0.34)	-2.44 *
Asian	372	0.04 (0.20)	468	0.04 (0.20)	-0.02
MEELA	372	0.02 (0.15)	468	0.02 (0.14)	0.49
Auckland City	372	0.10 (0.30)	468	0.08 (0.28)	0.69
Christchurch City	372	0.10 (0.30)	468	0.11 (0.31)	-0.57
Wellington City	372	0.04 (0.19)	468	0.03 (0.16)	0.81
<i>Crime</i>					
Category 1	372	0.36 (0.48)	468	0.21 (0.41)	4.70 **
Category 2	372	0.07 (0.25)	468	0.06 (0.24)	0.44
Category 3	372	0.17 (0.37)	468	0.18 (0.38)	-0.47
Category 4	372	0.22 (0.42)	468	0.44 (0.50)	-6.66 **
Category 5	372	0.17 (0.38)	468	0.08 (0.27)	4.05 **
Category 6	372	0.01 (0.12)	468	0.03 (0.17)	-1.59
<i>Education</i>					
NCEA 1	120	0.14 (0.35)	174	0.18 (0.38)	-0.84
NCEA 2	120	0.13 (0.34)	174	0.09 (0.29)	1.11
NCEA 3	120	0.19 (0.39)	174	0.21 (0.41)	-0.33
Low decile	102	0.47 (0.50)	165	0.44 (0.50)	0.40
Medium decile	102	0.42 (0.50)	165	0.41 (0.49)	0.11
High decile	102	0.11 (0.31)	165	0.14 (0.35)	-0.77
<i>Earnings</i>					
Gross 2007 WS	372	87,454 (608,989)	468	63,776 (169,594)	0.80
Gross 2008 WS	372	69,066 (381,539)	468	67,969 (200,445)	0.05

Notes: Offences are categorised as follows: (1) homicide, injury-causing and sexual offences; (2) dangerous acts, abduction and harassment offences; (3) robbery, extortion, burglary and theft; (4) fraud, deception, drugs and weapon crimes; (5) property offences, public order offences, traffic offences and offences against justice; and (6) miscellaneous offences and offences with inadequate information. Each NCEA level represents the highest attained secondary school qualification. School deciles are from the last attended secondary school, ranked as follows: 1-3 is low, 4-7 is medium and 8-10 is high. WS are earnings from wages and salaries, measured in NZD. Standard deviations are in parentheses. Equality of means are tested by the t-statistic where statistically significant differences are indicated with \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .

Source: Own calculations based on data in Statistics New Zealand's IDI.

## Appendix B. Instrumental Variables Estimation

To preface why an instrumental variable is required for estimating the causal effect of home detention, Equation B1 shows a simple regression of recidivism ( $Y_i$ ) on home detention receipt ( $D_i$ ). The parameter of interest ( $\mu$ ) cannot have causal interpretation because  $D_i$  is endogenous in the absence of random treatment assignment. In other words, delivered treatment is determined in part by unobserved characteristics that are likely related to the outcome (Angrist, 2005). This means the treatment indicator and the error term ( $u_i$ ) are correlated, as formalised in Equation B2.

$$(B1) \quad Y_i = \delta_0 + \mu D_i + u_i$$

$$(B2) \quad \text{corr}(D_i, u_i) \neq 0$$

An instrumental variable ( $Z_i$ ) can introduce randomisation to the causal effect estimation, making treatment assignment exogenous. A valid instrument must satisfy two conditions: (a) relevance and (b) exogeneity (Stock & Watson, 2012). The relevance condition requires that the instrument can predict variation in the endogenous regressor ( $D_i$ ), such that  $Z_i$  and  $D_i$  are correlated (Equation B3). Exogeneity requires the instrument to be unrelated to the error term (Equation B4). When both conditions are satisfied, then the variation in  $D_i$  that is captured by  $Z_i$  becomes exogenous in the treatment estimation, so the causal effect of treatment can be estimated using a two-staged least squares (2SLS) regression (Angrist & Pischke, 2009).

$$(B3) \quad \text{corr}(Z_i, D_i) \neq 0$$

$$(B4) \quad \text{corr}(Z_i, u_i) = 0$$

To recover the LATE of home detention using an instrumental variables 2SLS estimation, an offender's sentencing date becomes an instrument for home detention receipt in a sample of observations around the cut-off. Much like the Wald estimator (Equation 4), a 2SLS estimation considers two relationships. First, the relationship between the instrument and treatment status. In Equation B5,  $D_i$  is a binary indicator for home detention receipt;  $\alpha_0$  is a constant;  $R_i$  is the sentencing date running variable;  $Z_i$  is the instrument that equals one if the individual's sentencing date is after 1 October 2007 ( $Z_i \geq Z_0$ ) and zero if before ( $Z_i < Z_0$ ); and  $v_i$  is the error term. The second relationship is that between the treatment indicator and the outcome, known as the 'structural' equation. In Equation B6,  $Y_i$  is the probability that an individual reoffends

within 365 days of starting their sentence;  $\beta_0$  is a constant;  $\widehat{D}_i$  are the fitted values from Equation B5; and  $e_i$  is the error term.

$$(B5) \quad \widehat{D}_i = \alpha_0 + \alpha_1 R_i + \alpha_2 Z_i + v_i$$

$$(B6) \quad Y_i = \beta_0 + \lambda \widehat{D}_i + e_i$$

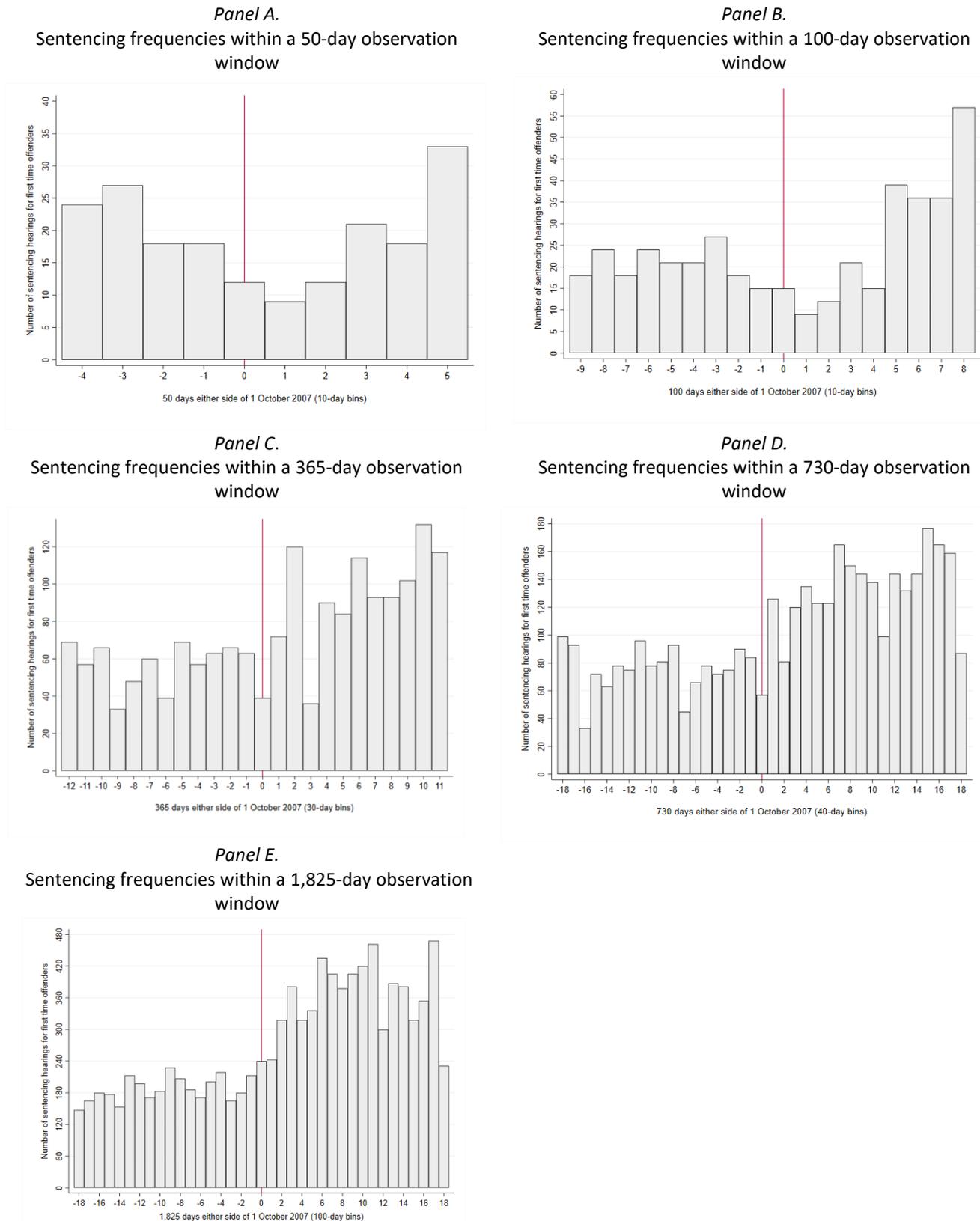
When the fitted values ( $\widehat{D}_i$ ) from the first stage (Equation B5) take the place of treatment status in the structural equation (Equation B6), random assignment is introduced via the sentencing date instrument. Specifically, the inability of offenders to precisely control their sentencing date around the 1 October 2007 policy cut-off ( $Z_0$ ) creates local randomised variation in treatment receipt (Lee & Lemieux, 2010).<sup>44</sup>  $\widehat{D}_i$  absorbs this randomisation such that treatment status is no longer an endogenous regressor in Equation B6. This two-step process creates a causal chain where the instrument ( $Z$ ) affects treatment receipt ( $D$ ), which in turn affects the outcome ( $Y$ ) (Angrist & Pischke, 2009). The parameter  $\lambda$  in Equation B6 is, therefore, the LATE of home detention on recidivism.

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<sup>44</sup> As explained by Lee and Lemieux (2010), the behavioural assumption that individuals cannot precisely manipulate the running variable across the cut-off has “the prediction that treatment is locally randomised” (p. 295).

## Appendix C. Figures and Tables

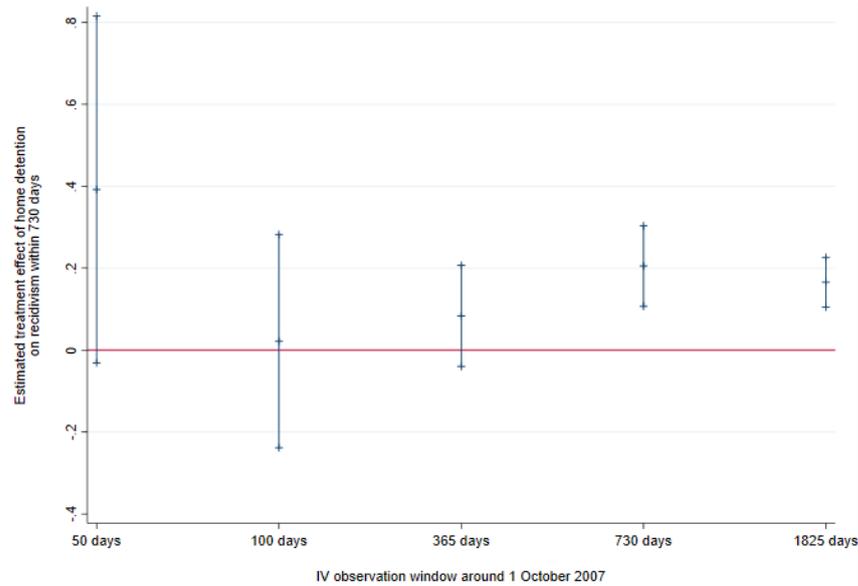
Figure C1. Density of sentencing hearings for population of interest around 1 October 2007



Notes: McCrary test p-values from Panel A to E are 0.852, 0.267, 0.002, 0.013 and 0.409 respectively.

Source: Own calculation based on data in Statistics New Zealand's IDI.

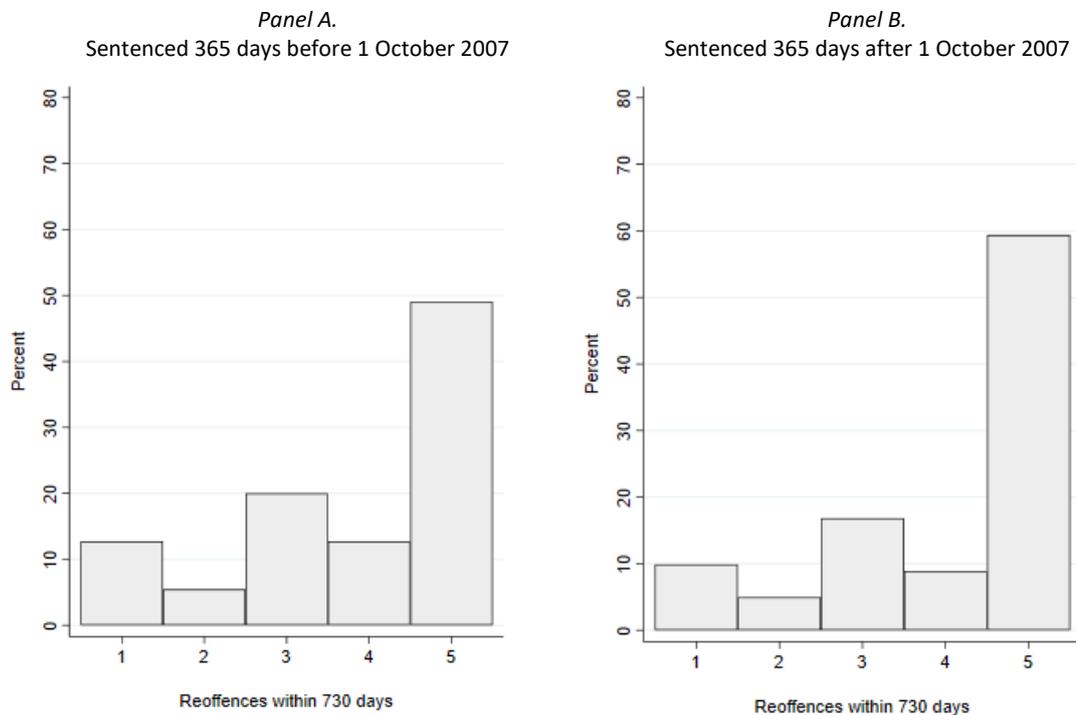
Figure C2. Instrumental variables LATE estimates of home detention on a two-year recidivism rate



Notes: The upper and lower limits represent the 95 percent confidence interval for the estimated LATE within each observation window. The LATE estimates for the 50-day, 100-day and 365-day windows are insignificant ( $p > 0.05$ ). The LATE estimates for the 730-day and 1,825-day windows are statistically significant at the 1 percent level ( $p = 0.00$ ).

Source: Own calculations based on data in Statistics New Zealand's IDI.

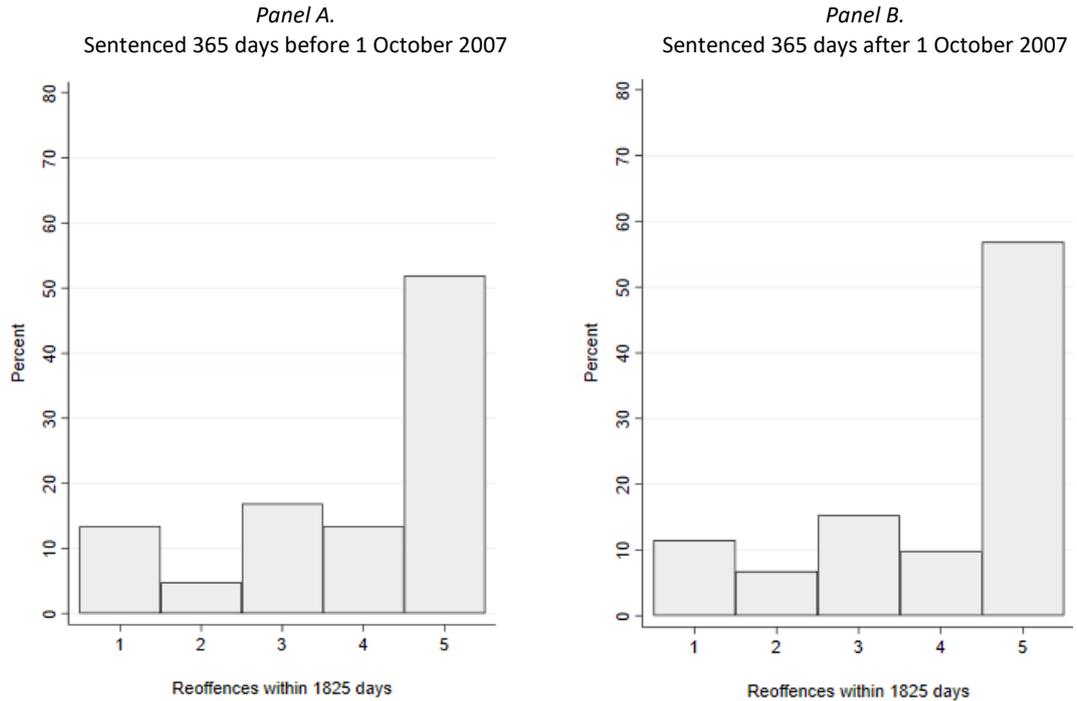
Figure C3. Category of reoffence by sentencing date for a two-year recidivism period



Notes: Offences are categorised as follows: (1) homicide, injury-causing and sexual offences; (2) dangerous acts, abduction and harassment offences; (3) robbery, extortion, burglary and theft; (4) fraud, deception, drugs and weapon crimes; and (5) property offences, public order offences, traffic offences and offences against justice. For confidentiality reasons, Category 6 offences (miscellaneous offences and offences with inadequate information) are suppressed in this analysis.

Source: Own calculations based on data in Statistics New Zealand's IDI.

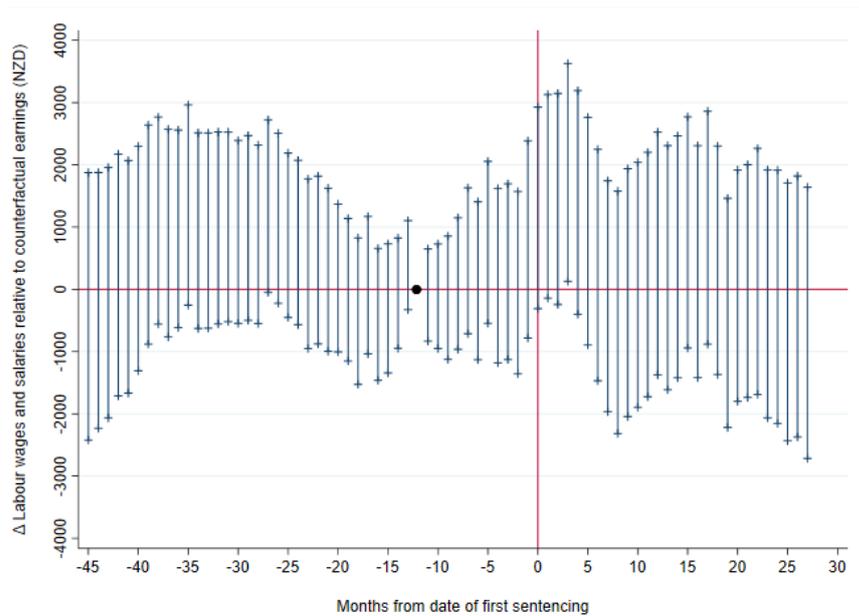
Figure C4. Category of reoffence by sentencing date for a five-year recidivism period



Notes: Offences are categorised as follows: (1) homicide, injury-causing and sexual offences; (2) dangerous acts, abduction and harassment offences; (3) robbery, extortion, burglary and theft; (4) fraud, deception, drugs and weapon crimes; and (5) property offences, public order offences, traffic offences and offences against justice. For confidentiality reasons, Category 6 offences (miscellaneous offences and offences with inadequate information) are suppressed in this analysis.

Source: Own calculations based on data in Statistics New Zealand’s IDI.

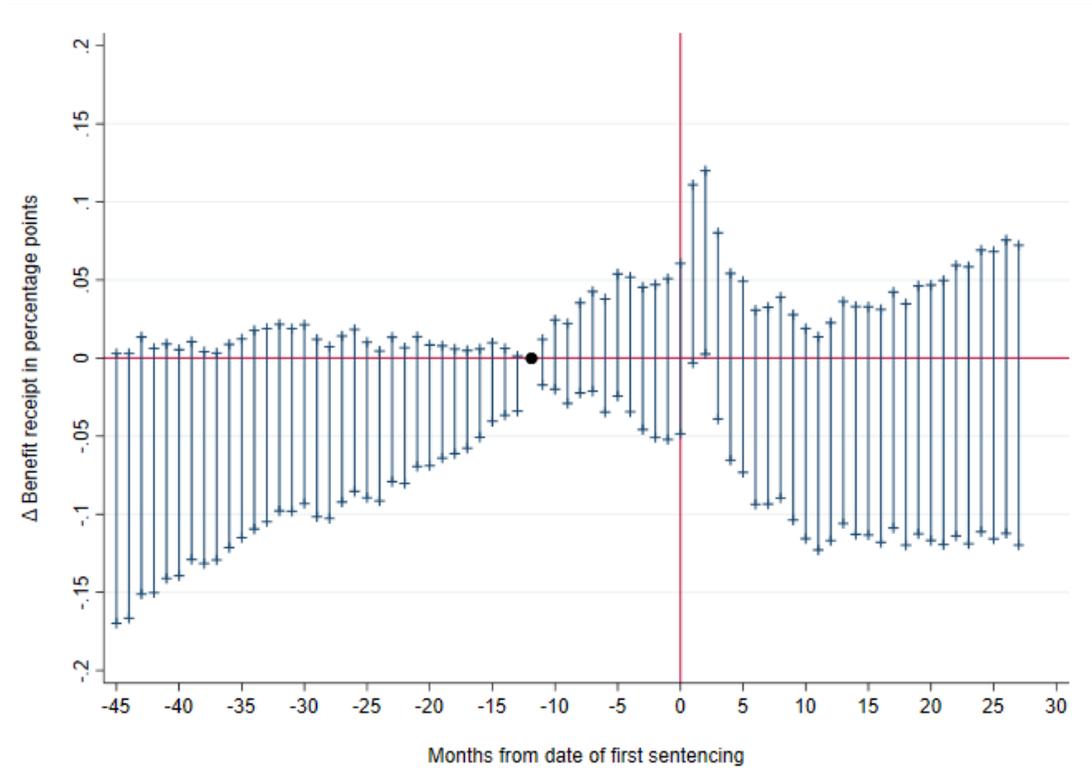
Figure C5. The average effect of the home detention reform on average monthly wages and salaries earnings



Notes: Offenders’ sentencing dates are normalised to month zero. This figure shows the effect of the home detention reform on average monthly wages and salaries earnings, measured by the change in the wages and salaries earnings between the treatment and control groups relative to month -12, shown by the black marker. The vertical lines represent the 95 percent confidence interval of each estimate, as calculated by Equation 7.

Source: Own calculations based on data in Statistics New Zealand’s IDI.

Figure C6. The average effect of the home detention reform on average benefit receipt



Notes: Offenders' sentencing dates are normalised to month zero. This figure shows the effect of the home detention reform on average benefit receipt, measured by the change in the benefit receipt between the treatment and control groups relative to month -12, shown by the black marker. The vertical lines represent the 95 percent confidence interval of each estimate, as calculated by Equation 7.

Source: Own calculations based on data in Statistics New Zealand's IDI.

Table C1. Covariate balance for sampled offenders sentenced 50 days before and after  
1 October 2007

	<i>Control group</i>		<i>Treatment group</i>		<i>t-statistic</i>
	<i>N</i>	<i>Mean (SD)</i>	<i>N</i>	<i>Mean (SD)</i>	
<i>Demographic</i>					
Male	99	0.80 (0.41)	96	0.83 (0.38)	-0.67
Age	96	29.6 (12.6)	93	28.5 (12.9)	0.62
Religious	39	0.46 (0.50)	39	0.45 (0.50)	0.10
Born in NZ	42	0.86 (0.35)	45	0.84 (0.37)	0.21
European	96	0.57 (0.50)	93	0.55 (0.50)	0.19
Māori	96	0.26 (0.44)	93	0.25 (0.43)	0.21
Pacific Peoples	96	0.12 (0.33)	93	0.10 (0.30)	0.62
Asian	96	0.04 (0.20)	93	0.05 (0.23)	-0.39
MEELA	96	0.01 (0.10)	93	0.05 (0.23)	-1.70
Auckland City	99	0.05 (0.22)	96	0.14 (0.34)	-2.04 *
Christchurch City	99	0.16 (0.37)	96	0.06 (0.24)	2.23 *
Wellington City	99	0.04 (0.20)	96	0.10 (0.31)	-1.71
<i>Crime</i>					
Category 1	99	0.38 (0.49)	96	0.25 (0.44)	1.92
Category 2	99	0.05 (0.22)	96	0.07 (0.26)	-0.63
Category 3	99	0.16 (0.37)	96	0.20 (0.40)	-0.62
Category 4	99	0.17 (0.38)	96	0.26 (0.44)	-1.47
Category 5	99	0.20 (0.41)	96	0.21 (0.41)	-0.07
Category 6	99	0.03 (0.17)	96	0.01 (0.10)	0.99
<i>Education</i>					
NCEA 1	30	0.29 (0.46)	48	0.15 (0.36)	1.56
NCEA 2	30	0.03 (0.18)	48	0.10 (0.31)	-1.17
NCEA 3	30	0.13 (0.34)	48	0.17 (0.38)	-0.45
Low decile	27	0.46 (0.51)	42	0.32 (0.47)	1.16
Medium decile	27	0.39 (0.50)	42	0.45 (0.50)	-0.46
High decile	27	0.14 (0.36)	42	0.22 (0.42)	-0.84
<i>Earnings</i>					
Gross 2006 WS	99	55,321 (139,856)	96	55,613 (142,961)	-0.01
Gross 2005 WS	99	53,570 (117,257)	96	57,103 (145,130)	-0.19
Gross WS year mix	99	53,570 (117,257)	96	55,613 (142,961)	-0.11
Gross 2006 BEN	99	28,187 (93,198)	96	15,233 (37,375)	1.27
Gross 2005 BEN	99	18,456 (56,148)	96	11,169 (36,279)	1.07

Notes: Offences are categorised as follows: (1) homicide, injury-causing and sexual offences; (2) dangerous acts, abduction and harassment offences; (3) robbery, extortion, burglary and theft; (4) fraud, deception, drugs and weapon crimes; (5) property offences, public order offences, traffic offences and offences against justice; and (6) miscellaneous offences and offences with inadequate information. Each NCEA level represents the highest attained secondary school qualification. School deciles are from the last attended secondary school, ranked as follows: 1-3 is low, 4-7 is medium and 8-10 is high. WS are earnings from wages and salaries. BEN are earnings from benefit receipt. Gross WS year mix observes wages and salaries earnings from 2006 for the treatment group and 2005 for the control group. All earnings variables are measured in NZD. Standard deviations are in parentheses. Equality of means are tested by the t-statistic where statistically significant differences are indicated with \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .

Source: Own calculations based on data in Statistics New Zealand's IDI.

Table C2. Covariate balance for sampled offenders sentenced 100 days before and after  
1 October 2007

	<i>Control group</i>		<i>Treatment group</i>		<i>t- statistic</i>
	<i>N</i>	<i>Mean (SD)</i>	<i>N</i>	<i>Mean (SD)</i>	
<i>Demographic</i>					
Male	198	0.81 (0.39)	231	0.75 (0.44)	1.68
Age	198	30.4 (13.4)	225	28.7 (12.6)	1.31
Religious	75	0.47 (0.50)	87	0.50 (0.50)	-0.42
Born in NZ	81	0.89 (0.32)	99	0.82 (0.39)	1.35
European	201	0.56 (0.50)	231	0.54 (0.50)	0.43
Māori	201	0.27 (0.44)	231	0.24 (0.43)	0.73
Pacific Peoples	201	0.09 (0.29)	231	0.11 (0.32)	-0.61
Asian	201	0.05 (0.21)	231	0.06 (0.23)	-0.54
MEELA	201	0.03 (0.17)	231	0.05 (0.22)	-1.14
Auckland City	201	0.06 (0.25)	231	0.13 (0.34)	-2.38 *
Christchurch City	201	0.14 (0.35)	231	0.07 (0.25)	2.43 *
Wellington City	201	0.05 (0.23)	231	0.08 (0.27)	-0.95
<i>Crime</i>					
Category 1	201	0.37 (0.48)	231	0.28 (0.45)	1.96 *
Category 2	201	0.05 (0.22)	231	0.05 (0.22)	-0.09
Category 3	201	0.15 (0.36)	231	0.17 (0.38)	-0.65
Category 4	201	0.24 (0.43)	231	0.32 (0.47)	-1.95
Category 5	201	0.17 (0.38)	231	0.16 (0.37)	0.29
Category 6	201	0.02 (0.14)	231	0.01 (0.09)	1.00
<i>Education</i>					
NCEA 1	66	0.22 (0.42)	90	0.19 (0.40)	0.50
NCEA 2	66	0.03 (0.17)	90	0.08 (0.27)	-1.29
NCEA 3	66	0.15 (0.36)	90	0.15 (0.35)	0.06
Low decile	57	0.46 (0.50)	78	0.38 (0.49)	0.95
Medium decile	57	0.41 (0.50)	78	0.37 (0.49)	0.49
High decile	57	0.13 (0.33)	78	0.25 (0.44)	-1.79 *
<i>Earnings</i>					
Gross 2006 WS	201	38,629 (108,774)	231	57,200 (160,887)	-1.38
Gross 2005 WS	201	37,627 (97,559)	231	56,342 (158,400)	-1.45
Gross mixed WS	201	37,627 (97,559)	231	57,200 (160,887)	-1.50
Gross 2006 BEN	201	22,222 (72,006)	231	17,007 (43,364)	0.93
Gross 2005 BEN	201	13,531 (42,936)	231	13,080 (38,883)	0.11

*Notes:* Offences are categorised as follows: (1) homicide, injury-causing and sexual offences; (2) dangerous acts, abduction and harassment offences; (3) robbery, extortion, burglary and theft; (4) fraud, deception, drugs and weapon crimes; (5) property offences, public order offences, traffic offences and offences against justice; and (6) miscellaneous offences and offences with inadequate information. Each NCEA level represents the highest attained secondary school qualification. School deciles are from the last attended secondary school, ranked as follows: 1-3 is low, 4-7 is medium and 8-10 is high. WS are earnings from wages and salaries. BEN are earnings from benefit receipt. Gross WS year mix observes wages and salaries earnings from 2006 for the treatment group and 2005 for the control group. All earnings variables are measured in NZD. Standard deviations are in parentheses. Equality of means are tested by the t-statistic where statistically significant differences are indicated with \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .

*Source:* Own calculations based on data in Statistics New Zealand's IDI.

Table C3. Covariate balance for sampled offenders sentenced 730 days before and after  
1 October 2007

	<i>Control group</i>		<i>Treatment group</i>		<i>t-statistic</i>
	<i>N</i>	<i>Mean (SD)</i>	<i>N</i>	<i>Mean (SD)</i>	
<i>Demographic</i>					
Male	1,371	0.74 (0.44)	2,454	0.72 (0.45)	1.36
Age	1,338	30.2 (12.3)	2,424	29.5 (13.0)	1.77
Religious	483	0.52 (0.50)	975	0.51 (0.50)	0.30
Born in NZ	528	0.84 (0.37)	1,068	0.82 (0.38)	1.05
European	1,371	0.59 (0.49)	2,451	0.54 (0.50)	3.28 **
Māori	1,371	0.26 (0.44)	2,451	0.29 (0.45)	-2.07 *
Pacific Peoples	1,371	0.09 (0.29)	2,451	0.11 (0.31)	-1.38
Asian	1,371	0.04 (0.20)	2,451	0.04 (0.20)	-0.30
MEELA	1,371	0.02 (0.13)	2,451	0.03 (0.16)	-1.49
Auckland City	1,386	0.09 (0.29)	2,460	0.09 (0.28)	0.21
Christchurch City	1,386	0.09 (0.29)	2,460	0.09 (0.28)	0.24
Wellington City	1,386	0.04 (0.19)	2,460	0.05 (0.21)	-1.06
<i>Crime</i>					
Category 1	1,386	0.27 (0.44)	2,460	0.28 (0.45)	-0.76
Category 2	1,386	0.04 (0.20)	2,460	0.05 (0.22)	-1.83
Category 3	1,386	0.17 (0.37)	2,460	0.19 (0.40)	-1.97 *
Category 4	1,386	0.32 (0.47)	2,460	0.30 (0.46)	1.35
Category 5	1,386	0.18 (0.38)	2,460	0.15 (0.36)	2.39 *
Category 6	1,386	0.02 (0.13)	2,460	0.02 (0.13)	-0.05
<i>Education</i>					
NCEA 1	465	0.17 (0.37)	945	0.19 (0.39)	-0.94
NCEA 2	465	0.07 (0.26)	945	0.10 (0.30)	-1.62
NCEA 3	465	0.16 (0.37)	945	0.17 (0.38)	-0.58
Low decile	402	0.47 (0.50)	852	0.42 (0.49)	1.69
Medium decile	402	0.37 (0.48)	852	0.42 (0.49)	-1.72
High decile	402	0.16 (0.36)	852	0.16 (0.36)	0.02
<i>Earnings</i>					
Gross 2006 WS	1,386	43,656 (141,576)	2,460	62,693 (278,591)	-2.38 *
Gross 2005 WS	1,386	57,002 (181,626)	2,460	56,868 (372,651)	0.01
Gross WS year mix	1,386	57,002 (181,626)	2,460	62,693 (278,591)	-0.68
Gross 2006 BEN	1,386	20,518 (101,747)	2,460	15,007 (52,598)	2.21 *
Gross 2005 BEN	1,386	19,161 (98,937)	2,460	13,025 (48,007)	2.58 **

Notes: Offences are categorised as follows: (1) homicide, injury-causing and sexual offences; (2) dangerous acts, abduction and harassment offences; (3) robbery, extortion, burglary and theft; (4) fraud, deception, drugs and weapon crimes; (5) property offences, public order offences, traffic offences and offences against justice; and (6) miscellaneous offences and offences with inadequate information. Each NCEA level represents the highest attained secondary school qualification. School deciles are from the last attended secondary school, ranked as follows: 1-3 is low, 4-7 is medium and 8-10 is high. WS are earnings from wages and salaries. BEN are earnings from benefit receipt. Gross WS year mix observes wages and salaries earnings from 2006 for the treatment group and 2005 for the control group. All earnings variables are measured in NZD. Standard deviations are in parentheses. Equality of means are tested by the t-statistic where statistically significant differences are indicated with \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .

Source: Own calculations based on data in Statistics New Zealand's IDI.

Table C4. Covariate balance for sampled offenders sentenced 1,825 days before and after  
1 October 2007

	<i>Control group</i>		<i>Treatment group</i>		<i>t-statistic</i>
	<i>N</i>	<i>Mean (SD)</i>	<i>N</i>	<i>Mean (SD)</i>	
<i>Demographic</i>					
Male	3,381	0.73 (0.44)	6,723	0.70 (0.46)	3.64 **
Age	3,288	30.6 (12.2)	6,636	29.9 (13.3)	2.39 *
Religious	1,233	0.52 (0.50)	2,883	0.50 (0.50)	1.39
Born in NZ	1,368	0.84 (0.37)	3,156	0.81 (0.39)	1.82
European	3,369	0.57 (0.50)	6,693	0.53 (0.50)	4.05 **
Māori	3,369	0.27 (0.44)	6,693	0.30 (0.46)	-3.14 **
Pacific Peoples	3,369	0.10 (0.30)	6,693	0.10 (0.30)	-0.82
Asian	3,369	0.05 (0.21)	6,693	0.05 (0.22)	-0.66
MEELA	3,369	0.02 (0.13)	6,693	0.02 (0.14)	-1.47
Auckland City	3,396	0.11 (0.31)	6,741	0.10 (0.30)	0.58
Christchurch City	3,396	0.10 (0.30)	6,741	0.08 (0.27)	3.33 **
Wellington City	3,396	0.04 (0.19)	6,741	0.04 (0.19)	-0.40
<i>Crime</i>					
Category 1	3,396	0.27 (0.45)	6,741	0.28 (0.45)	-0.17
Category 2	3,396	0.05 (0.21)	6,741	0.05 (0.22)	-0.64
Category 3	3,396	0.16 (0.37)	6,741	0.18 (0.39)	-2.91 **
Category 4	3,396	0.37 (0.48)	6,741	0.32 (0.47)	4.15 **
Category 5	3,396	0.14 (0.35)	6,741	0.15 (0.36)	-1.52
Category 6	3,396	0.02 (0.13)	6,741	0.02 (0.14)	-0.79
<i>Education</i>					
NCEA 1	1,146	0.15 (0.36)	2,691	0.18 (0.38)	-2.30 *
NCEA 2	1,146	0.08 (0.27)	2,691	0.10 (0.31)	-2.57 **
NCEA 3	1,146	0.13 (0.34)	2,691	0.17 (0.38)	-3.28 **
Low decile	993	0.43 (0.50)	2,445	0.41 (0.49)	1.22
Medium decile	993	0.41 (0.49)	2,445	0.45 (0.50)	-1.84
High decile	993	0.15 (0.36)	2,445	0.14 (0.35)	0.89
<i>Earnings</i>					
Gross 2006 WS	3,396	48,954 (145,199)	6,741	51,878 (213,652)	-0.72
Gross 2005 WS	3,396	49,420 (148,560)	6,741	47,667 (263,230)	0.36
Gross WS year mix	3,396	49,420 (148,560)	6,741	51,878 (213,652)	-0.60
Gross 2006 BEN	3,396	24,582 (92,664)	6,741	14,223 (50,355)	7.29
Gross 2005 BEN	3,396	22,056 (85,949)	6,741	13,132 (48,187)	6.69

Notes: Offences are categorised as follows: (1) homicide, injury-causing and sexual offences; (2) dangerous acts, abduction and harassment offences; (3) robbery, extortion, burglary and theft; (4) fraud, deception, drugs and weapon crimes; (5) property offences, public order offences, traffic offences and offences against justice; and (6) miscellaneous offences and offences with inadequate information. Each NCEA level represents the highest attained secondary school qualification. School deciles are from the last attended secondary school, ranked as follows: 1-3 is low, 4-7 is medium and 8-10 is high. WS are earnings from wages and salaries. BEN are earnings from benefit receipt. Gross WS year mix observes wages and salaries earnings from 2006 for the treatment group and 2005 for the control group. All earnings variables are measured in NZD. Standard deviations are in parentheses. Equality of means are tested by the t-statistic where statistically significant differences are indicated with \*\* if  $p < 0.01$  and \* if  $p < 0.05$ .

Source: Own calculations based on data in Statistics New Zealand's IDI.