# Going Straight Home?
# Exploring potential relationships between stable housing and reoffending for ex-prisoners

Anika Terry

A thesis submitted in partial fulfilment of the requirements
for the degree of Master of Science in Statistics,
The University of Auckland, 2022

Supervisor: Dr Barry Milne

# Acknowledgements

I would like to sincerely thank both my supervisor, Dr Barry Milne and co-supervisor, Dr Alice Mills for all their knowledge and support throughout this thesis. Their shared desire to provide research aimed to inform policies that may impact vulnerable people's lives for the better is inspiring. It has been an honour to be chosen to work on such a worthwhile piece of research.

Further thanks go to Cinnamon Lindsay Latimer, the research assistant on the Going Straight Home? project, who conducted most of the research interviews.

I would like to express my appreciation to the Royal Society of New Zealand Marsden Fund who funded this research.

I would like to thank Corrections staff for their part in supporting and facilitating the research, as well as the research participants who provided their time and experiences.

Further thanks go to the Māori external reference for this research: Sir Kim Workman, Professor Tracey McIntosh, Tui Gallagher (PARS), Shane White (Hoani Waititi Marae), Elaine Ngamu (Ahikaa Trust), and Karen Ngamu (Ahikaa Trust), for their assistance and advice

# Abstract

Recidivism is an issue of great complexity as so many factors come into play. Other research has identified offending being affected by many factors including personal history and upbringing, necessary support, and connections to the community. Many studies reference stable housing as being a crucial platform to reducing recidivism and successful reintegration into the community. Other research has primarily focused on the overall effect of wrap around services combined with stable housing or whether stable housing is a significant predictor of recidivism, rather than focusing on the estimated effect of stable housing. This research explores the possibility of causal effects of stable housing after release from prison on recidivism.

Eligible people from six New Zealand prisons were approached to participate in the study[1]. Participants were interviewed pre-release and approximately 6 and 12 months post-release. Four different analytic techniques were employed, all with covariate adjustment: *i) logistic regression*; *ii) model averaging*; *iii) non-response weighted logistic regression*; and *iv) Inverse Probability Treatment Weighting*.

Bivariate analysis showed participants who reported living in unstable housing approximately six months after release were between 1.5 to 4.6 times more likely to reoffend. The strongest bivariate relationship was between housing type and re-imprisonment (Chi-squared test p-value = 0.020). However, when key confounding variables identified were adjusted for, the estimates of the effects across each analytic technique showed agreement of no practically significant effect across models; almost all confidence intervals of estimated effects of stable housing spanned zero.

Despite some evidence of significant associations between stable housing measures and recidivism in bivariate analyses, there was no evidence the effects of stable housing were significant when other suspected confounding variables were incorporated in the analyses. This suggests either the sample size was too small to identify the significance of causal effects of stable housing, or these effects were explained by other suspected confounding variables (Skelly et al., 2012).


[1] Selected with the help of Corrections.

Keywords: recidivism; reoffending; stable housing; cohort study; observational study; logistic regression; model averaging; non-response; Inverse Probability Treatment Weighting (IPTW)

## Table of Contents

## List of Figures



## List of Tables

# 1. Introduction

## 1.1 Background

Interviews for this research began in 2019. Before 2019 the number of people in prison in New Zealand has been rising since the 1980s, seemingly due more to changes in political stances on offending rather than the crime rate (Workman & McIntosh, 2013). As of 2018, the year before this data was collected, New Zealand's imprisonment rates were among the highest in the developed world, with 219 prisoners per 100,000 people (Boomen & Corrections, 2018). Forecasts from the Ministry of Justice (2020) also predicted gradual increases from 2019 to 2029 in the total prison population due to the increasing remand population, rather than increases in the sentenced population.

However, promisingly, the total prison population has been reducing since it peaked in March 2018, starting with a slight reduction of almost 8% of the total prison population from March 2018 to June 2019 (Corrections, 2019), reducing by a further 12% from the peak in March 2018 to June 2020 (Corrections, 2020a). A report a year later from the Ministry of Justice (2021) reiterated recent decreases and reported further reductions in the total prison population due to: less serious offences being resolved out of court, an increased focus on intensive supervision and rehabilitation, as well as a decrease in the proportion time in being spent in prison before being released on parole. Moreover, New Zealand's imprisonment rate has dropped to 169 per 100,000 people in 2021 (Corrections, 2021).

Based on figures from the 2009 financial year the average cost of keeping an individual incarcerated for a full year was about $91,000 on average (Corrections, 2010). In 2018 the Minister of Corrections, Kelvin Davis, was quoted as saying each incarcerated person "costs about $100,000 a year" (Stewart, 2018). Corrections (Dorne et al., 2016) released a report noting investing in more intensive reintegrative services, which incorporate the individual's needs, have shown the best results in terms of rehabilitating previous offenders. If investing in reintegrative services helps ex-prisoners to reintegrate into the community successfully it could reduce the costs to the New Zealand government over time.

A recidivism report from Corrections by Arul Nadesu (2009) found that of the almost 5,000 offenders who were released within a span of a year between 2002-2003, 52% were reconvicted within 5 years, with half of these reconvictions happening within the first year. This suggests the one-year research period employed is likely to see half of the people who will reoffend within 5 years reoffend. Such a high reoffence rate suggests offenders are not being successfully rehabilitated and reintegrated into society. Rather than becoming settled, productive members of our society, previous offenders are being drawn again and again into the criminal justice system.

With just over 7,400[2] offenders released in 2019 it is of great importance what, if any, interventions or aid could be provided to successfully reintegrate these people back into the community. Otherwise, following from the report by Arul Nadesu (2009), over 3,700 of these ex-offenders may be reconvicted within 5 years. Stable housing is one of, if not the most crucial, tool for reintegration as the "platform for recovery, employment, education and wider community engagement and participation" (Ministry of Housing and Urban Development, 2019). International research has indicated stable housing is vital to reintegrating into society (Mills et al., 2013; Bradley et al., 2001; Lutze et al., 2014; Metraux & Culhane, 2004). Stable housing is integrated in New Zealand's Six Pillar Model of reintegration, which places accommodation as the first pillar (Tissera, 2019; Dorne et al., 2016**)**. However, despite this view of stable housing as a base for change, reintegration into the community and accessing other services, research has not had the same focus on the what the actual effects of stable housing may be. Rather, current research focuses on either the effects of living on the streets and in shelters, or the need for stable housing as part of other programmes and support. This research is based upon the premise that not only is stable housing important as part of a wraparound services but stable housing may also be beneficial in itself – in part acting as a platform to take part in other programmes and as a base to receive other support.

### 1.1.1 Research Focus

The aim of this research is to investigate the potential causal effect of stable housing on recidivism as measured by *i) re-sentencing* and *ii) re-imprisonment* within a year of release in New Zealand. The research does not relate to a specific area of New Zealand as the interviews were carried out in 6 prisons[3]. Stable housing was measured in two ways *i) housing type* and *ii) residential mobility*

[2] Corrections provided information on the number of releases in the 2019 calendar year, these consist of 5053 releases from short-term sentences (up to two years) and 2365 from long-term sentences (more than 2 years).
[3] In this paper the term 'prisons' will refer to the mix of prisons and Corrections facilities included in this research.

over a 6 month period. These measures are expanded upon further in section 1.4 Important Definitions below.

Housing type:

i. Stable types of housing are defined as: living in a housing type which is owned/partly owned, rented, state housing or NGO/Housing First accommodation.
ii. Unstable types of housing are defined as: living in a housing type which includes staying with family/friends, in a hotel/motel, hostel/boarding house/backpackers/campground/holiday park or on the street.

Residential mobility:

i. Having stable housing is related to low residential mobility, moving infrequently – at most one time during a six month period.
ii. Having unstable housing is related to having high residential mobility, moving frequently – two or more times during an approximately six month period.

Stable housing would abide by the causal criteria Hill (1965) proposed:

- *Strength and consistency*: the strength of the association and the number of times the association has been observed should be considered. Although this condition is not necessary or sufficient, associations between housing after prison and recidivism have been observed in many other studies (Baldry et al. 2006; Brunton-Smith et al., 2013; Lutze et al, 2014). Despite the association or effects varying across studies, researchers often support the relationship between stable/unstable housing and recidivism.
- *Specificity*: Hill suggests a cause-and-effect relationship is more reliably identified when a single cause can be identified to have one specific effect. However, Hill did acknowledge multi-causation is more common than one-to-one relationships (Hill, 1965). There is unlikely to be any one measure of housing that would cause any one measure of recidivism. However, to present a well-defined exposure and outcome two specific definitions of stable housing were used to best define both the type and the mobility of their housing situation. Two measures of recidivism were also used to capture if they had been caught and re-sentencing or more severely re-imprisoned. Directed A-cyclical Graphs were created to account for suspected causes for both housing and recidivism.

- *Temporality*: temporality must be considered to ensure the suspected cause occurs before the outcome. In this case living in housing or lacking housing after release from prison will occur before recidivism.
- *Dose-response*: if lack of stable housing is a cause, the more people released from prison living in unstable housing, the greater the likelihood of recidivism. From our own study research participants living in unstable housing reoffended more than those in stable housing, it is plausible more people in unstable housing would cause higher rates of recidivism.
- *Plausibility*: if it seems realistic a suggested cause might lead to a proposed effect. This is certainly the case when reviewing literature in the area, Bradley et al. (2001) have called stable housing the "lynch-pin" to an ex-offender's successful reintegration back into society.
- *Coherence*: there should not be any significant conflict between the proposed cause-and-effect alongside facts generally known (Hill, 1965). the interpretation of stable housing after release from prison effecting recidivism does not conflict with generally held knowledge.
- *Experiment*: in some cases, experimental evidence can be gathered and intervention can be made to the suspected cause then the frequency of the associated events can be measured. In a case such as this it is not possible to get experimental evidence, as withholding stable housing and assigning unstable housing to a person would be unethical.
- *Analogy*: if other similar effects of a variable on an outcome are seen, we may be more likely to accept another, similar relationship. The effect of stable housing on recidivism seems like other expected relationships, where a healthy environment offers room for positive change.

Although the actual causal model is suspected, unfortunately, whether this is true or not cannot be known. The statistical measures used to describe the pathways in which stable housing and other factors combine and act to affect recidivism are unlikely to describe the variables represented exactly and their relationships may have added layers of complexity that could not be reduced down to the data collected. Although this paper focuses on the effect of stable housing on recidivism, to "reflect the actual relationship" (Pourhoseingholi et al., 2012), other personal and societal factor variables related to securing stable housing after release as well as affecting exposure were accounted for in the analysis.

## 1.2 Literature Review

The role of homelessness and unstable accommodation on recidivism has been generally agreed upon in international research (Corden et al., 1979; Mills et al., 2013; Metraux & Culhane, 2004;

Brunton-Smith et al., 2013; Lutze et al., 2014; Baldry et al., 2006; Carlisle, 1996; Ellison et al., 2013). Housing is credited with having a pivotal role in facilitating reintegration into the community (Mills et al., 2013). The role of stable housing is agreed to be important even if just anecdotally, even in studies where the effects are not significant. Stable housing is needed for benefits such as government provided health and monetary benefits (Lutze et al., 2014), creating and reconnecting with social support systems, reintegrating into the community (Bradley et al., 2001), and gaining employment. Despite the overwhelming advocacy for stable housing after release from prison, the statistical evidence in support is not overwhelming. Some research on stable housing suggests housing services are effective, while others show no significant effect (Dorne et al., 2016). However, many studies have related to finding the effect of either unstable housing measured by homelessness or stable housing offered as part of a larger series of re-integrative wrap-around services.

Many studies have investigated street or shelter stay homelessness. Some more famous of these studies were undertaken by Metraux & Culhane (2004) and Lutze et al., (2014). This focus on homelessness over stable housing is likely due to its severity. Homelessness is the most serious manifestation of housing related issues as, in a twisted way, prison is a type of housing (Baldry et al., 2002), some people unable to find any housing before returning to prison. Homelessness is often a result of a cumulative set of issues such as a lack of social support, monetary resources, employment, and psychological issues (Metraux & Culhane, 2004). Lutze et al. (2014) found that periods of homelessness could significantly raise the risk of recidivism.

Still more research focuses on stable housing including the wrap around programmes and services that an organisation provides (Ellison et al., 2013; Lutze et al., 2014). In New Zealand research has been undertaken to estimate the effectiveness of services such as Out of Gate (Dorne et al., 2016). Although providing services such as stable housing and wrap-around reintegrative services may be very helpful for many, ex-offenders may still choose not to use them if they have to abide by rules of a programme. No matter the resources, if a person is not ready and willing to receive supportive transitional services for stable housing, they cannot be forced to use these services. Moreover, when spaces on programmes are given to people who are not committed to the rules, it often leads to rule breaking and termination from programmes anyway (Lutze et al., 2014). If an ex-offender is not prepared or committed to a programme it is unlikely to be effective.

Some studies such as Metraux & Culhane (2004) have relied upon administrative sources for their data. Although this provides a larger sample to work with, it does not provide the same depth of information that can be obtained from interviews. The government does not hold the same

information about housing as can be gathered from an interview (Baldry et al., 2006). A greater depth of information is required to properly control for confounding to better estimate the true effects of stable housing on recidivism.

Research exploring the statistical relationship between stable housing and recidivism has primarily come out of America, the United Kingdom, Canada, and Australia. New Zealand specific research into the relationship between stable housing and recidivism outside of wrap around programmes and services is lacking.

## 1.3 Housing in New Zealand

New Zealand has a housing crisis (King et al., 2020). Homelessness is prevalent, renters struggle to find suitable affordable houses and potential buyers fight against high costs of housing. Rent in New Zealand is expected to be about $230 a week per person for both Auckland and Wellington, based on living in a 3-bedroom house with two other people (Ministry of Social Development, n.d.). Furthermore, as may be intuitively thought, where housing is difficult and expensive to find for people who have clean records, housing is especially hard to find for ex-offenders (Mills et al., 2021).

The Department of Corrections supplies "over 1,100 accommodation places to people leaving prison or serving community-based sentences" (Corrections, 2020). With further trial housing project, Creating Positive Pathways, a partnership with Corrections, the Department of Housing and Urban Development (HUD), the Ministry of Social Development (MSD), and local iwi used local NGOs to providing further accommodation for ex-prisoners in private rental accommodation (Malatest International, 2020). Accommodation where an ex-prisoner can reintegrate into society has been identified as the most beneficial thing to successful reintegration by Morrison and Bowman (2017).

Housing supplied by family/friends is a large source of accommodation for people leaving prison (Baldry et al., 2006). In the pre-release interviews for this study, 43% of the research participants planned on living with family/friends, this was the main source of planned accommodation after release. Living with friends/family can help to provide much support to people released from prison, though was also often perceived as short-term, sometimes involving navigations of complicated relationships (Mills et al., 2021). Despite so many people who are released from prison going to live with family/friends, the New Zealand system largely focuses on an "individualised approach" (Mills

et al., 2021), offering individual flats or rooms in shared housing rather than support for the friends/families.

## 1.4 Important Definitions

**Housing stability measures**

This research has settled upon two measures of stable housing as also described in the final research report by Mills et al. (Forthcoming):

1. Residential mobility, as measured by the number of moves; and
2. Housing type, as grouped into stable and unstable housing types.

The first measure of stable housing is residential mobility: the number of housing moves within an approximately six month period. Moving two or more times is considered to be high residential mobility or unstable housing. Residential mobility helps to represent the instability and lack of consistency of the housing available (Baldry et al., 2003). The concept of residential mobility can capture what a type of housing cannot, how long-term or permanent the housing is to a participant. Despite many participants choosing to stay with friends/family after release, many appeared to be moving around between different households rather than this accommodation providing stable long-term accommodation. In cases where participants said they were 'homeless' or living 'on the street', such as in a car, even if they claimed not to have moved they were recorded to have moved at least two times to properly capture the instability they faced. Someone who is living in a situation such as this would not necessarily be able to return to the same place they slept the night before, so their situation is unstable.

The second measure of stable housing is based upon housing type. Participants were asked what type(s) of housing they had lived in an approximately 6 month period before an interview/prison. The housing options given were: owned or partly owned, rented housing, state housing, Housing First / NGO provider, friends/family, hotel or motel, hostel/boarding house/backpackers/campground/holiday park, on the street, and somewhere else. Although many participants would choose only one housing type, for those who chose multiple types the least stable housing type chosen was recorded as the type of housing they had lived in. This approach was required as there was not enough information given by most participants to understand the direction of the moves and unstable housing was thought to be the more harmful outcome. For example, if a

participant had answered they had periods of living with family/friends and renting in the six months after release, it was not always possible to know whether they had gone from renting to living with family/friends or vice versa. The housing types were further grouped into 'Stable' and 'Unstable' housing types. The 'Stable' housing types were owned or partly owned, rented, state housing, and Housing First/NGO housing. The 'Unstable' housing included living with family/friends, hotel/motel, hostel/boarding house/backpackers/campground/holiday park, and on the street. The housing categories were used to better understand where participants had been living, although information about when a research participant moved from one housing type to another was unavailable.

Aside from people who were 'living on the street' being forced to be categorised as having at least two moves in the past six months, each measure of stable housing was not used to inform the other. The two measures were also not used in any combination for analysis. Separate analyses were run for unstable housing due to residential mobility and unstable housing due to unstable housing type. In further studies it may be of use to incorporate more aspects of stable housing such as type of housing, number of moves, how shared the space is and perhaps even whether the room was furnished as in the paper by Corden et al. (1979). However, it is difficult to take into account the range of housing types lived in over a time period after release.

'Unstable' housing including family/friends may seem controversial. However, this decision was supported by existing research, which suggested accommodation offered by family and/or friends often come part and parcel with difficult relationships. Previous offenders may move away from family and/or friends in a bid for independence or to avoid conflict (Lutze et al., 2014). In most cases returning from prison to live with family and/or friends was unsustainable (Baldry et al., 2002). This is further supported by New Zealand's official homelessness definition as "a living situation where people with no other options to acquire safe and secure housing are: without shelter, in temporary accommodation, sharing accommodation with a household, or living in uninhabitable housing" (Stats NZ, 2015). This definition covers homelessness well as it can often be hidden and should cover people who are in unstable living types, not just seen in people living on the streets. Terms such as 'homeless' have been used "as a blanket term for many living circumstances" (Corden et al., 1979, p. 75), one of these being bouncing between accommodation of family/friends. There was evidence of family/friends housing being unstable in our study as well as more than half of the people who were living with 'family/friends' prior to their initial prison sentence had moved at least two times before prison (Appendix, Table 80). The way the housing types are categorised is believed to best separate the types of housing based on limited and unequal information across participants.

Housing First and NGO (Non-Government Organisation) housing are shown together throughout this report as the questionnaire gave one option of housing as "Housing First/NGO" housing. Housing First and NGO were grouped as the permanent Housing First accommodation is usually provided by NGOs. Unfortunately, NGO housing can represent permanent, transitional or emergency housing. However, to avoid incorrect recording, when a participant said they had been staying in emergency accommodation were counted as living 'on the street' or homeless rather than in NGO accommodation. At this stage of the research, it is impossible to tell the nature of NGO provided accommodation. There was evidence from other measures that NGO housing among this sample of participants was more stable than unstable. In the six months prior to their prison sentence, seven of those in Housing First/NGO moved once at most and two moved at least two times. Similarly, after release from prison more people in Housing First/NGO accommodation experienced high mobility than more than low mobility. In light of the above reasoning, Housing First/NGO accommodation was grouped as "stable" accommodation.

Statistics New Zealand defines homelessness as "a living situation where people with no other options to acquire safe and secure housing are: without shelter, in temporary accommodation, sharing accommodation with a household, or living in uninhabitable housing" (Stats NZ, 2015). Our definition of unstable housing largely falls under the Stats NZ definition of homelessness, with two exceptions. Firstly, all NGO/Housing First accommodation were classified as permanent housing. Secondly, there was no assessment of the inhabitability of the housing. Although housing may be separated into more stable and less stable types or by how often a person must relocate, whether housing is interpreted as stable by the person living there will be subjective. One person may feel comfortable alone in emergency accommodation, another may feel stable at home with their family, yet another may find living with their friends/family temporary and unstable – perhaps being surrounded by negative influences. This paper uses the information available on housing to provide two measures aimed at best capturing the stability of accommodation. However, it is recognised these are objective measures and will not necessarily represent actual stability of housing for each individual.

Another measure related to housing was briefly explored, the concept of 'home' i.e. whether housing felt like a home and if there was a sense of security in the space. The concept of 'home' gathered information that may help to explore what a home may mean to people recently released from prison in New Zealand, especially as compared to stable housing. Participants were asked in the post-

release interviews whether they thought the place they were living at the time of interview (or prior to returning to prison when re-interviewed in prison) was a 'home'. The prompt for this question asked whether it was "somewhere you feel you can be yourself". The relationship between both stable housing and recidivism measures was unexpected and the research called into question the relationship between perceptions of 'home' and the concept of stable housing.

**Recidivism measures**

Corrections provided information about any re-sentencings and re-imprisonments within 12 months of release for all participants who remained in the study. Whether there had been *i) at least one re-sentencing* or *ii) at least one re-imprisonment* within 12 months from release were the two measures for recidivism used for analysis in this study. Re-sentencing includes instances where "a sentence is a punishment given by a judge", which is administered by Corrections (Corrections, n.d.), it does not include those re-convicted and merely fined or discharged. Re-imprisonment is the most serious re-sentencing outcome and "the most punitive sentence a judge can impose" (Corrections, n.d.).

When re-sentencing or re-imprisonment are referred to throughout this paper it is implied, they will refer to at least one re-sentencing or at least one re-imprisonment within 12 months from release respectively

# 2. Methodology

## 2.1 Data Collection Method

The research gathered data from a cohort of people who were interviewed within four weeks prior to release from one of the following six prisons: Hawke's Bay Regional Prison, Christchurch Men's Prison, Northland Region Corrections Facility, Spring Hill Corrections Facility, Waikeria Prison, or Auckland Region Women's Corrections Facility. These six prisons were chosen out of the 18 adult prisons in New Zealand. The study consisted of one pre-release interview and two post-release interviews. Interviews were conducted by Cinnamon Lindsay Latimer and Dr Alice Mills. This analysis was undertaken to support an original research goal of exploring the potential role of stable housing and recidivism in New Zealand. A further series of interviews were to be undertaken as part of the 'Going straight home?' project to better understand how stable housing can influence the choices of ex-prisoners to desist from further criminal activity.

As explained by Mills et al. (forthcoming), permission for the research study was given by New Zealand Corrections after the Human Participants Ethics Committee at the University of Auckland granted ethics approval. Corrections then aided researchers to identify the most suitable 6 prisons. Prisons were contacted, then it was arranged for the researchers to visit and ask prisoners who were due to be released within four weeks to volunteer their participation in the study. Before signing a consent form all participants were given a comprehensive Participant Information Sheet, which was also verbally explained to each potential participant. The consent form asked permission for researchers to request contact details about participants from Corrections and other organisations such as reintegration agencies. At the end of the pre-release interview contact details were requested from the participants so they might be contacted for post-release interviews including phone numbers, addresses, and/or email addresses. To minimise sample attrition participants were sent postcard and text message reminders including options to update any contact detail changes. Contact details of those who could not be traced after release were requested from Corrections and, in some cases, community reintegration agencies. Participants in post-release interviews were given a koha in the form of a $40 phone or supermarket voucher (excluding interviews occurring in prisons).

The pre-release interviews occurred within four weeks of a participant's expected release. To our knowledge a full list of all prisoners who would be released within four weeks was provided at each prison, although eligible people could and did turn down interviews. This may be different from the population who are incarcerated at any one snapshot in time across New Zealand prisoners as those incarcerated for longer periods of time may change the demographics of this population. Post-release interviews aimed to capture interviewees at 6 months and 12 months post-release. If a participant had returned to one of the six prisons where interviews were initially held interviews were conducted there[4].

Originally 203 people agreed to a pre-release interview. Unfortunately, no information about the larger population of people who were eligible was recorded. There was no information available to ascertain whether the people approached who were asked to be a part of the study but declined were systematically different to those who participated. However, two people withdrew their consent to be a part of the study, leaving a final sample size of 201 participants. Pre-release interviews were held from March to October 2019. In the pre-release interview research participants were asked information about demographic variables, housing experiences before their current imprisonment, information about programmes participated in during imprisonment, and plans for after release. Interviews also collected information on plans for future housing, employment/education, familial/romantic relationships, programmes/treatments for mental health and/or addiction, post-release conditions, mental health, and substance use. Although many of the questions in the interview were closed, interviewers recorded any additional comments participants made during interviews – particularly at the end of interviews – to gain better insight into what may have affected their housing situation.

Post-release interviews were held at approximately 6 and 12 months after release either over the phone or in person. For the first and second wave of post-release interviews 80 (40%) and 66 (33%) participants could be found and interviewed respectively. Overall, 51% of the initial sample was interviewed in at least one post-release interview. In the post-release interviews research participants were asked about their experiences of post-release housing (or housing prior to imprisonment if they had returned to prison), employment/education, familial relationships, programmes/treatments for mental health and/or addiction, mental health, and substance use.

[4] It is important to note that interviewers could only return to the 6 prisons mentioned earlier in this section as permission was not given to enter other prisons to interview research participants.

Retention of the sample could appear low compared to studies done internationally. Similar studies such as Baldry et al. (2006) and Brunton-Smith and Hopkins (2013) with shorter periods between release and their first post release interview (3 and 2 months respectively) retained participants in higher numbers (70% and 57% respectively). Although the percentage of people was not altogether unexpected as it is comparable with Carlisle (1996) who retained only 34% of their original sample in post-release interviews. People who have been imprisoned have proved very difficult to retain in continued research after release, especially due to lack of valid contact details as found in similar studies (Baldry et al., 2002). The study was interested in how residential instability and unstable housing types affected recidivism. Unfortunately, it could have been due to these very factors that participants could not be found for post-release interviews. Moreover, the post-release interviews were impacted by the COVID-19 lockdowns as shown in Figure 4.

Unfortunately, as mentioned in the 3. Data section there was unignorable bias in the study against re-offenders by the second post-release interview. Due to this the focus for analysis was on the effects of stable housing in the approximately 6 months after release from prison before the first wave of post-release interviews. When the post-release interview is referred to it will be this first interview at approximately 6 months after release.

## 2.2 Bias

This study could be particularly prone to attrition bias, response bias, coverage error, selection bias, and confounding (Sedgwick, 2014). Attrition bias may be present depending on why the data are missing. Confounding bias will be present if not all confounding variables are correctly measured and controlled for. Response bias (Lavrakas, 2008) will be present if participants have consistently not accurately answered questions. Selection bias will be present if the prisons chosen were not representative of the whole population of New Zealand prisons.

To reduce missing data from partial non-responses, answers were imputed by hand based on other notes gathered from participants comments to interviewers when appropriate or recorded as "Missing/Unknown" when enough people had not responded to the same question. To avoid processing error, decisions on altering answers or inputting answers based on notes were double and occasionally triple checked by supervisor Dr Barry Milne and the Principal Investigator (PI) for the "Going straight home?" research Dr Alice Mills.

The main types of missing data (Mack et al., 2018) relevant to loss of follow-up in this study are:

1. Missing Completely At Random (MCAR), when every research participant is equally likely to be missing from the post-release interview or not answer a question. If an outcome variable such as participation in the follow-up interview is MCAR, the non-response does not depend on other variables, recorded or not. This type of missing data would not introduce bias.
2. Missing At Random (MAR), when missingness depends only on observable, recorded variables. If an outcome variable such as participation in the follow-up interview is MAR, the non-response depends on other recorded variables such as age. In this case those who did respond to the interview can be re-weighted to represent themselves and others like them who did not respond, to reduce bias. Weights can be found using the inverse of propensity scores as discussed later.
3. Missing Not At Random (MNAR) data is missing data that depends on variables not observed or recorded. It is impossible to determine if this were present in the data set as there is no information available that explains non-response. If present there is no way, or no information, to know what to account for. If the data is MNAR bias will remain present in analyses (Lacey et al., 2013).

As will be discussed, different types of models have been explored in this analysis, each making different assumptions about the interview non-responders.

### 2.2.1 Attrition

To ensure responses to post-release interviews: 1.) collection periods were very long, especially post-release; 2.) confidentiality was provided; 3.) participants were offered a koha in the form of a $40 phone or supermarket voucher (excluding when an interview occurred in prison); and 4.) many follow-up reminders were provided. Despite this there were many complete unit non-responses to the post-release interviews.

Attrition bias could have entered the study as some participants were unreachable for the post-release interview as they were: held in a prison not approved for interviews, lacking contact details, being unwilling to continue with the research, or unable to set up and attend an interview. No matter the analytic methods used, it should be noted that there is a large loss (60% of the initial sample) in the post-release interview. Losing this much of the sample size is extremely concerning and makes the study quite susceptible to bias. Loss of participants in follow-up interviews often leads to bias,

however, the attrition bias resulting from these losses depends very strongly on which of the three main types of missing data that can occur. Losses of 5-60% can introduce little bias if the missing data is MCAR or MAR and adjusted (Kristman et al., 2003). However, when MNAR data is involved heavy biases can occur with very little loss of sample size. Attrition bias arises from MNAR data (Lacey et al., 2013) and can affect internal and external validity of the study[5].

### 2.2.2 Response Bias

Corrections provided information about the number of previous convictions and imprisonments, release date, lead offence from the sentence participants were serving at initial interview, and the number of re-sentencings and re-imprisonments within 12 months. Due to the administrative nature of the data and under the statutory framework the Department of Corrections acts, it is believed to be true and accurate. However, the chosen measures of re-sentencing and re-imprisonment have not been designed to capture sentences not administered by Corrections such as fines, or criminal activity participants were not caught for.

All other information was gathered from the research participants themselves, which could have provided measurement error. Measurement error will be present if the true nature of the attribute being measured differs from the answer recorded to represent it. The interview questions at each stage relied upon the accuracy of answers given by the research participants. Unfortunately, especially for answers to sensitive questions concerning matters such as mental health or substance use, there is often misreporting present (Morral, 2000). Misreporting could be present due to a lack of trust in the confidentiality of the research or a self-consciousness about how the answers may seem, preferring to portray themselves in a better light.

The most sensitive and prone to incorrect self-reporting were the measures of mental health and substance abuse. The Alcohol, Smoking and Substance Involvement Screening Test (ASSIST) was used to best measure drug and alcohol risk. Where risk was measured by the risk score for each substance, where the highest risk across substances was taken as overall risk. The risk scores of ‘lower’, ‘moderate’ or ‘high’ relate to the intensity of intervention that would be recommended to treat substance abuse and severity of the addiction. The ASSIST screening test (Humeniuk & World Health Organization, 2010) is recommended for use by the WHO and was requested by Corrections.

[5] Compromised internal validity means associations between study variables may not be true to the sample. Compromised external validity means our analyses may not be generalisable to the larger ex-offender population.

The Patient Health Questionnaire (PHQ-9) was used as an indicator to suggest depression ranging from 'none' to 'severe' as some people may not have had a formal diagnosis (Health Navigator NZ, 2021), while getting a better idea of how a participants mental health changed over time. However, asking for/giving a formal diagnosis would have provided more accuracy for mental illness to be accurately recorded.

Unfortunately, at this point is very difficult to identify the effect this may have had on the data available. It is impossible to tell from the data available alone the validity of the measurements over people and across interviews, this would require external validation; it is assumed the data is representative of the variables it aims to represent.

### 2.2.3 Coverage Error

The target population was people finishing their sentence in New Zealand prisons in 2019. The sampling frame was based upon lists of prisoners being released within four weeks each of the prisons to which access was allowed. First, the interviewers would choose an approximately three-week interval to attend each of the 6 prisons to which access was allowed. The facility would then provide a list of eligible people who were to be released in 4 weeks or less who interviewers were allowed access to[6]. The people on these lists were then approached and asked if they would participate in an interview as described in the Methodology section. The aim was to capture all prisoners leaving each prison during the three-week intervals, however, prisoners who were approached could and did refuse to be interviewed.

There was no coverage error of duplication in our sample, as no one appeared at more than one prison during interview periods. Furthermore, it is assumed the list provided by each facility covered all people who were eligible for the study. Because of this, it is believed the sample frame properly captured the people are leaving the prisons at the time of interviews.

### 2.2.4 Selection Bias

This study could be prone to selection bias if the prisons chosen were not representative of the whole population of New Zealand prisons. Additionally, with the element of self-selection where-in a

[6] Interviewers may have been denied access to interview prisoners at the time due to factors such as if they were in isolation at the time of interviews.

participant had to opt-in to being a part of the study. If this group of participants was not representative of the people being released from the 6 prisons in questions and to a larger extent all people being released from prisons across New Zealand, the results from the study will not necessarily be comparable. To make valid influences of the population the sample must have characteristics like that of the larger population. Excluding some prisons could have introduced bias if they had different characteristics from the people interviewed. There could also be bias introduced from those who volunteered to participate as opposed to those who did not.

The six prisons selected for the study were not chosen at random, they were selected at the beginning of the study. The prisons were decided following discussions between the Principal Investigator (PI) for the research Dr Alice Mills and the Department of Corrections. Factors involved in choosing the prisons for this research included how frequently prisoners were expected to be released from each prison and inclusivity of prisons across a range of regions across the country.

### 2.2.5 Confounding

The ideal study design for identifying causal effects would be a randomised experimental design (Rubin, 2006) where stable housing was randomly assigned to research participants. This would ensure random allocation across both measured and unmeasured variables. A study such as this would not be ethical, stable housing should not be withheld from anyone and to allocate anyone to unstable housing such as homelessness would be cruel and unusual.

This study was of a cohort design, meaning confounding variables were likely to be present. Where confounding variables related to whether participants were exposed to stable or unstable housing after release from prison, as well as being plausibly causally related to recidivism. When confounding is present the results of the analysis may not properly represent the actual association (Pourhoseingholi et al., 2012). Confounding must be properly adjusted for to identify the true effect of stable housing, not accounting for confounding can result in type I error, putting internal validity at risk (Skelly et al., 2012). The expert knowledge of the PI at the beginning of the study led to a wide range of questions being used to gather information on participants such as: experiences of housing, plans for future housing, employment/education, familial/romantic relationships, programmes/treatments for issues such as mental health and addiction, conditions given with their current sentence, mental health currently, and substance abuse. Unfortunately, it is suspected information on region and financial situation would have added a lot to the study. Gathering

information of the region a person came from and was released to (Pleggenkuhle et al., 2015; Mills et al., 2013), as well as information about their financial situation (Baldry et al., 2002; Lutze et al., 2014) would be recommended in future research as such factors are suggested to be influential in other literature.

Analysis sought to estimate the causal effects of stable housing on recidivism by adjusting for confounding throughout our analyses. Ideally, there would have been information available on age, gender, ethnicity, previous housing, housing support/programmes, social support, financial situation, mental health, substance abuse, region, criminal history, sentence length, and employment/education. However, as the size of the sample interviewed at the post-release interview is small (n = 80) comparative to the number of suspected confounding variables, two different sets of suspected confounding variables were identified. These consisted of the 'Ideal' set of adjustment variables from our data and the 'Directed A-cyclical Graph (DAG)' identified set of adjustment variables.

For each set of suspected confounding variables all interactions suggested to have relationships by bivariate tests were tested. However, they were either not significant with everything else in the model or resulted non-convergence of these models. Variables included in our chosen models were given great consideration but to the sheer number of variables thought to be possible confounders, the sample size rule of 10:1 subject to predictors suggested (Harrell, 2010) is not met even before accounting for interactions. Therefore, including interactions was not viable.

#### 2.2.5.1 Ideal Adjustment Variable Sets

The PI for the "Going straight home?" research used knowledge in the field to evaluate the confounders suspected to be the most influential. The disjunctive cause criterion for confounding by VanderWeele (Ikram, 2019) suggests including any variables in that could reasonably be expected to affect both the outcome and the treatment as confounding variables. Due to only 80 participants remaining in the post-release interview, including all possible suspected confounders was not practical. PI identified an 'Ideal' set of confounding variables thought to contain the factors most significant to stable housing after release and recidivism.

The 'Ideal' set of adjustment variables identified (Mills et al., Forthcoming) consisted of:

- Ethnicity (Māori, NZ European & Other, and Pasifika ethnicities where multiple ethnicities could be chosen);
- Age (as measured by age group);
- Gender (as measured by the binary male/female; due to there being a single gender-diverse participant, they were removed from analysis as covariate adjustment or balance across a group with one individual being impossible);
- Housing before prison (as measured by residential mobility or stable housing type in the 6 months prior to prison);
- Social support (as measured by perceived family supportiveness in the pre-release interview, plans to live with family, and previous caring responsibilities);
- Substance abuse (as estimated using ASSIST scores from prior to prison);
- Support (whether a participant had received housing support after release);
- Employment (whether a participant gained paid employment after release); and
- Criminal history (estimated by the number of previous imprisonments as supplied by corrections).

Although these were expertly chosen suspected confounding variables, there is still room for misidentification of the relationship between confounders and recidivism. Our conclusions about stable housings effects could be wrong if the confounders adjusted for were not sufficient or correct.

#### 2.2.5.1 Directed A-cyclical Graph (DAG) Adjustment Variable Sets

Variables identified based on a Directed A-cyclical Graph (DAG) included ethnicity, age group, and gender even when not identified. Stable housing measures were also included even when not statistically significant as these were the measures we were interested in estimating the effect of.

Due to the large number of variables available with potential to be confounding factors, an attempt was made to better identify a possibly smaller set of confounding variables. Where the potential confounders would be correlated with both stable housing and recidivism (Pourhoseingholi et al., 2012). Variables occurring temporally after stable housing were precluded from being potential confounders as this would suggest being a mediator instead. A mediator in this case would be a variable stable housing acted through. For example, if a variable such as being in education/training after prison occurred after stable housing, stable housing may have acted through education/training

to impact whether a participant re-offended. As the total effect of stable housing was of interest, rather than the partial effect, these variables were not accounted for.

First bivariate analysis to identify variables associated to both stable housing and recidivism. The variables tested were restricted only to those that could be thought of as potentially causative based primarily on temporality. However, instead of only focusing on variables showing evidence of a relationship with both stable housing after release and recidivism, a larger model was built portraying further identified relationships in our data. This attempts to capture the greater pathways of effects present in the data. Variables were added sequentially to the DAGs, first including potentially causative associations for recidivism and stable housing, then of potentially causative associations for all current variables in the model, then of all the added variables now present on the DAG until a complete DAG is created. The direction of the associations was largely based upon temporality, with occasional input from the PI, to choose a direction of a relationship for seemingly concurrent variables.

This 'DAG' adjustment set aims to identify all potential confounding variables specific to this study. These identified potential confounding variables are not likely to have external validity as the relationships represented are specific to our data set. Moreover, as the questionnaire and key variables were both influenced by the PI, there may not have been independence between the two sets of variables.

The DAG was created in R using the dagify() function. The adjustmentSets() function identified confounding sets to adjust for give the DAG object created, the simplest set of confounding variables was used. Both functions were from the "daggity" package in R (Textor et al., 2017).

Figure 1 shows the Directed A-Cyclical Graph for possible causality of residential mobility on recidivism. In this figure, variables with significant Fisher's exact t-test p-values (<0.1) that can be connected to either residential mobility or recidivism have been included. Arrows are included between variables with evidence of bivariate relationship, the direction of the arrows is determined by temporality. Based on this information, to estimate the total effect of residential mobility in the 6 months after prison on recidivism the following confounding variables should be adjusted for:

- Ethnicity;
- Age group;
- Gender (male/female; the single gender-diverse participant was removed as it is not possible to adjust for a group with a single observation);

- Number of previous imprisonments;
- Housing before prison (as measured by number of moves in the 6 months prior to prison sentence);
- Whether they knew where they are going to live (pre-release); and
- Whether they were in education (post-release).

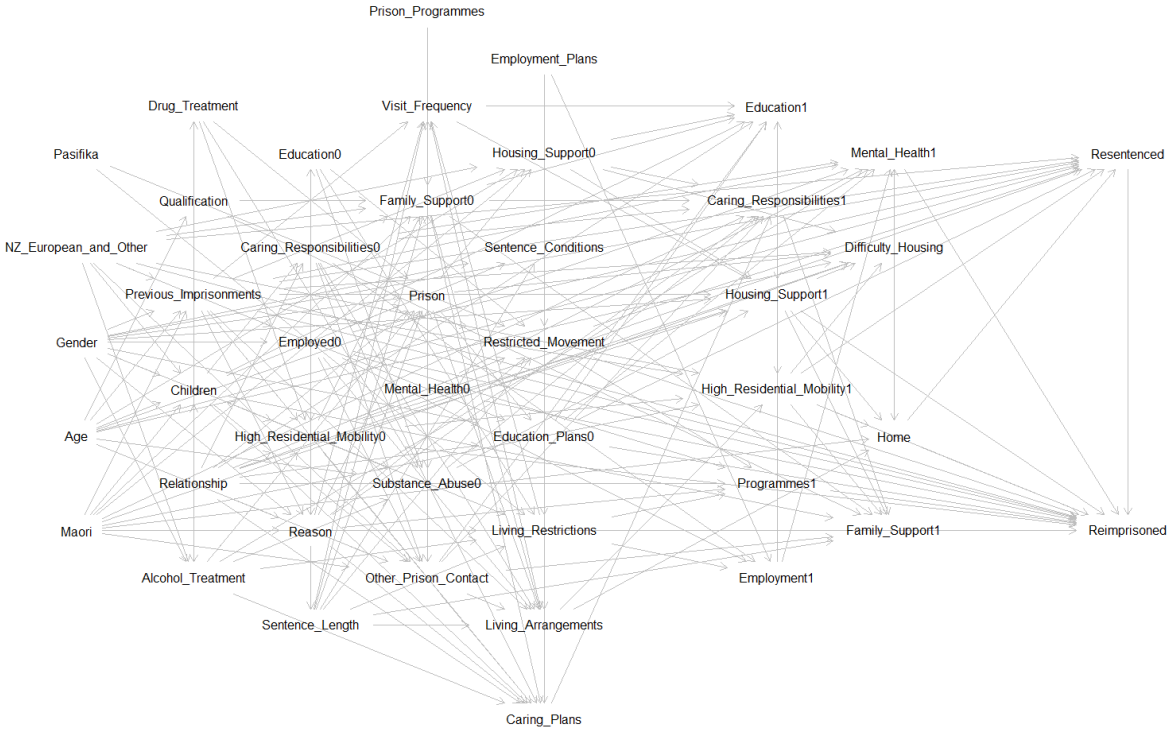


*Figure 1: Directed A cyclical Graph, with associations relevant to residential mobility and recidivism portrayed.*

Figure 2 shows the Directed A-Cyclical Graph for possible causality of housing type on recidivism. Variables with significant Fisher's exact t-test p-values (<0.1) that can be connected to either residential mobility or recidivism are shown. Arrows are included between these variables with some evidence of bivariate relationship, the direction of the arrows is determined by temporality. Based on this information, to estimate the total effect of housing type in the 6 months after prison on recidivism the following confounding variables should be adjusted for:

- Ethnicity;
- Age group;
- Gender (male/female; the single gender-diverse participant was removed as it is not possible to adjust for a group with a single observation);
- In a relationship (pre-release);

- Perceived family supportiveness (pre-release);
- Housing before prison (as measured by least stable type of housing in the 6 months prior to prison sentence);
- Whether they had unstable housing plans (pre-release);
- Whether they found paid employment (post-release); and
- Whether they received housing support (post-release).

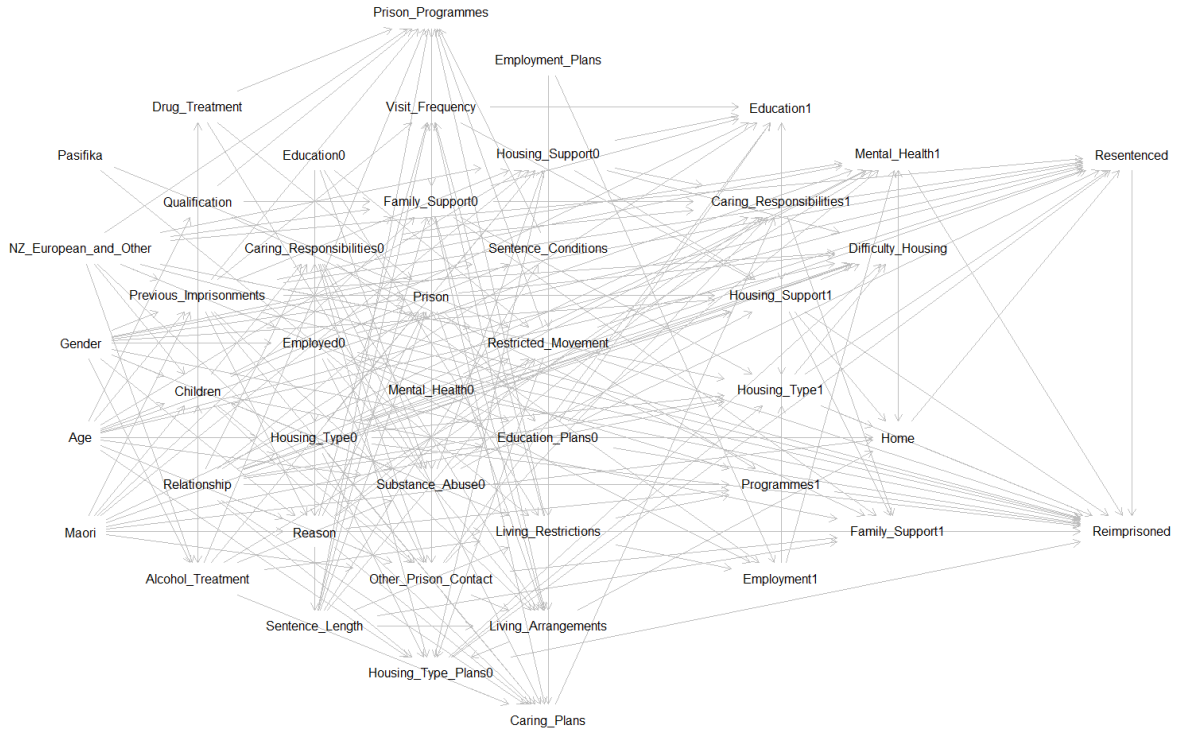


*Figure 2: Directed A cyclical Graph, with associations relevant to housing type and recidivism portrayed.*

## 2.3 Analytic Methods

The following section will go into greater detail about the analytic methods used for this analysis. All analyses were carried out using R statistical software. For our smaller sample size, a p-value of $p < 0.1$ was considered significant.

Analysis sought to estimate the causal effects of stable housing on recidivism by adjusting for confounding throughout our analyses. Confounding is adjusted for in four different ways using different assumptions:

1. Covariate adjustment: the most direct approach, using complete case analysis and excluding any missing data. The complete case covariate adjustment assumes the participants who responded to the post-release interview were representative of the entire initial sample. For this analysis to be representative of the initial sample it is assumed those who did not participate in the post-release interview 6 months after release were 'missing completely at random' to give valid estimates for all interviewees and extend to the larger population of ex-prisoners. If the people who are missing from the complete data set differ systematically from the initial sample bias will be present in the analysis.

2. Model averaging: due to the large number of variables suspected to be confounders, models including so many explanatory variables became less reliable. Although only predictors with good reasoning are included, as suggested by Grueber et al. (2011), overparameterization is still a concern as stable housing appears intertwined with many other factors. As stable housing is suspected to be intertwined with many other factors it becomes unclear which variables are truly necessary to include as confounders. Because of this, a model averaging approach was implemented to get a better sense of the effects of stable housing across all subsets of suspected models. In an ideal world there would be certainty of one model that would provide the correct causal effect of stable housing on recidivism. Instead, this approach allows averaging of parameter estimates over many possible models, so as not to rely on any one model. As it is built as an average of covariate adjusted models, participants not 'missing completely at random' will bias the results similarly to previously mentioned covariate adjustment.

3. Non-response covariate adjustment: covariate adjustment with non-response weighted data assumes observed variables affected retention in the follow-up interviews, resulting in research participants being treated as 'missing at random'. Logistic regression based on the best model to predict participation in the post-release interview is used to create propensity scores. Responders of the post-release interview are re-weighted by the inverse of their propensity score to represent themselves and the non-responders[7]. The approach assumes there is no unknown information that would identify a reason for non-response and that the non-responders could be properly represented by responders who had similar characteristics.

[7] It could have been valuable to provide a non-response weighted model that represents the characteristics of the eligible population, not just those in the pre-release sample. However, information was not recorded on the characteristics of the people who did not agree to participate in the study.

If there were unobserved variables that affected non-response this adjustment would not remove the bias.

4. Inverse Proportional Treatment Weighting (IPTW): re-weights the data set controlling for a set of confounders. Unlike the other methods IPTW does not make assumptions about what is missing from the data, instead taking the data available and re-weighting it to achieve balance of 'exposed' and 'unexposed' across confounding variables. The IPTW uses logistic regression to find the propensity score for a participant having been exposed to unstable housing based on a set of confounding variables. The inverse of the propensity score is then used to re-weight the data to have a balanced, rather than representative, distribution of 'exposed' and 'unexposed' across each confounding variable. A pseudo-population is created to represent what might be expected from a randomised trial. This approach will only be correct if all confounders have been correctly identified and balanced.

This analysis focused on the effects[8] of stable housing within approximately 6 months of release on recidivism. There were four combinations of effects analysed:

1. The effect of having lived in an unstable housing type in the 6 months after release on re-imprisonment.
2. The effect of having lived in an unstable housing type in the 6 months after release on re-sentencing.
3. The effect of having moved two or more times in the 6 months after release on re-imprisonment.
4. The effect of having moved two or more times in the 6 months after release on re-sentencing.

These will be discussed in the results section.

### 2.3.1 Bivariate Analysis

Bivariate analysis was used to indicate whether there were associations between variables in the data collected, to try to establish which variables may have significant relationships. For categorical variables like the ones collected for this research, chi-squared tests and Fisher's exact t-test were

8 When using the term 'effects' it will refer to average causal effects, this study hopes to estimate by adjusting for suspected confounding.

used. Chi-squared tests were used when possible, while fishers exact t-test were used when the table of the two variables had cell(s) that were expected to be valued 5 or less (Hess & Hess, 2017). These tests were used to aid in identifying statistically significant associations/relationships between categorical variables. These tests were carried out using the functions chisq.test() or fishers.test() in R.

Pearson's chi-squared test indicates how likely it would be to observe the values from an observed cross-tabulation of variables as opposed to what would be expected based solely on the marginal distribution of the cross-tabulation. Essentially, it is testing for independence between two categorical variables. The null hypothesis being that there is no association between the two variables, the joint distribution is simply a product of the row and column marginals. That is to say, the observed values were no different than expected by chance (Hess & Hess, 2017).

For the table below:

| | S | Non-S | Total |
|---|---|---|---|
| T | $O_{11}$ | $O_{12}$ | $\sum_j O_{1j}$ |
| Non-T | $O_{21}$ | $O_{22}$ | $\sum_j O_{2j}$ |
| Total | $\sum_i O_{i1}$ | $\sum_i O_{i2}$ | $\sum_{ij} O_{ij}$ |

The expected values are:

$$E_{ij} = \frac{\left(\sum_j O_{ij} \ * \ \sum_i O_{ij}\right)}{\sum_{ij} O_{ij}}$$

The formula to calculate the chi-squared statistic is:

$$\chi^2_{(\alpha,df)} = \sum_{i,j} \frac{(O_{ij} - E_{ij})^2}{E_{ij}}$$

Where df is the degrees of freedom:

$$df = (number\ of\ rows - 1) * (number\ of\ columns - 1)$$

and alpha ($\alpha$) is the significance level set to 5% or $\alpha = 0.05$.

Fisher's exact test may be more conservative in its estimates for smaller data sets (Hess & Hess, 2017). However, it is better designed to deal with crosstabulations resulting in low expected cell counts that would cause difficulties for the chi-squared test. The conditional probability of getting the observed matrix given the row and column totals is calculated by:

$$P = \frac{(C_1!\, C_2! \ldots C_m!)(R_1!\, R_2! \ldots R_n!)}{N! \prod_{ij} O_{ij}!}$$

Where N = $\sum_{ij} O_{ij}$, $C_j = \sum_i O_{ij}$ and $R_i = \sum_j O_{ij}$.

During initial searches for relationships many larger tables were investigated, which violated the expected cell size requirements for usual chi-squared tests. In order to loop through many calculations, the p-value was simulated with the functions chisq.test() or fishers.test() in R to avoid the need for large sample approximations. The p-values are computed from the Monte Carlo test (Hope, 1968) with 2000 replicates.

### 2.3.2 Covariate Adjustment

The first technique used to analyse whether there was any identifiable effect of stable housing on recidivism was binomial (or quasi-binomial) logistic regression with covariate adjustment. The binomial or quasi-binomial models were created using the glm() function (with family = binomial() or quasibinomial()). Binomial models in R act as complete case analysis, excluding any missing values from the data it uses to create its model. As only complete cases are used in modelling, they will only represent those who remained in the study for the post-release interview. If it is assumed the people who were lost in the post-release interviews were Missing Completely at Random (MCAR) this would still give valid estimates for all interviewees and may extend to the larger population of previously imprisoned populations.

Unfortunately, this approach discards anyone not interviewed after release without any attempt to represent them. If one answer is missing in the variables of interest to a model, all information of that participants is discarded from the already reduced data set. Because of this, where there was relevant information provided by the research participants by way of notes, answers were imputed as mentioned in the 3. Data section. This meant almost all participants who completed the post-release interview were retained in modelling.

Binomial models, which compute the log-odds, use the logit link in the below formula. Where $\beta_0, \beta_1, \ldots \beta_j$ are the coefficients, $X_1, \ldots X_j$ are the independent variables, *j* is the total number of independent variables:

$$log(\frac{p_i}{1 - p_i}) = \beta_0 + \beta_1 X_{i1} + \ldots + \beta_j X_{ij}$$

Where,

$$Prob(Observation_i) = \frac{e^{\boldsymbol{\beta X_i}}}{1 + e^{\boldsymbol{\beta X_i}}}$$

Unweighted models were created using binomial models or quasi-binomial models (Venables & Ripley, 2002). Over-dispersion is known to interfere with logistic regression and can cause statistical assumptions to go unmet, providing too small a standard error. Where the dispersion parameter ($\phi$) had a value above 1.2 a quasi-binomial model was used.

$$\phi = \frac{residual\ deviance}{residual\ degrees\ of\ freedom}$$

Multicollinearity can also cause problems in logistic regression. Multicollinearity was tested for after the models were fitted, through the Variance Inflation Factor (VIF) (Vu et al., 2015) using the function vif() from the "car" package in R (Fox & Weisberg, 2018). The variance of the $\hat{\beta}_k$ is expressed by the formula (Craney & Surles, 2002):

$$\frac{1}{1 - R_k^2}$$

Where $R_k^2$ is the coefficient of determination of the regression equation. The VIF helps detects the strength of the correlation between the explanatory variable in the model. When it is too large (>5) the estimate for our coefficients with multicollinearity become more variable. However, high VIF

values would only be removed if they were concerning for stable housing measures, not when confounder variables were indicated to have high VIF. As focus being upon estimating the causal effect of stable housing on recidivism, it did not matter if multicollinearity existed among the other coefficients as correct estimation of their effects and standard errors were not the goal of modelling (Allison, 2021).

### 2.3.3 Model Averaging

Model averaging took a weighted average of all possible subsets of an adjustment set between the full and base model. The base model used for model averaging still included adjustment for ethnicity, age group, and gender. The equally weighted models were used to average the models to incorporate how subsets of models influenced the effect of stable housing on recidivism. This allows the effects of stable housing across many models with different subsets of covariates to be accounted for rather than choosing any one model. Model averaging aims to minimise issues that could arise from many covariates and limited sample size by averaging estimated effects over the full model and its subsets. The model.sel() and model.avg() functions from the "MuMIn" package in R were used to create averaged models (Burnham & Anderson, 2002).

Testing multiple hypotheses in the form of different averaged models does not restrict analysis to one model and a combination of the best fitting or most a priori expected model estimates can be combined. Resulting in an estimate of the effect of a causal variable based upon multiple possible models, introducing less model uncertainty.

The models were all given equal weighting[9], to avoid any one model being chosen to best fit the quirks of the small data set. The Weights() function was used in order to change the weights in the model averaged object. The function model.avg() in R was used for the calculations, using quasibinomial models. Where the weights being used are $w_i$ for i different models, the estimated for coefficients ($\hat{\theta}$) based on the model averages are given by:

$$\hat{\theta} = \sum w_i * \hat{\theta}_i$$

$$1 = \sum w_i$$

[9] A set of chosen weights, which gave more weight to variables though to be of greater importance, were also tested but resulted in very similar values.

The variances given by Burnham and Anderson (2002) are:

$$Var(\hat{\theta}_i) = \sum w_i(Var(\hat{\theta}_i) - (\hat{\theta}_i - \hat{\theta})^2)$$

However, the adjusted standard error[10] by Burnham and Anderson (2002) better takes into account the weighting applied to the models:

$$\widehat{ase}(\hat{\theta}_i) = \sum_{i=1}^{R} \widehat{w_i} \sqrt{\left(\frac{t_{df_i, 1-\frac{\alpha}{2}}}{z_{1-\frac{\alpha}{2}}}\right)^2 \widehat{Var}(\hat{\theta}_i | g_i) + (\hat{\theta}_i - \hat{\bar{\theta}})^2}$$

Where the conditionality of variance upon a model is considered and there are $i$ models with a given model is denoted $g_i$. The p-values are found based on confidence intervals for each weighted-average coefficient (Grueber et al., 2011). A confidence interval that spanned zero would be insignificant.

### 2.3.4 Covariate Adjustment with Non-response Weighted Data

Although a cohort of 201 people were gathered from across the 6 prisons of interest, only 80 could be contacted and interviewed for the follow-up interview approximately 6 months after release. Non-response weighted data is created from the assumption loss of follow-up in the post-release interview was due to observed characteristic(s). That is, observed variable(s) were correlated with retention in the post-release interview, resulting in data assumed to be Missing at Random (MAR). MAR data would result in the post-release interview being systematically different from the initial pre-release interview (Seaman & White, 2011). Inverse propensity score weighting for non-responses assumes missingness does not depend on unobserved variables and is independent from the outcome (Little, 1986). It assumes the responders can be re-weighted to represent the non-responders and there is no other unknown information that would identify a reason for non-response.

Inverse propensity score weighting adjusts data from the follow-up interview to give more weight to the people who were more likely to drop out of the research. Ideally this would lead to people in

[10] This will be the standard error reported for the estimates from the averaged models.

the post-release interviews accurately representing themselves and those who were not interviewed. Inverse propensity score weighting aims to re-weight the data to represent the initial sample population.

First a binomial logistic regression model was created to predict a person from the initial sample's participation in the post-release interview. This binomial logistic regression model was created from variables with significant bivariate relationships with participation in the post-release interview. All interaction terms were also considered, though none remained in the model as significant. Variables not significant at the 0.05 level[11] were removed from the model in sequential steps. Akaike's Information Criterion and likelihood ratio tests were also consulted to ensure the chosen model was the best fit. The outcome of participation in the post-release interview was found to be significantly predicted by age, prison they were interviewed at, and whether they had restrictions to where they could go attached to their sentence.

For responders to the post-release interview, weights were calculated as the inverse of the propensity score (Wang et al., 2017; Littman et al., 2010) (estimated probability of participating in the post-release interview). Logistic regression incorporated the re-weighted data into the models using the "survey" package in R (Lumley, 2020). If the MAR assumption is not valid then the non-response bias may still be present in the analysis. If this were the case, estimates from these models would be very sensitive to model choice (Wang et al., 2017). Comparison of these models to the unweighted covariate adjustment will serve as an indication of whether/to what degree the complete case logistic regression was affected by MAR non-response.

Although some people were in prisons at the time of the approximately 6 month post-release interview, there was no evidence missingness was due to recidivism. All participants who were interviewed in prison at the time of the first post-release interview had not yet been officially re-imprisoned. No Corrections re-imprisonment sentence date occurred before their first post-release interview. Any people who could not be reached for the first post-release interview due to being in prison may have been held and not yet been given a sentence of imprisonment, which may have caused them to remain in prison for the second wave of post-release interviews. Further, the official dates recorded for judgements of recidivism appear to have occurred after the first wave of interviews for everyone in the initial sample.

[11] This is a lower threshold due to using the larger initial sample size of people in the model creation.

In cases where the weights were large, weight truncation was used to limit the amount of people one research participant could represent no more than five people. If the model correctly identified non-response this could have introduced bias, however, if this were not the correct model truncation is sensible (Seaman & White, 2011). If one person were given too large a weight, they would have been overly represented while not adding much information, potentially introducing bias re-weighting aimed to reduce. However, if one person did accurately represent many then this would have reduced bias. With a sample size of 80 people it was thought allowing one person to represent more than 5 in the data would not be ideal. Then binomial or quasi-binomial models were fitted with adjustment for the identified confounding variables.

For this method it is assumed the data were MAR, if this were not the case then this analysis may not have adjusted for non-response bias (Littman et al., 2010). If information was not available for factors that affected non-response, the bias will not have been corrected for. Unfortunately, missingness could depend on factors occurring after release there is no available information on. Moreover, the sample retained – even with re-weighting – may not be representative of people who did not participate in the post-release interview. Participants lost may include those who were doing incredibly well, did not wish to be reminded of their time in prison, or were doing incredibly badly.

The "survey" package in R (Lumley, 2010) was used to apply weights to the data using the svydesign() function and incorporate weights in analysis. The svyglm() function was used for logistic regression incorporating weighted data (Lumley & Scott, 2017).

### 2.3.5 Inverse Probability Treatment Weighting (IPTW)

Inverse Probability Treatment Weighting (IPTW) re-weights the data set based on a set of confounders. IPTW uses the inverse of the propensity score to re-weight the data to have an even spread of those exposed to unstable housing and those exposed only to stable housing, across confounding variables. After re-weighting the distribution of those exposed and unexposed to unstable housing should be similar across confounders, representing what might be expected from a randomised trial.

For valid causal inference the following assumptions should be met (Hernán & Robins, 2020):

1. Exchangeability: no unmeasured confounders. All known confounders should be used to create IPTWs. However, if region, financial situation, or some other factors(s) are true confounder this method may not provide valid results;
2. Positivity: Probability of getting treatment (stable housing) or not is non-zero. This is assumed to be true for our study as it is foreseeable stable housing could be achieved for everyone and conversely anyone could fall on unstable times with regards to housing; and
3. Correctness of the model: the logistic regression model used to create propensity scores was correctly specified. As there are issues of power surrounding the number of variables literature suggests including in a model, the 'Ideal' and 'DAG' suspected confounding sets were created to capture the most important confounding. Due to uncertainty, these multiple models were used to see the estimated effects under different model specifications.

The interest of this analysis is the effect of the exposure to unstable housing on the outcome of recidivism. As each person in our study is restricted to representing either a stable or unstable housing living, the effect of exposure can be observed as a comparison between people with similar characteristics who had the treatment and those who did not. Like non-response weighting, a binomial logistic regression model is used to create a propensity score for an outcome of exposure to unstable housing – where unstable housing is measured by housing type or high residential mobility – as predicted by a set of confounding variables. Weights for the data were then estimated as $\frac{1}{Propensity\ Score}$ if exposed and $\frac{1}{1-Propensity\ Score}$ if not. In cases where the weights were large, weight truncation was used to limit the amount of people one research participant could represent to five. The "survey" package in R (Lumley, 2020) was used to apply weights to the data using the svydesign() function and incorporate weights in analysis.

Due to the small sample size for the relatively large number of variables to be balanced across, Standard Mean Differences (SMDs) of 0.2 or below will be considered a good balance across covariates between exposed and unexposed variables (Moik et al., 2019). As this balance was not achieved across some covariates, propensity scores were restricted to the overlapping propensity scores in both the exposed and unexposed groups (Stürmer et al., 2014; Thomas et al., 2020). This retains only individuals who, given the confounding variables adjusted for, were predicted to have similar likelihoods of being exposed to unstable housing.

The IPTW method may give biased results when the balance of the covariates across treatment is not balanced even after re-weighting. When there was no way to satisfactorily re-weight the data set

further analysis was not conducted. When the covariates could be satisfactorily re-weighted the svyglm() function from the "survey" package (Lumley, 2020) was used with binomial models to compute the relative risks using the log link shown below:

$$log(\mathrm{p_i}) = \beta_0 + \beta_1 X_1 + \ldots + \beta_j$$

Where,

$$Prob(Observation_i) = e^{\boldsymbol{\beta X}}$$

# 3. Data

The final data set contained both data supplied by corrections and information collected from 3 waves of interviews. The interviews consisted of one pre-release interview and two post-release interviews. In the pre-release interview prisoners with less than 4 weeks remaining on their sentences were interviewed. The post-release waves of interviews were planned to be held approximately 6 and 12 months after release from prison.

Corrections supplied official data on: the number of previous imprisonments and convictions, the offence type leading to the participants last sentence, actual release dates, the number of re-sentences and re-imprisonments within 12 months of release, and outcome start date. Of the participants who were interviewed approximately 6 months after release none had been officially re-sentenced before these first post-release interviews.

The data from interviews was collected from 6 different locations. There were 203 people in the initial pre-release interviews. Unfortunately, information was not recorded on how many people were eligible for the study, or the characteristics of the people who either were not accessible at the prison (due to such circumstances as being in solitary confinement) or decided against participating. However, two people withdrew from the study. Of those n = 201 who remained in the study 80 people were retained in the first wave of post-release interviews representing 40% of those in the pre-release sample, 14 of which were in prison and 66 from out of prison. For the second wave of post-release interviews, 66 people were interviewed representing 33% of those in the pre-release sample, 13 in prison and 53 out of prison.

Not only was there a large drop off in interviewees in the post-release interviews, but the participants were also reached inconsistently as seen in Figure 3. Not all the people in the first wave of follow up interviews were in the second wave of follow up interviews. In fact, a large movement can be seen between participation and a lack of contact and vice versa. Only 44 participants were interviewed in both post release interviews. Of the sample, 102 participants or about 51% of the sample were interviewed at either first or second wave of post-release interviews. People who were not released from prison after being interviewed were not interviewed again at the second wave of post-release interviews (Figure 3), as they were unable to provide any new information about stable housing and integration into the community.

Retention of the rates seen in this study were not entirely unexpected compared to studies done internationally. Similar studies retained 34-70% of their original samples (Baldry et al., 2006; Brunton-Smith & Hopkins, 2013; Carlisle, 1996), where studies with shorter time periods between release and the first post-release interview retained participants in greater numbers.

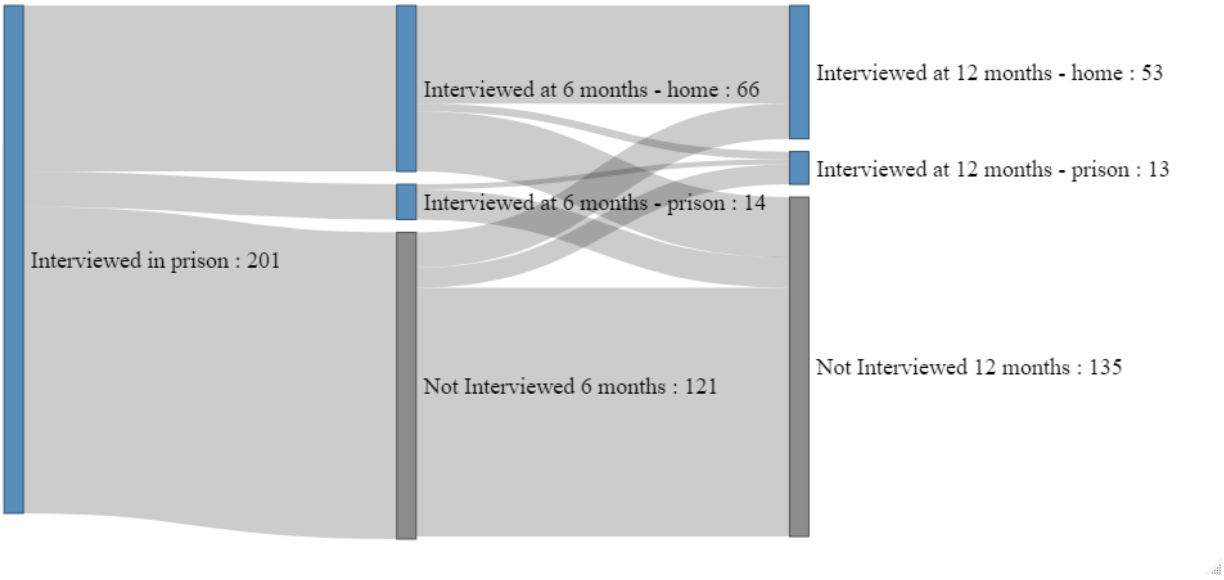


*Figure 3: Sankey flow chart of which interviews participants took part in.*

Our research participants were released between 17th of March 2019 and 4th December 2019. The first wave of post-release interviews were held between 14th October 2019 and 28th May 2020. These were interrupted/delayed by the COVID-19 lockdowns as can be seen in Figure 4. The first wave of interviews was supposed to be held 6 months or about 180 days after release. In actuality the first wave of follow up interviews occurred between 142 to 341 days after their prison releases as can be seen from Table 1[12].

*Table 1: Days to 6 month interview.*

| Min | Lower quartile | Median | Mean | Upper quartile | Max |
|---|---|---|---|---|---|
| 142 | 190 | 209 | 215 | 230 | 341 |

The second post-release interviews were held between the 28th of May 2020 and the 21st of February 2021. The second wave of post-release interviews managed to reach only 33% of the original sample. Although the second wave of interviews began before the first wave of interviews had

[12] The end of the first post-release interview period was quite affected by COVID-19 as Level 4 lockdown happened during initial interviewing in the Hawkes Bay region. Although some phone interviews were undertaken during lockdown, the progress was limited and contact was increasingly difficult.

concluded, the number of days between first and second follow up interviews ranged from 100 to 354 days. The interviews were supposed to occur approximately 12 months after release, however, in reality they occurred 344 to 588 days after release (Table 2).

*Table 2: Days to 12 month interview.*

| Min | Lower quartile | Median | Mean | Upper quartile | Max |
|---|---|---|---|---|---|
| 344 | 427 | 453 | 466 | 512 | 588 |

A visualisation of events over time is displayed in Figure 4 below. There is a particular focus placed on the Auckland COVID-19 lockdowns as this is where our researchers were based.

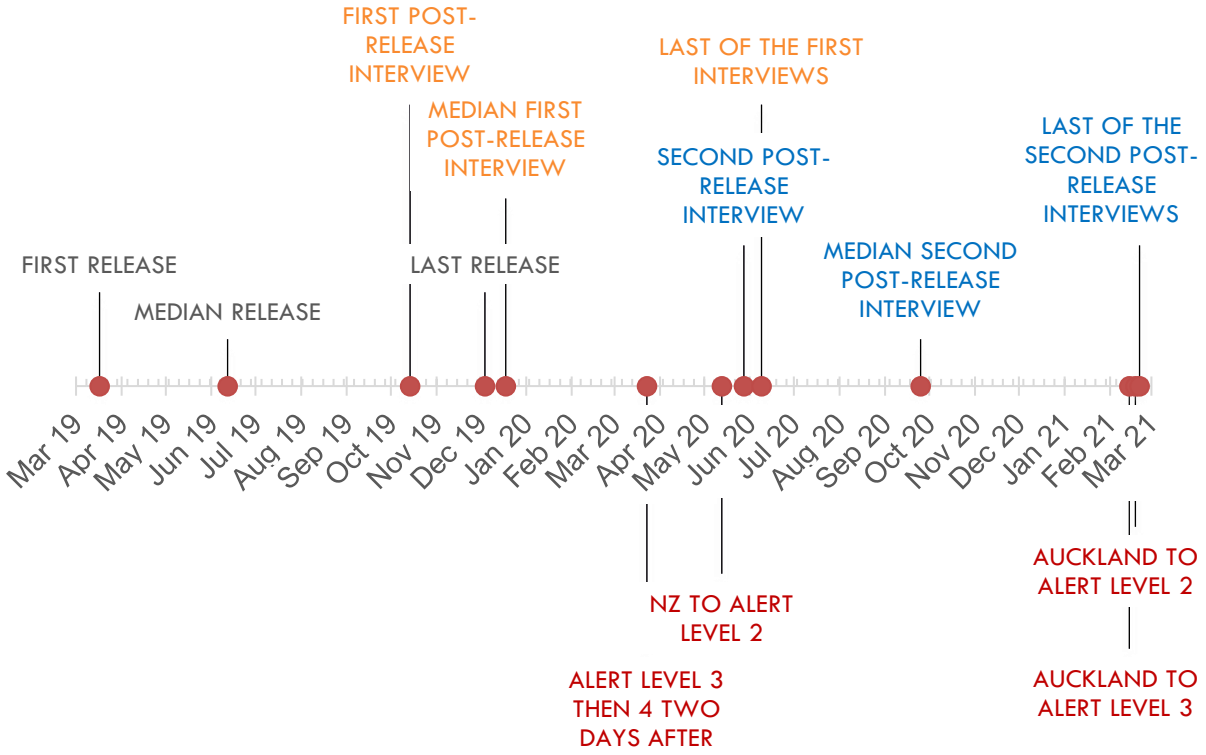


*Figure 4: Timeline of the interview periods and the impact of COVID-19 on interview periods.*

## 3.1 Data Cleaning

The sample started with 203 research participants. However, as two people withdrew from the study these people have not been included in any tables, graphs, or statistics in this thesis.

The reoffence measures provided by Corrections included the number of times a participant was re-sentenced or re-imprisoned within 12 months of release. This information was used to create a binary variable indicating whether a participant was re-sentenced or re-imprisoned within 12 months.

Participants were asked about the type(s) of accommodation they were living in the 6 months before prison in their pre-release interview and in the 6 months prior to their post-release interviews (before any returns to prison) after release. As they could have lived in more than one type of accommodation during these 6 month periods they could choose multiple types of accommodation to reflect this. However, for analysis, one variable was created and the least stable of their accommodations was recorded as their type of accommodation over the 6 month period. This does properly consider participants who had a reduction in their living arrangements in the 6 months but does not capture improvements in living arrangements well. Participants who lived in multiple accommodations were coded as living in the most unstable housing type they had lived in within 6 months of release, capturing the most difficult living situations. Accommodation type options given were: 'owned', 'rented', 'state housing', 'Housing First/NGO Provider', 'with friends/family', 'hotel', 'in a hostel/boarding house/backpackers/campground/holiday park', 'prison', 'on the street', and 'elsewhere'.

If a participant answered they were living 'elsewhere' to these questions interviewers would add in discussion notes to specify where these people were living. These were then categorized into the type of accommodation most closely matched to their description. This was done with input from supervisor Dr Barry Milne and PI, Dr Alice Mills to ensure living descriptions matched the type of accommodation. However, when participants were asked for their planned living arrangements after release, those who answered "elsewhere" were not often able to be assigned to a more accurate housing type due to lack of information about where exactly they planned to live after release from prison. As the measurement of planned housing is less exact than measurements of actual housing type lived in, housing experiences have been focussed on for this study rather than plans.

Stable housing types were defined as: 'owned', 'rented', 'state housing', or 'Housing First/NGO Provider'. Accommodation supplied by family and friends is not included as stable due to often being temporary in nature. This was supported by over half of those staying in accommodation with friends and family having moved at least two times before their prison sentence.

Residential mobility was measured by the number of times a participant had moved in an approximately 6 month period before the prison sentence and in the approximately 6 months before

each post-release wave of interviews. This was asked as an open question so the answers could be quite vague, many would answer they had ‘no fixed address’ or did not remember the number of times they moved – as they had moved so often. Following the precedent set by Baldry et al. (2006), moving at most one time in a 6 month period was considered stable housing as it involves low residential mobility, moving at least two times was considered high residential mobility or unstable housing. Those who answered they were “homeless” or had “no fixed address” were counted as having at least two moves. Some did not answer the question directly, however, interviewers often gathered enough information about a respondent’s housing situations for their number of moves to be extrapolated. Where notes on the number of moves were less easily interpreted, their classifications as high or low residential mobility were checked by the PI. Some participants who answered they were living ‘on the streets’ would claim they had not moved despite living in cars, tents or on the street. Participants who answered they were living ‘on the streets’ were recoded as having moved at least two times to better capture the highly transitory nature of someone who may not be able to return to the same spot each night.

Due to the heavy reliance on models not allowing for variables to change over time, the age group of a person in their pre-release interview was the measurement of age used throughout. Age groups were used rather than continuous age measurements due to the short period of interest and the specific interest in the younger and older age groups. The largest number of people were recorded as being between 20 to 29 years of age. As the original age groups (‘under 20’, ‘20-29’, ‘30-39’, ‘40-49’, ‘50-59’, or ‘60+’) had too few people, especially in the oldest and youngest categories, the categories were grouped further (‘under 30’, ‘30-39’, ‘40-49’, or ‘50 or over’).

Ethnicity allowed choice of identifying as more than one ethnicity, including many categories and an option for self-specification. In this paper only the most prominent ethnicities were used, ‘Pasifika’ (Samoan, Cook island Māori, Tongan, Niuean, or Fijian), ‘Māori’ (New Zealand Māori) and ‘New Zealand European and other’ (New Zealand European, Other European, Chinese, Indian, Korean, Filipino, or Malaysian). Ethnicities remained as separate variables rather than being grouped into prioritized ethnic groups.

Marital group options were originally asked with many possible relationship types to choose from, 5 declined to answered. The decision was made not to try to allocate the unknown relationship statuses to a category based on other answers as it seems their relationships were quite confusing and unstable. The marital status was grouped as: in a relationship (‘de facto’, ‘married’, or ‘in a civil union’), and not in a relationship (‘divorced’, ‘widowed’, ‘separated’, or ‘single, never married).

Family supportiveness was recorded as how supportive the participants felt their families were of them. Some other researchers have chosen to use visits with family members to estimate how supportive families were such as Brunton-Smith & Hopkins (2013). However, a Likert scale to measure perceived family supportiveness from "very supportive" to "very unsupportive" (where "no contact" was also an option) was included. It was believed families who perhaps cannot visit due to participants not wishing for family to see them or the costs of travelling to visit the person in prison may not adequately capture how supportive a family truly is. Despite participants who thought their families were 'supportive' or 'very supportive' being more likely than others to receive visits, only 34% received visits in prison. This could suggest family supportiveness is not as associated as assumed on being visited in prison, or that visiting a person in prison has other hidden difficulties perhaps financially, emotionally, or due to time constraints.

Sentence length was based off the question "What is the full term of your current sentence?". This allowed room for error as it was what was remembered and reported by the participants rather than formal record. Moreover, a participant may have answered with the time served rather than the total length of the sentence given. This was asked as an open question receiving a large array of answers. They were grouped into the categories: 'less than a year', '1-2 years', or 'more than 2 years'.

An Alcohol and Substance Involvement Screening Test (ASSIST) was applied to get a better idea of substance addition/abuse risks of the participants (World Health Organisation, 2010). Where the ASSIST score was calculated for each type of drug and the most severe score was recorded as the overall drug score as McNeely et al. (2014) chose to do. The recorded risks ranged from 'none' to 'high' risk. Finally, to get an overall substance risk the highest of the alcohol and the drug risk range was used, say a participant had a 'Moderate' alcohol risk and a 'Lower' drug risk, a 'Moderate' substance abuse risk would be recorded overall.

The PHQ9 scores relate to participants answers over the 2 weeks before interview. It is designed to get a sense of mental states though it does not identify or diagnose mental illness. It is a questionnaire (Health Navigator NZ, 2021) consisting of the nine questions below:

1. In the last couple of weeks, how often have you been bothered by feeling little interest or pleasure in doing things?
2. And how often have you been bothered by feeling down, depressed, or hopeless?
3. How about trouble falling asleep, or staying asleep or sleeping too much – how often have you been bothered by those?

4. And how often have you been bothered by feeling tired, or having little energy?
5. How often have you been bothered by a poor appetite, or found yourself overeating?
6. How often have you been bothered by just feeling bad about yourself? For instance feeling that you are a failure or have let yourself or your family down.
7. How about focus? How often have you been bothered by having difficulty concentrating on things, say reading the newspaper or watching television?
8. How often have you been bothered when you found yourself moving or speaking so slowly that other people could have noticed a difference, or the opposite – being so fidgety and restless that you were moving around a lot more than usual?
9. Finally, in the last two weeks, how often have you been bothered by thinking that you would be better off dead, or thinking of hurting yourself in some way?

Each of these questions requires an answer as one of the following:

- Not at all (worth 0 points)
- Several days (worth 1 point)
- More than half the days (worth 2 points)
- Nearly every day (worth 3 points)

Participants were required to have answered at least 6 of the 9 questions in the PHQ-9 questionnaire, otherwise their overall answer was listed as missing. If they had answered at least 6 but less than 9 questions a prorated value was created. In the prorated value the missing answers were replaced with the average of their other answers (Mazza et al., 2015) in an attempt to gauge what their mental health might be like. The PHQ-9 score returns a value between 0 to 27 to indicates possible depression. A score from 10-14 indicates ‘mild’ depression, 15-19 indicates ‘moderate’ depression, and a score of 20 or higher indicates ‘severe’ depression. Mental health measures the mental health specifically asking about the last two weeks so it should be used as an indicator of mental health in the weeks leading up to the interview rather than overall throughout their lifetimes.

## 3.2 Restricting Information Used

The total sample is represented in the pre-release interview (time 0), of the sample 51 (25%) were re-imprisoned and 92 (46%) were re-sentenced within a year. In the first post-release interview (time 1) 80 people (40%) from the total sample were represented, similarly to the total sample, 20 (25%) were re-imprisoned and 32 (40%) were re-sentenced within a year. In the second post-release

interview (time 2) 33% (66 people) of total sample is represented and shows a bias against people who reoffended being found and interviewed in the second post-release interview. In the second post-release interview only 15% (10 people) were re-imprisoned and 36% (24 people) were re-sentenced within a year, as shown in Figure 5 and Figure 6 displayed below[13].

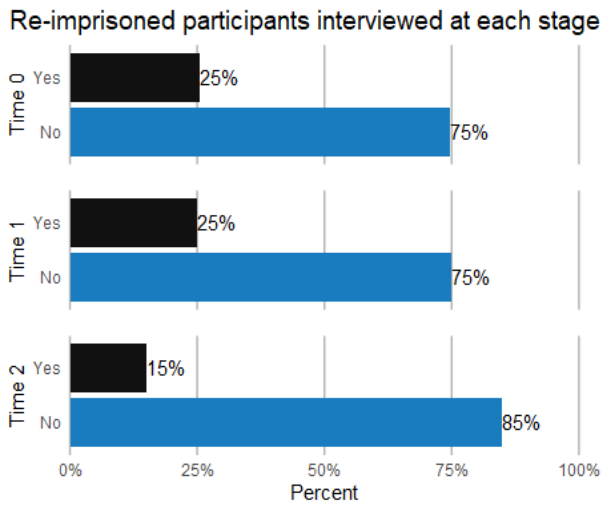


*Figure 6: Percentage of participants involved in each wave of interviews who were re-imprisoned.*

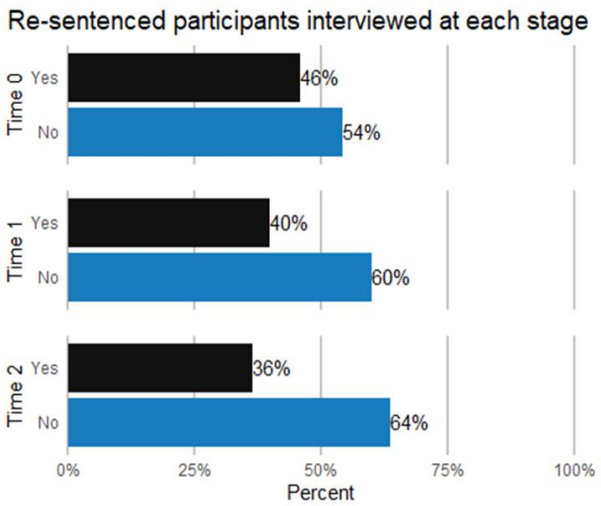


*Figure 5: Percentage of participants involved in each wave of interviews who were re-sentenced.*

Unfortunately, although most subpopulations were retained in similar proportions in later interviews, those who were re-sentenced or re-imprisoned within a year were not able to be retained well in the second wave of post-release interviews. The first post-release interview captured a reasonably similar percentage of people who had, or would, reoffend within a year of release. However, the second post-release interview did not manage to represent similar amounts of participants who were, had been, or were about to be, re-imprisoned or re-sentenced. There is a significant negative relationship (chi-squared test p-value of 0.031) between being re-imprisoned and participating in second wave of post-release interviews. A weakly significant relationship (chi-squared test p-value of 0.085) is also present between being re-sentenced and participating in the second post-release wave of interviews. This will be partly due to participants who were interviewed in prison in the first wave of post-release interviews not being tracked down for the second post-release interviews if they had not been released between interview periods. These participants who had not been released were not sought out as they had not left prisons and as such could not add insight into any new housing experiences. This could also have been due to people simply being

[13]Time 0, displays the percentage of all participants in the original sample who were re-sentenced or re-imprisoned within a year. Time 1 or 2 show the percentages of people in the first or second wave of post-release interviews respectively that were re-sentenced or re-imprisoned within a year.

harder to reach before they did reoffend, or unreachable due to reoffending – perhaps being held or re-imprisoned in a prison not of the six accessible to interviewers. Although none of the times supplied by Corrections as the date of re-sentencing/re-imprisonment occurred before any person's first post-release interview, many official dates for re-sentencing or re-imprisonment occurred before the second wave of interviews. Whatever the cause, the sample becomes biased with informative attrition prior to the second wave of interviews as it has been unable to adequately capture the reoffending population of interest.

As there was a relationship between who had been interviewed in the second wave of interviews and reoffence, this research focuses on whether participants had stable housing in the approximately 6 months post-release. This means the focus will be on the association between recidivism and housing in the time between release from prison and the first post-release interview.

Another reason in support of using only the information on stable housing on the approximately 6 months post-release is the avoidance of reverse causality. Reverse causality is when the exposure could cause the outcome which in turn affects the future exposure as passing in and out of prison would affect stable housing, which is in turn expected to affect future reoffence. This study aims to identify the effect of housing after finishing a sentence. Identifying any effect of stable housing after release becomes difficult if participants are moving in and out of prison.

## 3.3 Descriptive Statistics

*Table 3: Descriptive statistics for demographics and other variables.*

| **Demographic/other variables** | **Pre-release interview Count (percentage%[14])** | **First post-release interview Count (percentage%)** |
|---|---|---|
| **Gender** | | |
| Female | 34 (17%) | 13 (16%) |
| Gender Diverse/Transgender[15] | 2 (1%) | 1 (1%) |
| Male | 165 (82%) | 66 (83%) |
| **Ethnicity[16]** | | |
| New Zealand Māori | 150 (75%) | 57 (71%) |
| New Zealand European and Other | 75 (37%) | 33 (41%) |
| Pasifika | 11 (5%) | 3 (4%) |
| **Age** | | |

[14] Percentages may not add to 100% due to rounding.
[15] As there were so few gender diverse/transgender participants, they will not be reported in any other tables.
[16]The Ethnicity category does not add to 100% since participants could belong to more than one ethnic group.

| | | |
|---|---|---|
| Under 30 | 74 (37%) | 25 (31%) |
| 30–39 | 64 (32%) | 19 (24%) |
| 40–49 | 38 (19%) | 21 (26%) |
| 50 or over | 25 (12%) | 15 (19%) |
| **Length of original sentence** | | |
| Less than a year | 88 (44%) | 34 (43%) |
| One to two years | 85 (42%) | 35 (44%) |
| Over two years | 28 (14%) | 11 (14%) |
| **Connection to others** | | |
| In a relationship with a partner in pre-release interview | 44 (22%) | 16 (20%) |
| Has child(ren) | 153 (76%) | 63 (79%) |
| Was visited at least once whilst in prison | 53 (26%) | 23 (29%) |
| Perceived their family as 'supportive' or 'very supportive' whilst in prison | 147 (73%) | 61 (76%) |
| Cared for someone before prison[17] | 73 (36%) | 27 (34%) |
| **Employment & education** | | |
| Was in paid employment prior to prison | 95 (47%) | 38 (48%) |
| Was enrolled in education prior to prison | 32 (16%) | 14 (18%) |
| **Mental health (PHQ-9 scores)** | | |
| Evidence of no or mild depression at time of pre-release interview | 162 (81%) | 63 (79%) |
| Evidence of moderate depression at time of pre-release interview | 19 (9%) | 7 (9%) |
| Evidence of severe or moderately severe depression at time of pre-release interview | 17 (8%) | 9 (11%) |
| **Substance abuse (ASSIST) risk score prior to prison[18]** | | |
| Unknown | 4 (2%) | 1 (1%) |
| Lower risk | 25 (12%) | 10 (13%) |
| Moderate risk | 112 (56%) | 46 (58%) |
| High risk | 60 (30%) | 23 (29%) |
| **Treatments/programmes** | | |
| Received alcohol treatment prior to this imprisonment | 63 (31%) | 30 (38%) |
| Received drug treatment prior to this imprisonment | 70 (35%) | 34 (43%) |
| Completed a programme in prison (including offending behaviour and drug and alcohol awareness courses) | 110 (55%) | 51 (64%) |
| **Recidivism** | | |
| Re-sentenced within 12 months of release | 92 (46%) | 32 (40%) |
| Re-imprisoned within 12 months of release | 51 (25%) | 20 (25%) |

**Gender** – participants were asked what gender they identified as: "Female", "Male", or "Gender diverse/Transgender" (where the "Gender diverse" option was expanded to include transgender

[17] Research participants were asked if they cared for someone else before prison, whether they planned to care for people after prison and if they were caring for someone in the 6 months before each of the following waves of post release interviews. They would be asked to specify who they cared for out of their parents, spouse, other whānau members, someone else, children, someone else's children.

[18] Where risk as a measure for the risks arising from substance abuse (Humeniuk & World Health Organization, 2010). Four participants did not answer enough questions to have a score.

based on other answers). There was slightly better retention of males in the interviews than females, although this was not a large disparity. Overall, the ratio of males and females interviewed at each time point were very similar. Only one "Gender diverse/Transgender" identifying person was interviewed post-release, as creates unbalanced analytic tables across this category as n = 1, the people who identified as "Gender diverse/Transgender" were removed for analysis.

**Ethnicity** – the interview questions allowed choices of more than one ethnicity, including self-specification. More than one ethnicity can be selected in the questionnaire, because of this the percentages in tables will not sum to 100% due to double counting. The ethnic make-up of the post-release interviews was very similar to that of the initial pre-release prison interview.

**Age groups** – Those who were 40 and over were much more likely to be interviewed in the post-release interviews, while the under 40 ages were less likely to be found/included in interviews post-release. The few under 20-year-old ex-prisoners interviewed in the pre-release interview were not retained in post-release interviews.

**Sentence length** – based off the question "What is the full term of your current sentence?" this question allowed room for error as sentence length was self-reported by the participants rather than from a formal record. Self-reported longer and shorter sentences were retained in the study in very similar proportions.

**Criminal history** – as measured by either of the number of previous imprisonments (Table 5) or previous convictions (Table 4). Corrections supplied official data on the number of previous imprisonments and convictions for research participants. The study managed to keep a similar range of criminal histories throughout the study. Retaining both those who have a short and long relationships with crime.

*Table 4: Number of previous sentences.*

| Minimum | Median | Mean | Maximum |
|---|---|---|---|
| 0 | 39 | 49 | 250 |

*Table 5: Number of previous imprisonments.*

| Minimum | Median | Mean | Maximum |
|---|---|---|---|
| 0 | 2 | 5 | 59 |

**Relationship status** – relationship status in the pre-release interview was split into the categories: in a relationship (which included 'de facto', 'married', and 'civil union' relationships), not in a relationship (which included 'widowed', 'separated', and 'single, never married' relationships), and five 'unknown' relationships for those who declined to answered. Those who were in a relationship at their first interview while in prison were not retained quite as well as those who were not, although the difference is not significant.

**Children** – participants were asked whether they had children, then how many children they had if they answered in the affirmative. Those who answered they had children in their pre-release interview were retained in relatively similar proportions as those who did not have children.

**Frequency of visits** – was coded from answers to the open-ended question of how often participants received visits. They were grouped into the categories: 'frequent' (fortnightly or weekly), 'Infrequent' (less often than fortnightly), or 'None'. People who received visit(s) were not retained in significantly different numbers to those who did not.

**Who they were living with** – research participants were asked who they lived with, out of: parents, spouses, other whānau members, guests, children, alone, or with non-whānau before prison. This question was also asked of who they planned on living with on release, as well as who they had lived with prior to each wave of post-release interviews. Where more than one option could be selected, this resulted in confusion as participants would on occasion claim to be living alone as well as with a spouse. The people they were planning on living with after release with did not show as significantly predictive of participation in the first post-release interview.

**Family supportiveness** – participants answered in pre-release and post-release interviews how supportive they felt their families were of them, on a likert scale ranging from 'very supportive' to 'very unsupportive'. This is a very subjective question, which could change very quickly. Especially with possibly difficult and conflicting relationships after release. Those with unsupportive families or neither supportive nor unsupportive families were less likely to be retained in the study, while those who believed they had supportive families in the pre-release interview were slightly more likely to be retained in later interviews. Those who had no contact with their families in the pre-release interview were more likely to be involved in the second follow up interview. However, these differences are comparatively small, with no evidence of statistical significance.

**Other contact** – participants were asked if they had any contact with family or friends other than visits, such as letters and phone calls. Participants who had other contact while in prison were retained in very similar proportions to those who did not.

**Highest qualifications** – qualification groups chosen to try to signify their education level. The options were: no formal qualification, school level education (consisting of 'School Certificate, National Certificate, Level 1, NCEA Level 1', 'Sixth Form Certificate, National Certificate Level 2, NCEA Level 2', or 'Higher School/Leaving Certificate, Bursary/Scholarship, NCEA Level 3'), post school level education (consisting of 'Trade or Professional Certificate', 'Undergraduate university degree', 'Postgraduate or higher qualification', or 'Diploma below degree level'), or unknown. Participants with higher qualifications were slightly more likely to be retained in the first post-release interview, though this difference was not significant.

**Employment** – participants were asked if they were in paid employment before prison as well as their plans for employment after prison then finally about whether they secured paid employment in their pre-release interview. Participants who had employment before prison or planned on getting employment after prison were present in relatively similar proportions throughout the study. There was a slight tendency to retain people who had employment before their prison sentence and those who wanted to pursue work after their prison sentence.

**Education** – participants were asked if they were in education before prison as well as their plans for education after prison in their pre-release interview. They were then asked again about their actual undertaken education after release in the post-release interviews.

**Mental health** – a measure of the mental state of a person as judged by Patient Health Questionnaire (PHQ-9) scores (Health Navigator NZ, 2021). PHQ-9 scores ask participants to answer questions relating to their mental health over the 2 weeks before an interview. It is designed to screen for the presence and severity of depression, though it does not identify or diagnose mental illness. The mental health as measured in the pre-release interview seemed relatively ineffective in predicting who may be retained in the post-release interview.

**Substance abuse ASSIST score** – this was a score with possible values ranging from none to high risk as previously specified. The risk score was created from the seriousness of answers to multiple questions about alcohol/drug use over the course of their lifetime and in the most recent 3 months where they might have had 'normal' access to them (not including as prescribed as a doctor). Those

who answered questions showing less substance dependence/risk in the pre-release interview were better retained than those with high substance dependence/craving. Although, this was not significant and there was still reasonable retention of those who endorsed the most boxes for high substance risk.

**Alcohol/Drug treatment** – pre-release the participants were asked whether they had ever participated in a drug or alcohol treatment program. Participants who had done a substance treatment program were more likely to be retained in post-release interviews, though it does not remain significant when age group is taken into account.

**Programmes** – the trend of alcohol/drug treatments being an indicator of participation in post-release interviews is mirrored for participants involved in programmes while in prison. Though it does not remain significant when age group is taken into account. It could be these people are the most likely to engage in not just programmes, but projects such as this research.

**Residential mobility** – the people who moved around frequently and infrequently before their prison sentence were both retained in very similar numbers in follow up interviews.

**Housing type** – people who had inhabited all housing types before their prison sentence were all quite well retained. However, stable forms of housing (owned, rented, states housing or NGO/housing first housing) were better retained in subsequent interviews. This was likely due to being more easily contacted. People who had been living with friends and family appeared to be slightly harder to retain.

## 3.4 Housing Support

**Housing support** – whether or not participants had been offered housing support prior to release and again post-release participants were asked if they actually received support and where the offer came from (Probation/Corrections, NGOs, family/whānau/friends or other sources).

In the pre-release interview 59% of participants answered they had not been offered housing support. The most explicit of these was at Spring Hill (a women's prison) where none of the people who participated in the study answered they had been offered housing support (Appendix, Table 81). However, this may be expected as all ten people interviewed at Spring Hill serving short-term

sentences. Unfortunately, this trend continued in the first post-release interview, while 75% of women interviewed had not been offered any housing support after prison (Appendix, Table 82), 25% reported not needing any support. Comparatively, post-release 33% of men received housing support.

There is significant evidence to suggest an association between whether support was received after prison and re-imprisonment (Fisher's exact test p-value = 0.029). The association essentially suggests not needing support is the best prevention for re-imprisonment (Appendix, Table 83).

## 3.5 Amount of the Prison Population Involved

The prison with the largest number of participants was Hawke's Bay where 7% of long-term prisoners and 12% of short-term prisoners released from the prison in 2019 agreed to participate in the study. The prison where the least prisoners were being released and agreed to participate in the study were from Spring Hill, although the PI remembered only a few people declining to participate, suggesting an unfortunate time period with relatively few releases may have been to blame. Of the 2,778 people with short-term sentences who left the prisons of interest in 2019, 6% became involved in our study. However, only 3% of people with long-term sentences who left the prisons of interest in 2019, 1,100 people, became involved in our study. This should be about in line with the amount of time spent at each prison, as 3 weeks is almost 6% of the year.

*Table 6: Percentage of long-term sentences released in 2019 involved in the study.*

| | Percentage of people being released from long-term sentences in the 2019 calendar year who became involved in the study |
|---|---|
| Auckland Women's | 2% of 119 total released from this prison |
| Christchurch Men's | 2% of 204 total released from this prison |
| Hawke's Bay | 7% of 198 total released from this prison |
| Northland | 2% of 165 total released from this prison |
| Spring Hill | 0% of 255 total released from this prison |
| Waikeria | 2% of 159 total released from this prison |

*Table 7:Percentage of short-term sentences released in 2019 involved in the study.*

| | Percentage of people being released from short-term sentences in the 2019 calendar year who became involved in the study |
|---|---|
| Auckland Women's | 10% of 347 total released from this prison |

| | |
|---|---|
| Christchurch Men’s | 6% of 382 total released from this prison |
| Hawke’s Bay | 12% of 341 total released from this prison |
| Northland | 7% of 416 total released from this prison |
| Spring Hill | 2% of 570 total released from this prison |
| Waikeria | 5% of 722 total released from this prison |

# 4. Results

To explore the possible causal effect of stable housing on recidivism four different analytic techniques were employed: *i) logistic regression* with covariate adjustment; *ii) model averaging* across covariate adjusted logistic regressions; *iii) non-response weighted logistic regression* with covariate adjustment; and *iv) Inverse Probability Treatment Weighting* adjusting for covariate adjustment sets. The suspected confounding sets are discussed in the 2.2.5 Confounding section.

Four analyses were undertaken:

1. The effect of having lived in an unstable housing type in the 6 months after release on re-imprisonment;
2. The effect of having lived in an unstable housing type in the 6 months after release on re-sentencing;
3. The effect of having moved two or more times in the 6 months after release on re-imprisonment; and
4. The effect of having moved two or more times in the 6 months after release on re-sentencing.

These will be discussed in turn.

Although the participants retained in the first wave of post-release interviews did not seem to involve bias introduced by re-sentencing or re-imprisonment, there was evidence it had affected the sample at the second wave of post-release interviews (see section 3.2 Restricting Information Used). As attrition in the second wave of post-release interviews was influenced significantly by re-imprisonment it was evident the sample had become biased and was not focused on for the analysis.

## 4.1 Covariate Adjustment

Unfortunately, all unweighted covariate adjusted logistic regression models used to estimate the effects of stable housing type or residential mobility on re-imprisonment had convergence warnings, as fitted probabilities reaching the boundaries of 0 or 1 occurred. The most likely explanation for this error is a combination of convergence problems and the Hauck-Donner phenomenon (Venables

& Ripley, 2002, p. 197-8). The Hauck-Donner phenomenon occurs when the probabilities predicted almost certain outcomes (to do with re-imprisonment in our case) very close to 1 or 0. This phenomenon occurs when one or more of the explanatory variables is rare, yet accurately has only accompanied only one of either being re-imprisoned or not. Variables of this type are assigned a quite extreme effect in the model often with a large standard error too. These convergence issues occurred as Pasifika ethnicity was included in all models and the three Pasifika people who were interviewed in the first post-release interview and were not re-imprisoned. Moreover, in some models whether a research participant received support after prison as well cause similar convergence problems, specifically as the 14 people who reported they did not need support and were not re-imprisoned.

Due to the warnings of algorithm non-convergence, the results in the summary should not be taken as reliable on their own. However, as the estimated effects of stable housing under covariate adjusted logistic regression did agree overall with other methods it should hold relevance.

## 4.2 Model Averaging

As mentioned in the methods section, the models were all given equal weighting. As the models averaged were subsets of the models, or the same as created in covariate adjustment the same problem prevailed of model convergence when estimating effects on re-imprisonment. The averaged models for the effects of stable housing and residential mobility on re-imprisonment included the models that did not converge as noted in the covariate adjustment section as all models averaged included a variable for Pasifika ethnicity.

## 4.3 Non-Response Weighting

Bivariate tests were used to identify which variables collected in the pre-release interview showed a relationship with participation in the first wave of post-release interviews. Logistic regression was conducted with all interactions, of which none showed as significant. Variables not significant to the model[19] were removed sequentially until only three predictors remained in the model. The variables remaining significant to predicting participation in the first wave of post-release interviews were: age group, the prison research participants were interviewed in, and whether had restrictions

[19] At the 0.05 level

on where they could go after prison. The inverse of the predicted value of the logistic regression model (propensity score) was then applied as the weight given to an individual. The svydesign() function was then used to re-weight the data.

As can be seen from the below table, the non-response weighted data shows a better balance across the variables shown to most affect retention in the post-release interview.

*Table 8: Initial sample characteristics compared to the post-release sample and the non-response weighted data.*

| | **Overall count(percentage%)** | **Post-release count(percentage%)** | **Post-release weighted count(percentage%)** |
|---|---|---|---|
| n | 199[20] | 79[21] | 188[22] |
| **Age group (%)** | | | |
| Under 30 | 74 (37%) | 24 (30%) | 67 (36%) |
| 30–39 | 64 (32%) | 19 (24%) | 57 (30%) |
| 40–49 | 38 (19%) | 21 (27%) | 38 (20%) |
| 50 or Over | 25 (12%) | 15 (19%) | 26 (14%) |
| **Restrictions on where they could go[23] (%)** | | | |
| No | 144 (72%) | 49 (62%) | 131 (70%) |
| Yes | 36 (18%) | 21 (27%) | 34 (18%) |
| Unknown | 19 (10%) | 9 (11%) | 23 (12%) |
| **Prison[24] (%)** | | | |
| Auckland Women's | 34 (17%) | 13 (16%) | 38 (20%) |
| Christchurch Men's | 28 (14%) | 16 (20%) | 29 (15%) |
| Hawke's Bay | 56 (28%) | 19 (24%) | 45 (24%) |
| Northland | 35 (18%) | 8 (10%) | 31 (16%) |
| Spring Hill | 10 (5%) | 6 (8%) | 9 (5%) |
| Waikeria | 36 (18%) | 17 (22%) | 37 (20%) |

[20] This is 199 rather than 201 due to "gender diverse/transgender" category being removed for analysis, as only one could be interviewed post-release leading to unbalanced analytic tables as n = 1.
[21] This is 79 rather than 80 due to the "gender diverse/transgender" category being removed for analysis.
[22] This is lower than 199 due to weights being truncated so any one person would not represent no more than 5 people.
[23] Places you cannot go – a self-reported measure indicating whether a research participant had any places they could not go or visit.
[24] Prison – the prison of correctional facility that the research participants were in in the pre-release interview.

### 4.3.1 Informative Loss

There was a large amount of non-response from individuals involved in the research in the post-release interviews. Variables which appeared important to predicting participation in the first post-release interview were: age, the prison they were first interviewed at, and whether they could not go anywhere after prison to predict retention.

For future research focus should be put into retaining the younger participants in the study. It seems those who were younger may have been less likely to want to engage in the interview process and share their experiences.

Those who were restricted in where they could go were more likely to be retained, most likely due to corrections requiring correct contact information. This could be a group of individuals to focus on for better retention in studies of previously incarcerated populations. However, this approach will introduce bias that should be adjusted for.

The prisons were significant only because those who were interviewed pre-release in Northland were harder to reach. However, it was speculated this may be due to many of the participants who were originally interviewed in Northland in their pre-release being unreached during the first wave of post-release interviews as they were held in other prisons. Suggesting the same relationship may not be seen if the research was to be repeated.

The largest difference between covariate adjustment and covariate adjustment with non-response weighting was in the effect of unstable housing type on re-sentencing, though the confidence interval overlaps with estimates of other methods.

## 4.4 Inverse Probability Treatment Weighting (IPTW)

Predicted values of logistic regression were used to estimate propensity scores, the probability of a research participant being exposed to unstable housing. The predictions were informed by each of the sets of confounding variables identified. Where the probability of not being exposed to unstable housing is estimated as: 1 – propensity score. The weights applied to each individual were then estimated as $\frac{1}{Propensity\ Score}$ if exposed and $\frac{1}{1 - Propensity\ Score}$ if not. The svydesign() function was then used to re-weight the data.

IPTW aims to re-weight the sample to have a uniform distribution within each confounding factor across those exposed to unstable housing and unexposed. Comparisons of Standard Mean Differences (SMDs) often recommend SMDs of < 0.1 (Zhang et al., 2019). However, due to our smaller sample size with a relatively large number of variables in the confounding variable adjustment sets SMDs of 0.2 or below will be considered a good balance as used by Florian Moik, et al (2019).

Where the IPTW data did not provide SMDs of below 0.2 across all suspected confounding variables, the propensity scores were restricted to the overlapping propensity scores in both the exposed and unexposed groups. Restricting the propensity scores meant only individuals who had similar likelihoods of being exposed to unstable housing would be included in the re-weighted data. Those who were almost certain to experience or not experience unstable housing based on confounding variables were excluded.

SMD looks at the balance in distribution of a subgroup across outcomes. It is the difference in mean outcome between groups standardised by division of the standard deviation of the outcome across participants. SMDs were produced using the CreateTableOne() function from the "survival" library in R (Therneau, 2022).

For a continuous variable SMD values based on:

$$SMD = \frac{\bar{x}_1 - \bar{x}_2}{\sqrt{\frac{1}{2}\left(s_{x_1}^2 + s_{x_2}^2\right)}}$$

Where $\bar{x}_w$ is the mean of $x$ in each group and, $s_w^2$ $are\ sample\ variances$.

For a categorical variable SMD values based on:

$$SMD = \frac{\hat{P}_1 - \hat{P}_2}{\sqrt{\frac{\left[\hat{P}_1\left(1-\hat{P}_1\right) + \hat{P}_2(1-\hat{P}_2)\right]}{2}}}$$

Where $\hat{P}_1$ is the proportion of a binary variable in the exposed group and $\hat{P}_2$ is the proportion of a binary variable in the unexposed group. The approach for the multinomial case is based on a formula presented in a paper by Yang and Dalton (2012).

### 4.4.1 Stable Housing

#### 4.4.1.1 Ideal Adjustment

The SMD between 'Ideal' confounding variables initially were balanced across those of Pasifika ethnicity, whether they cared for someone after prison, substance abuse risk, and whether they were planning to live with family. However, many variables showed significant imbalance.

*Table 9: Original SMD balance of stable housing type across 'Ideal' confounding variables.*

| | Stable | Unstable | SMD |
|---|---|---|---|
| n | 24.00 | 53.00 | |
| Unstable housing before prison (mean (SD)) | 0.29 (0.46) | 0.57 (0.50) | 0.568 |
| Māori (mean (SD)) | 0.54 (0.51) | 0.81 (0.39) | 0.592 |
| NZ European & Other (mean (SD)) | 0.62 (0.49) | 0.30 (0.46) | 0.674 |
| Pasifika (mean (SD)) | 0.04 (0.20) | 0.04 (0.19) | 0.020 |
| Gender (%) | | | 0.204 |
| Female | 5.0 (20.8) | 7.0 (13.2) | |
| Male | 19.0 (79.2) | 46.0 (86.8) | |
| Age group (%) | | | 0.461 |
| Under 30 | 4.0 (16.7) | 19.0 (35.8) | |
| 30–39 | 7.0 (29.2) | 11.0 (20.8) | |
| 40–49 | 7.0 (29.2) | 14.0 (26.4) | |
| 50 or Over | 6.0 (25.0) | 9.0 (17.0) | |
| Number of previous imprisonments (%) | | | 0.219 |
| 1-2 | 7.0 (29.2) | 11.0 (20.8) | |
| 3-6 | 5.0 (20.8) | 12.0 (22.6) | |
| More than 6 | 6.0 (25.0) | 17.0 (32.1) | |
| None | 6.0 (25.0) | 13.0 (24.5) | |
| Caring for someone after prison = Yes (%) | 9.0 (37.5) | 25.0 (47.2) | 0.197 |
| Perceived family supportiveness (initial interview) (%) | | | 0.608 |
| Neither | 4.0 (16.7) | 1.0 ( 1.9) | |
| No contact | 2.0 ( 8.3) | 3.0 ( 5.7) | |
| Supportive | 15.0 (62.5) | 45.0 (84.9) | |
| Unsupportive | 3.0 (12.5) | 4.0 ( 7.5) | |
| Substance abuse (initial interview) (%) | | | 0.156 |
| Unknown | 0.0 ( 0.0) | 0.0 ( 0.0) | |
| Lower | 4.0 (16.7) | 6.0 (11.3) | |
| Moderate | 13.0 (54.2) | 31.0 (58.5) | |
| High | 7.0 (29.2) | 16.0 (30.2) | |
| Received housing support (post-release) (%) | | | 0.658 |
| Have not needed | 8.0 (33.3) | 5.0 ( 9.4) | |

| | | | |
|---|---|---|---|
| No | 9.0 (37.5) | 33.0 (62.3) | |
| Yes | 7.0 (29.2) | 15.0 (28.3) | |
| Paid employment (post-release) = Yes (%) | 16.0 (66.7) | 23.0 (43.4) | 0.481 |
| Plan to live with family (mean (SD)) | 0.54 (0.51) | 0.58 (0.50) | 0.086 |

After IPTW many variables in the 'Ideal' adjustment set had SMDs > 0.2, showing an imbalance across the variables displayed below:

*Table 10: Re-weighted SMD balance of stable housing type across the 'Ideal' confounding variables not meeting the threshold for balance.*

| | Stable | Unstable | SMD |
|---|---|---|---|
| n | 39.90 | 68.76 | |
| Unstable housing before prison (mean (SD)) | 0.33 (0.48) | 0.53 (0.50) | 0.397 |
| Māori (mean (SD)) | 0.63 (0.49) | 0.77 (0.43) | 0.288 |
| NZ European & Other (mean (SD)) | 0.50 (0.51) | 0.34 (0.48) | 0.313 |
| Age group (%) | | | 0.215 |
| Under 30 | 9.0 (22.6) | 21.6 (31.4) | |
| 30–39 | 11.6 (29.0) | 16.2 (23.5) | |
| 40–49 | 9.4 (23.6) | 16.4 (23.8) | |
| 50 or Over | 9.9 (24.9) | 14.6 (21.3) | |
| Perceived family supportiveness (initial interview) (%) | | | 0.252 |
| Neither | 5.6 (14.1) | 5.0 ( 7.3) | |
| No contact | 3.8 ( 9.5) | 7.0 (10.2) | |
| Supportive | 26.5 (66.4) | 51.6 (75.0) | |
| Unsupportive | 4.0 (10.0) | 5.2 ( 7.5) | |
| Substance abuse (initial interview) (%) | | | 0.271 |
| Unknown | 0.0 ( 0.0) | 0.0 ( 0.0) | |
| Lower | 6.1 (15.3) | 7.3 (10.6) | |
| Moderate | 18.6 (46.7) | 41.3 (60.1) | |
| High | 15.2 (38.0) | 20.1 (29.3) | |
| Received housing support (post-release) (%) | | | 0.458 |
| Have not needed | 11.0 (27.6) | 7.7 (11.2) | |
| No | 16.3 (40.8) | 40.0 (58.1) | |
| Yes | 12.6 (31.6) | 21.1 (30.6) | |
| Paid employment (post-release) = Yes (%) | 25.8 (64.7) | 35.5 (51.6) | 0.269 |

Due to the imbalance still present in the initial IPTW people with propensity scores that did not overlap for people in stable and unstable housing types were removed from the data set (Stürmer et al., 2014; Thomas et al., 2020). The highest and lowest propensity scores were taken from those in

the group who experienced stable housing and those in the group who experienced unstable housing. The highest of the two minimum propensity scores and the lowest of the two maximum propensity scores were used as minimum and maximum cut-off values. Only people who could have been predicted to be either exposed or unexposed to stable housing based on other variables would be present, the ones whose circumstances could be too well predicted by other confounding variables would be removed. This left a re-weighted data set based on only 42 participants, as over 30 people were being predicted too accurately to stable/unstable housing based on the confounding variables identified here. After restricted-IPTW was applied, the propensity the SMDs were all < 0.2, showing a reasonable balance across variables.

*Table 11: Re-weighted SMD balance of stable housing type across the 'Ideal' confounding variables for people who had similar probabilities of living in unstable housing types.*

| | Stable | Unstable | SMD |
|---|---|---|---|
| n | 35.86 | 37.45 | |
| Unstable housing before prison (mean (SD)) | 0.37 (0.50) | 0.38 (0.50) | 0.017 |
| Māori (mean (SD)) | 0.68 (0.48) | 0.68 (0.48) | 0.006 |
| NZ European & Other (mean (SD)) | 0.47 (0.51) | 0.46 (0.51) | 0.008 |
| Pasifika (mean (SD)) | 0.03 (0.18) | 0.03 (0.17) | 0.014 |
| Gender (%) | | | 0.171 |
| Female | 4.8 (13.5) | 3.1 ( 8.2) | |
| Male | 31.0 (86.5) | 34.4 (91.8) | |
| Age group (%) | | | 0.059 |
| Under 30 | 9.0 (25.1) | 8.5 (22.7) | |
| 30–39 | 9.6 (26.7) | 10.1 (27.0) | |
| 40–49 | 8.4 (23.4) | 9.3 (24.8) | |
| 50 or Over | 8.9 (24.8) | 9.5 (25.5) | |
| Number of previous imprisonments (%) | | | 0.135 |
| 1-2 | 7.7 (21.5) | 7.1 (19.1) | |
| 3-6 | 10.6 (29.5) | 12.4 (33.2) | |
| More than 6 | 8.5 (23.7) | 10.0 (26.6) | |
| None | 9.1 (25.3) | 7.9 (21.2) | |
| Caring for someone after prison = Yes (%) | 11.8 (33.0) | 11.2 (29.8) | 0.067 |
| Perceived family supportiveness (initial interview) (%) | | | 0.120 |
| Neither | 4.6 (12.9) | 5.0 (13.4) | |
| No contact | 3.8 (10.6) | 5.0 (13.4) | |
| Supportive | 23.5 (65.4) | 24.3 (64.9) | |
| Unsupportive | 4.0 (11.1) | 3.1 ( 8.4) | |
| Substance abuse (initial interview) (%) | | | 0.104 |
| Unknown | 0.0 ( 0.0) | 0.0 ( 0.0) | |

| | | | |
|---|---|---|---|
| Lower | 5.1 (14.1) | 6.3 (16.7) | |
| Moderate | 16.6 (46.4) | 18.1 (48.3) | |
| High | 14.2 (39.5) | 13.1 (34.9) | |
| Received housing support (post-release) (%) | | | 0.119 |
| Have not needed | 8.0 (22.2) | 6.7 (17.9) | |
| No | 16.3 (45.4) | 18.8 (50.2) | |
| Yes | 11.6 (32.4) | 11.9 (31.9) | |
| Paid employment (post-release) = Yes (%) | 23.8 (66.4) | 25.3 (67.7) | 0.026 |
| Plan to live with family (mean (SD)) | 0.52 (0.51) | 0.49 (0.51) | 0.055 |

#### 4.4.1.2 DAG Adjustment

The SMD between 'DAG' identified confounding variables initially were balanced across those of Pasifika ethnicity, and gender. However, many variables showed significant imbalance:

*Table 12: Original SMD balance of stable housing type across 'DAG' confounding variables.*

| | Stable | Unstable | SMD |
|---|---|---|---|
| n | 25.00 | 52.00 | |
| Māori (mean (SD)) | 0.52 (0.51) | 0.81 (0.40) | 0.629 |
| NZ European & Other (mean (SD)) | 0.64 (0.49) | 0.31 (0.47) | 0.695 |
| Pasifika (mean (SD)) | 0.04 (0.20) | 0.04 (0.19) | 0.008 |
| Gender (%) | | | 0.176 |
| Female | 5.0 (20.0) | 7.0 (13.5) | |
| Male | 20.0 (80.0) | 45.0 (86.5) | |
| Age group (%) | | | 0.351 |
| Under 30 | 5.0 (20.0) | 18.0 (34.6) | |
| 30–39 | 7.0 (28.0) | 11.0 (21.2) | |
| 40–49 | 7.0 (28.0) | 14.0 (26.9) | |
| 50 or Over | 6.0 (24.0) | 9.0 (17.3) | |
| Unstable housing before prison (mean (SD)) | 0.32 (0.48) | 0.58 (0.50) | 0.527 |
| In a relationship (pre-release) = Not in a relationship (%) | 16.0 (64.0) | 45.0 (86.5) | 0.541 |
| Perceived family supportiveness (initial interview) (%) | | | 0.574 |
| Neither | 4.0 (16.0) | 1.0 ( 1.9) | |
| No contact | 2.0 ( 8.0) | 3.0 ( 5.8) | |
| Supportive | 16.0 (64.0) | 44.0 (84.6) | |
| Unsupportive | 3.0 (12.0) | 4.0 ( 7.7) | |
| Unstable housing plans (pre-release) (mean (SD)) | 0.52 (0.51) | 0.75 (0.44) | 0.484 |
| Paid employment (post-release) = Yes (%) | 17.0 (68.0) | 22.0 (42.3) | 0.535 |
| Received housing support (post-release) (%) | | | 0.719 |

| | | | |
|---|---|---|---|
| Have not needed | 9.0 (36.0) | 5.0 ( 9.6) | |
| No | 9.0 (36.0) | 33.0 (63.5) | |
| Yes | 7.0 (28.0) | 14.0 (26.9) | |

After IPTW many variables in the 'DAG' adjustment set had SMDs > 0.2, showing an imbalance across the variables displayed below:

*Table 13: Re-weighted SMD balance of stable housing type across the 'DAG' confounding variables not meeting the threshold for balance.*

| | Stable | Unstable | SMD |
|---|---|---|---|
| n | 43.28 | 70.81 | |
| Māori (mean (SD)) | 0.62 (0.50) | 0.76 (0.43) | 0.310 |
| NZ European & Other (mean (SD)) | 0.54 (0.51) | 0.39 (0.49) | 0.291 |
| Age group (%) | | | 0.200 |
| Under 30 | 10.8 (24.9) | 23.8 (33.6) | |
| 30–39 | 11.5 (26.6) | 15.8 (22.3) | |
| 40–49 | 10.4 (24.1) | 16.6 (23.4) | |
| 50 or Over | 10.6 (24.4) | 14.7 (20.7) | |
| Unstable housing before prison (mean (SD)) | 0.36 (0.49) | 0.53 (0.50) | 0.333 |
| In a relationship (pre-release) = Not in a relationship (%) | 29.4 (68.0) | 61.1 (86.3) | 0.447 |
| Unstable housing plans (pre-release) (mean (SD)) | 0.58 (0.50) | 0.68 (0.47) | 0.203 |
| Paid employment (post-release) = Yes (%) | 28.2 (65.2) | 37.1 (52.4) | 0.263 |
| Received housing support (post-release) (%) | | | 0.392 |
| Have not needed | 11.6 (26.9) | 9.5 (13.4) | |
| No | 17.7 (40.9) | 40.6 (57.4) | |
| Yes | 13.9 (32.2) | 20.7 (29.2) | |

With restricted propensity score values a re-weighted data set based on only 59 participants was created. However, even restricted there was no balance across those in stable and unstable housing types after prison could not be reached for Māori ethnicity or relationship status as in pre-release interview. Because of this imbalance, models were not fitted predict the effect of stable housing on recidivism for IPTW data based on the 'DAG' identified set of adjustment variables.

*Table 14: Re-weighted SMD balance of stable housing type across the 'DAG' confounding variables that still do not meet the threshold for balanced, despite only including people who had similar probabilities of living in unstable housing types.*

| | Stable | Unstable | SMD |
|---|---|---|---|
| n | 38.13 | 57.71 | |
| Māori (mean (SD)) | 0.62 (0.50) | 0.73 (0.45) | 0.221 |
| In a relationship (pre-release) = Not in a relationship (%) | 28.4 (74.6) | 48.0 (83.2) | 0.213 |

### 4.4.2 Residential Mobility

#### 4.4.2.1 Ideal Adjustment

The SMD between 'Ideal' confounding variables initially were balanced across those of Pasifika ethnicity, whether they cared for someone after prison, substance abuse risk, and whether they were in paid employment after release. However, many variables showed significant imbalance:

*Table 15: Original SMD balance of residential mobility across 'Ideal' confounding variables.*

| | 0-1 moves | 2+ moves | SMD |
|---|---|---|---|
| n | 42.00 | 35.00 | |
| 2+ moves before prison (mean (SD)) | 0.33 (0.48) | 0.57 (0.50) | 0.486 |
| Māori (mean (SD)) | 0.62 (0.49) | 0.86 (0.36) | 0.555 |
| NZ European & Other (mean (SD)) | 0.50 (0.51) | 0.29 (0.46) | 0.444 |
| Pasifika (mean (SD)) | 0.02 (0.15) | 0.06 (0.24) | 0.167 |
| Gender (%) | | | 0.366 |
| Female | 9.0 (21.4) | 3.0 ( 8.6) | |
| Male | 33.0 (78.6) | 32.0 (91.4) | |
| Age group (%) | | | 0.308 |
| Under 30 | 10.0 (23.8) | 13.0 (37.1) | |
| 30–39 | 10.0 (23.8) | 8.0 (22.9) | |
| 40–49 | 13.0 (31.0) | 8.0 (22.9) | |
| 50 or Over | 9.0 (21.4) | 6.0 (17.1) | |
| Number of previous imprisonments (%) | | | 0.542 |
| 1-2 | 13.0 (31.0) | 5.0 (14.3) | |
| 3-6 | 7.0 (16.7) | 10.0 (28.6) | |
| More than 6 | 10.0 (23.8) | 13.0 (37.1) | |
| None | 12.0 (28.6) | 7.0 (20.0) | |
| Caring for someone after prison = Yes (%) | 19.0 (45.2) | 15.0 (42.9) | 0.048 |
| Perceived family supportiveness (initial interview) (%) | | | 0.379 |
| Neither | 3.0 ( 7.1) | 2.0 ( 5.7) | |
| No contact | 2.0 ( 4.8) | 3.0 ( 8.6) | |
| Supportive | 35.0 (83.3) | 25.0 (71.4) | |
| Unsupportive | 2.0 ( 4.8) | 5.0 (14.3) | |
| Substance abuse (initial interview) (%) | | | 0.079 |
| High | 13.0 (31.0) | 10.0 (28.6) | |
| Lower | 5.0 (11.9) | 5.0 (14.3) | |

| | | | |
|---|---|---|---|
| Moderate | 24.0 (57.1) | 20.0 (57.1) | |
| Received housing support (post-release) (%) | | | 0.284 |
| Have not needed | 9.0 (21.4) | 4.0 (11.4) | |
| No | 21.0 (50.0) | 21.0 (60.0) | |
| Yes | 12.0 (28.6) | 10.0 (28.6) | |
| Paid employment (post-release) = Yes (%) | 21.0 (50.0) | 18.0 (51.4) | 0.029 |
| Plan to live with family (mean (SD)) | 0.67 (0.48) | 0.46 (0.51) | 0.426 |

After IPTW many variables in the 'Ideal' adjustment set had SMDs > 0.2, showing an imbalance across the variables displayed below:

*Table 16: Re-weighted SMD balance of residential mobility across the 'Ideal' confounding variables not meeting the threshold for balance.*

| | 0-1 moves | 2+ moves | SMD |
|---|---|---|---|
| n | 70.35 | 64.77 | |
| Gender (%) | | | 0.242 |
| Female | 10.4 (14.8) | 4.7 ( 7.3) | |
| Male | 59.9 (85.2) | 60.1 (92.7) | |
| Number of previous imprisonments (%) | | | 0.220 |
| 1-2 | 20.8 (29.5) | 14.0 (21.7) | |
| 3-6 | 13.4 (19.1) | 15.8 (24.4) | |
| More than 6 | 19.5 (27.7) | 21.2 (32.7) | |
| None | 16.6 (23.6) | 13.8 (21.3) | |
| Perceived family supportiveness (initial interview) (%) | | | 0.207 |
| Neither | 5.1 ( 7.2) | 5.0 ( 7.7) | |
| No contact | 2.9 ( 4.1) | 4.0 ( 6.1) | |
| Supportive | 59.0 (83.9) | 49.8 (76.9) | |
| Unsupportive | 3.4 ( 4.8) | 6.0 ( 9.2) | |

Due to the imbalance still present in the initial IPTW people with propensity scores that did not overlap for people in stable and unstable housing types were removed from the data set. This left a re-weighted data set based on 68 participants, excluding people whose residential mobility post-release was predicted too accurately based on the confounding variables identified here. After restricted-IPTW was applied, the propensity the SMDs were all < 0.2 with most being < 0.1, showing a reasonable balance across variables.

*Table 17: Re-weighted SMD balance of residential mobility across the 'Ideal' confounding variables for people who had similar probabilities of high residential mobility.*

| | 0-1 moves | 2+ moves | SMD |
|---|---|---|---|
| n | 68.32 | 57.14 | |
| 2+ moves before prison (mean (SD)) | 0.46 (0.51) | 0.45 (0.51) | 0.025 |
| Māori (mean (SD)) | 0.72 (0.45) | 0.74 (0.45) | 0.037 |
| NZ European & Other (mean (SD)) | 0.36 (0.49) | 0.35 (0.49) | 0.027 |
| Pasifika (mean (SD)) | 0.04 (0.19) | 0.05 (0.22) | 0.071 |
| Gender (%) | | | 0.177 |
| Female | 9.4 (13.8) | 4.7 ( 8.2) | |
| Male | 58.9 (86.2) | 52.4 (91.8) | |
| Age group (%) | | | 0.099 |
| Under 30 | 18.1 (26.5) | 16.8 (29.3) | |
| 30–39 | 16.8 (24.6) | 13.1 (23.0) | |
| 40–49 | 21.4 (31.4) | 16.1 (28.1) | |
| 50 or Over | 11.9 (17.4) | 11.2 (19.5) | |
| Number of previous imprisonments (%) | | | 0.078 |
| 1-2 | 18.8 (27.5) | 14.0 (24.6) | |
| 3-6 | 13.4 (19.7) | 12.5 (22.0) | |
| More than 6 | 19.5 (28.5) | 16.8 (29.4) | |
| None | 16.6 (24.3) | 13.8 (24.1) | |
| Caring for someone after prison = Yes (%) | 28.7 (42.1) | 25.4 (44.5) | 0.050 |
| Perceived family supportiveness (initial interview) (%) | | | 0.109 |
| Neither | 5.1 ( 7.4) | 5.0 ( 8.8) | |
| No contact | 2.9 ( 4.3) | 1.7 ( 3.0) | |
| Supportive | 57.0 (83.4) | 46.6 (81.6) | |
| Unsupportive | 3.4 ( 4.9) | 3.8 ( 6.6) | |
| Substance abuse (initial interview) (%) | | | 0.030 |
| High | 19.3 (28.2) | 16.1 (28.1) | |
| Lower | 10.6 (15.5) | 8.3 (14.5) | |
| Moderate | 38.5 (56.3) | 32.8 (57.4) | |
| Received housing support (post-release) (%) | | | 0.065 |
| Have not needed | 12.9 (18.9) | 9.9 (17.3) | |
| No | 36.8 (53.8) | 32.6 (57.0) | |
| Yes | 18.6 (27.2) | 14.6 (25.6) | |
| Paid employment (post-release) = Yes (%) | 38.2 (55.9) | 35.0 (61.3) | 0.110 |
| Plan to live with family (mean (SD)) | 0.59 (0.50) | 0.62 (0.50) | 0.050 |

#### 4.4.2.2 DAG Adjustment

The SMD between ‘DAG’ identified confounding variables initially were balanced across those of Pasifika ethnicity. However, many variables showed significant imbalance:

*Table 18: Original SMD balance of residential mobility across ‘DAG’ confounding variables.*

| | 0-1 moves | 2+ moves | SMD |
|---|---|---|---|
| n | 44.0 | 35.0 | |
| Gender (%) | | | 0.397 |
| Female | 10.0 (22.7) | 3.0 ( 8.6) | |
| Male | 34.0 (77.3) | 32.0 (91.4) | |
| Māori (mean (SD)) | 0.59 (0.50) | 0.86 (0.36) | 0.616 |
| NZ European & Other (mean (SD)) | 0.52 (0.51) | 0.29 (0.46) | 0.491 |
| Pasifika (mean (SD)) | 0.02 (0.15) | 0.06 (0.24) | 0.174 |
| Age group (%) | | | 0.271 |
| Under 30 | 11.0 (25.0) | 13.0 (37.1) | |
| 30–39 | 11.0 (25.0) | 8.0 (22.9) | |
| 40–49 | 13.0 (29.5) | 8.0 (22.9) | |
| 50 or Over | 9.0 (20.5) | 6.0 (17.1) | |
| 2+ moves before prison (mean (SD)) | 0.36 (0.49) | 0.57 (0.50) | 0.420 |
| Number of previous imprisonments (%) | | | 0.581 |
| 1-2 | 14.0 (31.8) | 5.0 (14.3) | |
| 3-6 | 7.0 (15.9) | 10.0 (28.6) | |
| More than 6 | 10.0 (22.7) | 13.0 (37.1) | |
| None | 13.0 (29.5) | 7.0 (20.0) | |
| In education (post-release) = Yes (%) | 3.0 ( 6.8) | 8.0 (22.9) | 0.463 |
| Know where they are going to live (pre-release) = Yes (%) | 34.0 (77.3) | 18.0 (51.4) | 0.560 |

After IPTW many variables in the ‘DAG’ adjustment set had SMDs > 0.2 showing an imbalance across the variables displayed below:

*Table 19: Re-weighted SMD balance of residential mobility across the ‘DAG’ confounding variables not meeting the threshold for balance.*

| | 0-1 moves | 2+ moves | SMD |
|---|---|---|---|
| n | 71.4 | 68.4 | |
| Gender (%) | | | 0.292 |
| Female | 11.4 (16.0) | 4.7 ( 6.8) | |
| Male | 59.9 (84.0) | 63.8 (93.2) | |
| In education (post-release) = Yes (%) | 4.3 ( 6.1) | 9.2 (13.4) | 0.248 |
| Know where they are going to live (pre-release) = Yes (%) | 51.8 (72.5) | 43.0 (62.9) | 0.207 |

Due to the imbalance still present in the initial IPTW, people with propensity scores that did not overlap for people in stable and unstable housing types were removed from the data set. Leaving a re-weighted data set based on 57 participants, without the people whose residential mobility post-release was predicted too accurately based on the confounding variables identified here. After restricted-IPTW was applied, the propensity the SMDs were all < 0.2, showing a reasonable balance across variables.

*Table 20: Re-weighted SMD balance of residential mobility across the 'DAG' confounding variables for people who had similar probabilities of high residential mobility.*

| | 0-1 moves | 2+ moves | SMD |
|---|---|---|---|
| n | 62.0 | 53.4 | |
| Gender (%) | | | 0.136 |
| Female | 6.2 (10.1) | 3.4 ( 6.3) | |
| Male | 55.7 (89.9) | 50.0 (93.7) | |
| Māori (mean (SD)) | 0.73 (0.45) | 0.65 (0.49) | 0.162 |
| NZ European & Other (mean (SD)) | 0.34 (0.48) | 0.42 (0.51) | 0.155 |
| Pasifika (mean (SD)) | 0.05 (0.22) | 0.03 (0.19) | 0.072 |
| Age group (%) | | | 0.037 |
| Under 30 | 18.4 (29.7) | 16.4 (30.6) | |
| 30–39 | 14.4 (23.2) | 11.6 (21.7) | |
| 40–49 | 14.0 (22.6) | 12.1 (22.6) | |
| 50 or Over | 15.2 (24.6) | 13.3 (25.0) | |
| 2+ moves before prison (mean (SD)) | 0.44 (0.50) | 0.38 (0.50) | 0.123 |
| Number of previous imprisonments (%) | | | 0.191 |
| 1-2 | 15.1 (24.3) | 15.2 (28.5) | |
| 3-6 | 12.2 (19.7) | 7.5 (14.0) | |
| More than 6 | 20.5 (33.0) | 20.1 (37.6) | |
| None | 14.2 (22.9) | 10.6 (19.9) | |
| In education (post-release) = Yes (%) | 4.3 ( 7.0) | 1.6 ( 2.9) | 0.188 |
| Knew where they are going to live (pre-release) = Yes (%) | 44.5 (71.8) | 38.2 (71.5) | 0.007 |

## 4.5 Housing Type

Of the 80 people who could be found and interviewed in the 6 month post-release interview, only 34% were found and had remained in stable housing types since their release.

*Table 21: Least stable housing type lived in within 6 months of release.*

| Housing type 6 months after prison | Count | Percentage |
|---|---|---|

| | | |
|---|---|---|
| Stable | 27 | 34 |
| Unstable | 53 | 66 |
| Total | 80 | 100 |

### 4.5.1 Re-imprisonment

The strongest bivariate relationship of the four analyses using the original data available was of stable housing on re-imprisonment. There is significant evidence to suggest a relationship between stable housing type 6 months after prison and re-imprisonment (Chi-squared test p-value = 0.020). Those who remained in stable housing types after prison (7%) were less likely than those who had not (34%) to be re-imprisoned. Those without support were 4.6 times more likely to be re-imprisoned.

*Table 22: Least stable housing type lived in within 6 months of release by re-imprisonment within 12 months of release.*

| Unweighted | Re-imprisoned | | |
|---|---|---|---|
| | No | Yes | Total |
| Stable | 25 | 2 | 27 |
| Unstable | 35 | 18 | 53 |
| Total | 60 | 20 | 80 |

After re-weighting the data for non-response, the evidence of a relationship grew in strength. After re-weighting the data to better represent those who were lost to follow-up, Table 23 shows a very similar relationship between stable housing type and re-imprisonment, though stronger. Of those in stable housing 6% were expected to be re-imprisoned compared to 33% of those in unstable housing. There was strong evidence of a relationship between stable housing after prison and re-imprisonment (chi-squared p-value[25] = 0.0072).

*Table 23: Non-response weighted data table for least stable housing type lived in within 6 months of release by re-imprisonment within 12 months of release.*

| Non-response weighted | Re-imprisoned | |
|---|---|---|
| | No | Yes |

[25] Calculated with the weights incorporated using the survey package in R (Lumley, 2020).

| | | |
|---|---|---|
| Stable | 61 | 4 |
| Unstable | 84 | 41 |

The estimated effects are presented as a difference to log odds for covariate adjusted logistic regression, non-response weighted logistic regression and model averaging. The estimated effects are presented as a difference to log relative risk for IPTW. Under adjustment for the 'Ideal' adjustment variables, the estimated effect of living in an unstable housing type on re-imprisonment had no statistical significance across any of the calculated models.

*Table 24: Stable housing effects on re-imprisonment adjusting for 'Ideal' confounding variables.*

| Effects of stable housing type on re-imprisonment | Estimate | Standard error[26] | t or z value | Pr(>\|t\|) or Pr(>\|z\|) | Lower CI | Upper CI |
|---|---|---|---|---|---|---|
| IPTW-restricted Ideal | 0.53 | 0.83 | 0.64 | 0.52 | -1.09 | 2.16 |
| Non-response IPW Ideal | 0.13 | 1.34 | 0.10 | 0.92 | -2.49 | 2.74 |
| Covariate adjusted Ideal | 0.13 | 1.31 | 0.10 | 0.92 | -2.49 | 2.87 |
| Averaged Ideal equal weight | 1.17 | 1.00 | 1.16 | 0.25 | -0.80 | 3.13 |

Under adjustment for the 'DAG' adjustment variables, the estimated effects of living in an unstable housing type on re-imprisonment had no statistical significance across any of the calculated models. The model for IPTW was not calculated due to continuing imbalance across suspected continued confounding.

*Table 25: Stable housing effects on re-imprisonment adjusting for 'DAG' confounding variables.*

| Effects of stable housing type on re-imprisonment | Estimate | Standard error | t or z value | Pr(>\|t\|) or Pr(>\|z\|) | Lower CI | Upper CI |
|---|---|---|---|---|---|---|
| IPTW-restricted DAG | - | - | - | - | - | - |
| Non-response IPW DAG | 1.52 | 1.33 | 1.14 | 0.26 | -1.09 | 4.13 |
| Covariate adjusted DAG | 0.99 | 1.13 | 0.88 | 0.38 | -1.19 | 3.40 |
| Averaged DAG equal weight | 1.17 | 1.00 | 1.18 | 0.24 | -0.78 | 3.13 |

Despite evidence of a relationship between stable housing and re-imprisonment in non-response weighted and unweighted bivariate analysis, there was no evidence of a significant relationship after

[26] For all averaged models this is the adjusted standard error covered by Burnham and Anderson (2002)

any suspected confounding set was adjusted for. There was no evidence of a significant relationship after suspected confounders were adjusted for as confidence intervals for all estimates straddled zero. Encouragingly all the estimated effects of unstable housing were in the same direction.

### 4.5.2 Re-sentencing

There is no significant evidence of an association between stable housing 6 months after prison and re-sentencing that was not due to chance either in the data collected or the data re-weighted for non-response (Chi-squared test p-value of 0.27 and 0.20[27] respectively). However, in both cases those in unstable housing were more likely to be re-sentenced than those in stable housing.

*Table 26: Least stable housing type lived in within 6 months of release by re-sentencings within 12 months of release.*

| Unweighted | Re-sentenced | | |
|---|---|---|---|
| | No | Yes | Total |
| Stable | 19 | 8 | 27 |
| Unstable | 29 | 24 | 53 |
| Total | 48 | 32 | 80 |

*Table 27: Non-response weighted data table for least stable housing type lived in within 6 months of release by re-sentencings within 12 months of release.*

| Non-response weighted | Re-sentenced | |
|---|---|---|
| | No | Yes |
| Stable | 46 | 19 |
| Unstable | 69 | 56 |

Under adjustment for the 'Ideal' adjustment variables, the estimated effect of living in an unstable housing type on re-sentencing had no statistical or practical significance across any of the calculated models.

[27] Calculated with the weights incorporated using the survey package in R (Lumley, 2020).

*Table 28: Stable housing effects on re-sentencing adjusting for 'Ideal' confounding variables.*

| Effects of stable housing type on re-sentencing | Estimate | Standard error | t or z value | Pr(>\|t\|) or Pr(>\|z\|) | Lower CI | Upper CI |
|---|---|---|---|---|---|---|
| IPTW-restricted Ideal | 0.21 | 0.43 | 0.49 | 0.63 | -0.63 | 1.06 |
| Non-response IPW Ideal | -0.14 | 0.80 | -0.17 | 0.87 | -1.71 | 1.43 |
| Covariate adjusted Ideal | 0.19 | 1.04 | 0.18 | 0.86 | -1.88 | 2.32 |
| Averaged Ideal equal weight | 0.36 | 0.86 | 0.42 | 0.67 | -1.32 | 2.05 |

Under adjustment for the 'DAG' adjustment variables, the estimated effect of living in an unstable housing type on re-sentencing had no statistical or practical significance across any of the calculated models. The model for IPTW was not calculated due to continuing imbalance across suspected continued confounding.

*Table 29: Stable housing effects on re-sentencing adjusting for 'DAG' confounding variables.*

| Effects of stable housing type on re-sentencing | Estimate | Standard error | t or z value | Pr(>\|t\|) or Pr(>\|z\|) | Lower CI | Upper CI |
|---|---|---|---|---|---|---|
| IPTW-restricted DAG | - | - | - | - | - | - |
| Non-response IPW DAG | 0.43 | 0.93 | 0.46 | 0.65 | -1.40 | 2.26 |
| Covariate adjusted DAG | 0.19 | 0.95 | 0.20 | 0.84 | -1.73 | 2.13 |
| Averaged DAG equal weight | 0.37 | 0.85 | 0.43 | 0.66 | -1.29 | 2.02 |

There was no evidence of a significant effect of stable housing types on re-sentencing after suspected confounding set was adjusted for. After suspected confounders were adjusted for all confidence intervals for estimates straddled zero.

### 4.5.3 Other Significant Relationships

Ethnicity is associated with housing after prison. There is significant evidence of a relationship between stable housing after prison and New Zealand Māori ethnicity (chi-squared test p-value = 0.013). Participants who identified as having Māori ethnicity were more likely to experience living in unstable housing types. This will be heavily influenced by how stable housing types were categorised as 30 of the 57 post-release interview participants who identified as Māori were recorded as living with friend(s) and/or family after release. Of the 38 people who were recorded as living with family/friends, 30 identified as Māori.

*Table 30: Post-release housing type for Māori.*

| | Lived in stable housing type(s) post-release | Lived in unstable housing type(s) post-release | Total |
|---|---|---|---|
| Not Māori | 13 (57%) | 10 (43%) | 23 (100%) |
| Māori | 14 (25%) | 43 (75%) | 57 (100%) |
| Total | 27 | 53 | 80 |

New Zealand Māori (75%) were more likely than non-New Zealand Māori (43%) to be in unstable housing types. Participants who identified as New Zealand Māori were 1.7 times more likely to be in unstable housing types.

Conversely, participants who identified as New Zealand European and Other ethnicities were less likely to experience living in unstable housing types. This was a significant bivariate relationship (chi-squared test p-value = 0.010). However, again this is heavily influenced by how unstable housing types were categorised as only 12 of the 33 participants identifying as New Zealand European and Other ethnicities lived with friend(s) and/or family.

*Table 31:Post-release housing type for New Zealand European and Other.*

| | Lived in stable housing type(s) post-release | Lived in unstable housing type(s) post-release | Total |
|---|---|---|---|
| Not New Zealand European and Other | 10 (21%) | 37 (79%) | 47 (100%) |
| New Zealand European and Other | 17 (52%) | 16 (48%) | 33 (100%) |
| Total | 27 | 53 | 80 |

There was some evidence of a relationship between being in a relationship and living in stable housing after release (chi-squared test p-value = 0.074). Participants who answered they were in a relationship in the pre-release interview were more less likely to experience living in unstable housing types. Again, this is heavily influenced by how unstable housing types were categorised as living with friend(s) and/or family was classified as unstable housing.

*Table 32: Post-release housing type and relationship status.*

| | Lived in stable housing type(s) post-release | Lived in unstable housing type(s) post-release | Total |
|---|---|---|---|

| | | | |
|---|---|---|---|
| In a relationship | 9 (56%) | 7 (44%) | 16 (100%) |
| Not in a relationship | 18 (29%) | 45 (71%) | 63 (100%) |
| Total | 27 | 52 | 79 |

Participants in the study who lived in unstable housing types the 6 months before prison were more likely to have expected to live in unstable housing types after prison. This was highly significant at the bivariate level (chi-squared test p-value = 1.9e-13). Moreover, participants appear to have been released to worse accommodation, as only 10 participants interviewed in the post-release interview had experienced unstable housing types before prison and managed to find more stable housing after release. A further 23 who had lived in stable housing types before prison experienced unstable housing types after release.

*Table 33: Housing type prior to prison and planned housing after release from prison.*

| | Planned to live in stable housing type(s) post-release | Planned to live in unstable housing type(s) post-release | Total |
|---|---|---|---|
| In stable housing type(s) prior to prison | 51 (58.0%) | 37 (42.0%) | 88 (100.0%) |
| In unstable housing type(s) prior to prison | 10 (8.8%) | 103 (91.2%) | 113 (100.0%) |
| Total | 60 | 141 | 201 |

*Table 34: Housing type prior to prison and post-release.*

| | Lived in stable housing type(s) post-release | Lived in unstable housing type(s) post-release | Total |
|---|---|---|---|
| In stable housing type(s) prior to prison | 17 (43%) | 23 (58%) | 40 (100%) |
| In unstable housing type(s) prior to prison | 10 (25%) | 30 (75%) | 40 (100%) |
| Total | 27 | 53 | 80 |

It seems although having stable housing plans meant securing stable housing for the 6 months after release was more likely, many participants plans were overly optimistic. Over half of the people who planned on living in a stable housing type prior to release ended up living in unstable housing during the 6 months post-release. Only 27% of those who planned on living in unstable housing types ended up finding more stable housing post-release. There was some evidence of significance

of this relationship in Fisher's exact t-test but not in a chi-squared test (p-values of 0.080 and 0.12 respectively).

*Table 35: Housing type planned while in prison and actual housing post-release.*

| | Lived in stable housing type(s) post-release | Lived in unstable housing type(s) post-release | Total |
|---|---|---|---|
| Planned to live in a stable housing type post-release | 12 (48%) | 13 (52%) | 25 (100%) |
| Planned to live in an unstable housing type post-release | 15 (27%) | 40 (73%) | 55 (100%) |
| Total | 27 | 53 | 80 |

Over 75% of our sample who could be found and interviewed in the post-release interview thought of their family as supportive. There was evidence of a significant bivariate relationship (Fisher's exact t-test p-value = 0.046). This result is highly comparable to the 73% of the pre-release interview sample. Due to accommodation with friend(s) and/or family being recorded as an unstable housing type the results show counterintuitively that participants who had families they thought of as supportive were the most likely to be living in unstable housing types.

*Table 36: Perceive family supportiveness prior to release and housing type post-release.*

| | | Lived in stable housing type(s) post-release | Lived in unstable housing type(s) post-release | Total |
|---|---|---|---|---|
| Perceived family supportiveness pre-release | No contact | 3 (50%) | 3 (50%) | 6 (100%) |
| | Unsupportive | 4 (50%) | 4 (50%) | 8 (100%) |
| | Neither | 4 (80%) | 1 (20%) | 5 (100%) |
| | Supportive | 16 (26%) | 45 (74%) | 61 (100%) |
| | Total | 27 | 53 | 80 (100%) |

Housing support after prison was significantly related to living in stable housing types in the 6 months after release (Fisher's exact t-test p-value = 0.013). Those who had not needed housing support had the highest percentage of people in stable housing post-release. Those who did receive housing support were less likely to be in unstable housing (65%) than those who did not receive support (79%).

*Table 37:Housing type post-release and housing support received post-release.*

| | Lived in stable housing type(s) post-release | Lived in unstable housing type(s) post-release | Total |
|---|---|---|---|

| | | | |
|---|---|---|---|
| Have not needed housing support | 9 (64%) | 5 (36%) | 14 (100%) |
| Did not receive housing support | 9 (21%) | 33 (79%) | 42 (100%) |
| Received housing support | 8 (35%) | 15 (65%) | 23 (100%) |
| Total | 26 | 53 | 79 |

Those who were interviewed post-release and had taken up paid employment post-release found themselves less likely to experience unstable housing types (chi-squared test p-value = 0.041). Again, there was some concern about this measure acting in this direction as stable housing and employment were thought to be very intertwined. However, it seemed most likely employment would affect abilities to secure housing, especially as some participants secured housing as part of their employment. There is still some room for concern that housing stability would affect stable employment.

*Table 38: Housing type post-release and employment post-release.*

| | Lived in stable housing type(s) post-release | Lived in unstable housing type(s) post-release | Total |
|---|---|---|---|
| Unemployed post-release | 8 (21%) | 30 (79%) | 38 (100%) |
| Employed post-release | 19 (45%) | 23 (55%) | 42 (100%) |
| Total | 27 | 53 | 80 |

## 4.6 Residential Mobility

Of the 80 people interviewed in the first post-release interview 44% had moved two or more times since their release.

*Table 39: Residential mobility within 6 months of release.*

| Number of moves 6 months after prison | Count | Percentage |
|---|---|---|
| 0-1 moves | 45 | 56 |
| 2+ moves | 35 | 44 |
| Total | 80 | 100 |

### 4.6.1 Re-imprisonment

There is no significant evidence to suggest a relationship between the number of moves 6 months after prison and re-imprisonment not due to chance in either the data collected or the re-weighted data (Chi-squared test p-value of 0.36 and 0.30[28]). However, in both cases those who moved at least twice since release were more likely to be re-imprisoned than those who had only moved once.

*Table 40: Residential mobility within 6 months of release by re-imprisonment within 12 months of release.*

| Unweighted | Re-imprisoned | | |
|---|---|---|---|
| | No | Yes | Total |
| 0-1 moves | 36 | 9 | 45 |
| 2+ moves | 24 | 11 | 35 |
| Total | 60 | 20 | 80 |

*Table 41: Non-response weighted data table for residential mobility within 6 months of release by re-imprisonment within 12 months of release.*

| Non-response weighted | Re-imprisoned | |
|---|---|---|
| | No | Yes |
| 0-1 moves | 87 | 21 |
| 2+ moves | 57 | 25 |

The estimated effects are presented as a difference to log odds for covariate adjusted logistic regression, non-response weighted logistic regression and model averaging. The estimated effects are presented as a difference to log relative risk for IPTW. Under adjustment for the 'Ideal' adjustment variables the estimated effect of high residential mobility on re-imprisonment had no statistical significance across any of the calculated models.

*Table 42: Residential mobilities effects on re-imprisonment adjusting for 'Ideal' confounding variables.*

| Effect of high residential mobility on re-imprisonment | Estimate | Standard error | t or z value | Pr(>\|t\|) or Pr(>\|z\|) | Lower CI | Upper CI |
|---|---|---|---|---|---|---|
| IPTW-restricted Ideal | -0.37 | 0.54 | -0.69 | 0.49 | -1.43 | 0.69 |
| Non-response IPW Ideal | -0.83 | 0.82 | -1.01 | 0.32 | -2.44 | 0.78 |

28 Calculated with the weights incorporated using the survey package in R (Lumley, 2020).

| | | | | | | |
|---|---|---|---|---|---|---|
| Covariate adjusted Ideal | -1.44 | 1.02 | -1.41 | 0.16 | -3.58 | 0.48 |
| Averaged Ideal equal weight | -0.46 | 0.86 | 0.54 | 0.59 | -2.15 | 1.22 |

Under adjustment for the 'DAG' adjustment variables the estimated effect of high residential mobility on re-imprisonment had no statistical significance across any of the calculated models.

*Table 43: Residential mobilities effects on re-imprisonment adjusting for 'DAG' confounding variables.*

| Effect of high residential mobility on re-imprisonment | Estimate | Standard error | t or z value | Pr(>\|t\|) or Pr(>\|z\|) | Lower CI | Upper CI |
|---|---|---|---|---|---|---|
| IPTW-restricted DAG | 0.44 | 0.48 | 0.93 | 0.36 | -0.49 | 1.37 |
| Non-response IPW DAG | 0.11 | 0.88 | 0.12 | 0.90 | -1.62 | 1.84 |
| Covariate adjusted DAG | 0.25 | 0.80 | 0.32 | 0.75 | -1.33 | 1.86 |
| Averaged DAG equal weight | 0.86 | 0.73 | 1.18 | 0.24 | -0.56 | 2.28 |

The confidence intervals for all estimates straddled zero, showing a lack of evidence for an effect of high residential mobility in the 6 months after release on re-imprisonment when these confounding variables are adjusted for.

### 4.6.2 Re-sentencing

There was no significant evidence to suggest a relationship between the number of moves 6 months after prison and re-sentencing was not due to chance in the data collected (Chi-squared test p-value = 0.11). However, after re-weighting the data for non-response there was some evidence of a relationship between number of moves after prison and re-sentencing (chi-squared p-value[29] = 0.078).

*Table 44: Residential mobility within 6 months of release by re-sentencing within 12 months of release.*

| Unweighted | Re-sentenced | | |
|---|---|---|---|
| | No | Yes | Total |
| 0-1 moves | 31 | 14 | 45 |
| 2+ moves | 17 | 18 | 35 |
| Total | 48 | 32 | 80 |

[29] Calculated with the weights incorporated using the survey package in R (Lumley, 2020).

*Table 45: Non-response weighted data table for residential mobility within 6 months of release by re-sentencing within 12 months of release.*

| Non-response weighted | Re-sentenced | |
|---|---|---|
| | No | Yes |
| 0-1 moves | 75 | 33 |
| 2+ moves | 40 | 42 |

Under adjustment for the 'Ideal' adjustment variables the estimated effect of high residential mobility on re-sentencing had no statistical significance across any of the calculated models.

*Table 46: Residential mobilities effects on re-sentencing adjusting for 'Ideal' confounding variables.*

| Effect of high residential mobility on re-sentencing | Estimate | Standard error | t or z value | Pr(>\|t\|) or Pr(>\|z\|) | Lower CI | Upper CI |
|---|---|---|---|---|---|---|
| IPTW-restricted Ideal | 0.13 | 0.31 | 0.41 | 0.68 | -0.49 | 0.75 |
| Non-response IPW Ideal | 0.35 | 0.80 | 0.44 | 0.66 | -1.22 | 1.93 |
| Covariate adjusted Ideal | 0.47 | 0.80 | 0.58 | 0.56 | -1.15 | 2.06 |
| Averaged Ideal equal weight | 0.62 | 0.68 | 0.91 | 0.36 | -0.72 | 1.96 |

Under adjustment for the 'DAG' adjustment variables, high residential mobility showed no significant effects under non-response weighted logistic regression or model averaging. However, there was evidence to suggest high residential mobility increased the log relative risk of re-sentencing by 0.73 under IPTW and very minimal evidence to suggest high residential mobility increased the log odds of re-sentencing under covariate adjusted logistic regression.

*Table 47: Residential mobilities effects on re-sentencing adjusting for 'DAG' confounding variables.*

| Effect of high residential mobility on re-sentencing | Estimate | Standard error | t or z value | Pr(>\|t\|) or Pr(>\|z\|) | Lower CI | Upper CI |
|---|---|---|---|---|---|---|
| IPTW-restricted DAG | 0.73 | 0.32 | 2.28 | 0.03 | 0.10 | 1.35 |
| Non-response IPW DAG | 1.14 | 0.72 | 1.57 | 0.12 | -0.28 | 2.55 |
| Covariate adjusted DAG | 1.17 | 0.71 | 1.65 | 0.10 | -0.19 | 2.61 |
| Averaged DAG equal weight | 0.90 | 0.69 | 1.30 | 0.20 | -0.46 | 2.26 |

Table 47 shows the only time a confidence interval did not straddle zero. There was very slight evidence of a significant effect using IPTW of the DAG identified set of confounders. However, these results still show a lack of evidence for an effect of high residential mobility in the 6 months after release on re-sentencing when confounding variables are adjusted for.

### 4.6.3 Other Significant Relationships

Again, ethnicity seems to play a role in stability of housing after prison. There is significant evidence of a relationship between residential mobility after prison and New Zealand Māori ethnicity (chi-squared test p-value = 0.023). Participants who identified as having Māori ethnicity were more likely to experience high residential mobility.

*Table 48: Post-release residential mobility by Māori ethnicity.*

| | Moved 0-1 times post-release | Moved 2+ times post-release | Total |
|---|---|---|---|
| Not Māori | 18 (78%) | 5 (22%) | 23 (100%) |
| Māori | 27 (47%) | 30 (53%) | 57 (100%) |
| Total | 45 | 35 | 80 |

New Zealand Māori being in unstable housing types may be largely explained away by being heavily influenced by the number living with family/friends. However, this trend of instability of housing continues for New Zealand Māori in terms of residential stability, as New Zealand Māori were also 2.4 times more likely to have moved at least two times. These results combined suggest New Zealand Māori may be experiencing worse housing after prison. These results suggest New Zealand Māori are staying in less stable accommodation types and experiencing more residential mobility.

Participants who identified as having New Zealand European and Other ethnicity were less likely to experience high residential mobility. There was some evidence of this relationship (chi-squared test p-value = 0.071).

*Table 49: Post-release residential mobility by New Zealand European and Other ethnicity.*

| | Moved 0-1 times post-release | Moved 2+ times post-release | Total |
|---|---|---|---|
| Not New Zealand European and Other | 22 (47%) | 25 (53%) | 47 (100%) |
| New Zealand European and Other | 23 (70%) | 10 (30%) | 33 (100%) |
| Total | 45 | 35 | 80 |

The number of previous imprisonments seemed to impact residential mobility after release based upon the sample interviewed post-release (chi-squared test p-value = 0.087). Those with three or more previous imprisonments were much more likely to experience high residential mobility. Participants with one or two previous imprisonments were the most likely to experience low residential mobility, surprisingly even more so than participants who had only served their first imprisonment. Perhaps due to shorter sentences or less support provided to first time imprisonments.

*Table 50: Number of previous imprisonments and residential mobility post-release.*

| | | Moved 0-1 times post-release | Moved 2+ times post-release | Total |
|---|---|---|---|---|
| Number of previous imprisonments | None | 13 (65%) | 7 (35%) | 20 (100%) |
| | 1-2 | 15 (75%) | 5 (25%) | 20 (100%) |
| | 3-6 | 7 (41%) | 10 (59%) | 17 (100%) |
| | More than 6 | 10 (43%) | 13 (57%) | 23 (100%) |
| | Total | 45 | 35 | 80 |

Participants who knew where they were going to live after prison were almost half as likely as those who did not know where they were going to live to experience high residential mobility. There was statistical evidence of this (chi-squared test p-value = 0.025).

*Table 51: Planned accommodation while in prison compared to residential mobility post-release.*

| | Moved 0-1 times post-release | Moved 2+ times post-release | Total |
|---|---|---|---|
| No housing plans for after release | 10 (37%) | 17 (63%) | 27 (100%) |
| Had housing plans for after release | 35 (66%) | 18 (34%) | 53 (100%) |
| Total | 45 | 35 | 80 |

Although quite statistically significant (Fisher's exact t-test p-value = 0.11) it is of interest to note in our sample participants with high residential mobility before prison were more likely to have experienced high residential mobility after prison as well. Of the participants who experienced high residential mobility in the 6 months before prison, 54% experienced high residential mobility in the approximately 6 months after release as well. While 17 participants interviewed in the post-release interview had experienced high residential mobility before prison managed to find more residential stability after release, 15 who had residential stability before prison experienced high residential mobility after release.

*Table 52: Residential mobility prior to prison compared to residential mobility post-release.*

| | Moved 0-1 times post-release | Moved 2+ times post-release | Total |
|---|---|---|---|
| Moved 0-1 times prior to prison | 28 (65%) | 15 (35%) | 43 (100%) |
| Moved 2+ times prior to prison | 17 (46%) | 20 (54%) | 37 (100%) |
| Total | 45 | 35 | 80 |

Although quite statistically significant (Fisher's exact t-test p-value = 0.11), a common theme was present in that the few who were interviewed post-release and had taken up education after release found themselves much less likely to experience high residential mobility. There was some concern about this measure as although it seemed most likely education would affect abilities to secure housing, especially as many our participants were doing further education as part of their employment. There is still some room for concern that housing stability and access to other financial resources would affect a decision to continue education/training.

*Table 53: Education and residential mobility post-release.*

| | Moved 0-1 times post-release | Moved 2+ times post-release | Total |
|---|---|---|---|
| No education post-release | 41 (60%) | 27 (40%) | 68 (100%) |
| Education post-release | 4 (33%) | 8 (67%) | 12 (100%) |
| Total | 45 | 35 | 80 |

## 4.7 Significant Associations with Re-sentencing

There was an inverted U-shaped relationship between age group and re-sentencing, this relationship was significant (chi-squared test p-value = 0.0041). Participants who were under 30 were slightly less likely than the 30–39 year-old participants to be re-sentenced. When the 30-39 age group is compared to those who were 40 and older, the older age group carried a lower likelihood of re-sentencing. The participants who were at least 50 years old were the least likely to be re-sentenced.

*Table 54: Re-sentencing across age groups.*

| | Not re-sentenced | Re-sentenced | Total |
|---|---|---|---|
| Under 30 | 41 (55%) | 33 (45%) | 74 (100%) |
| 30–39 | 25 (39%) | 39 (61%) | 64 (100%) |
| 40–49 | 23 (61%) | 15 (39%) | 38 (100%) |
| 50 or over | 20 (80%) | 5 (20%) | 25 (100%) |
| Total | 109 | 92 | 201 |

Men were slightly more likely than women to be re-sentenced (Fisher's exact t-test p-value = 0.077). Where only 32% of women in the study were re-sentenced, 48% of the men were re-sentenced within a year.

*Table 55: Re-sentencing across gender.*

| | Not re-sentenced | Re-sentenced | Total |
|---|---|---|---|
| Female | 23 (68%) | 11 (32%) | 34 (100%) |
| Male | 86 (52%) | 79 (48%) | 165 (100%) |
| Total | 109 | 92 | 199[30] |

An association between New Zealand European and Other ethnicity and re-sentencing showed participants who identified as New Zealand European and Other were less likely to be re-sentenced (chi-squared test p-value = 0.022).

*Table 56: Re-sentencing by New Zealand European and Other ethnicity.*

| | Not re-sentenced | Re-sentenced | Total |
|---|---|---|---|
| Not New Zealand European and Other | 60 (48%) | 66 (52%) | 126 (100%) |
| New Zealand European and Other | 49 (65%) | 26 (35%) | 75 (100%) |
| Total | 109 | 92 | 201 |

Participants who were employed prior to prison were less likely to be re-sentenced again after release (chi-squared test p-value = 0.068). Of the people who were employed before prison only 39% were re-sentenced compared to 53% who were unemployed before prison.

*Table 57: Re-sentencing across employment prior to prison.*

| | Not re-sentenced | Re-sentenced | Total |
|---|---|---|---|
| Unemployed prior to prison | 49 (47%) | 55 (53%) | 104 (100%) |
| Employed prior to prison | 58 (61%) | 37 (39%) | 95 (100%) |
| Total | 107 | 92 | 199 |

Participants who planned on furthering their education after prison were re-sentenced at a lower rate than those who did not (chi-squared test p-value = 0.0096). This could suggest a different, more hopeful mindset about future plans, perhaps even a better reintegration into society. This could also

[30] This is 199 as the gender diverse/transgender gender options have been removed due to cell size. The gender diverse/transgender identifying people being removed for analysis, as only one could be interviewed post-release leading to unbalanced analytic tables as n = 1 for this category.

be influenced by income, resources available and whether there was support available to provide opportunities to return to study.

*Table 58: Re-sentencing and planned further education after release.*

| | Not re-sentenced | Re-sentenced | Total |
|---|---|---|---|
| No Planned Further Education | 62 (47%) | 70 (53%) | 132 (100%) |
| Planned Further Education | 44 (68%) | 21 (32%) | 65 (100%) |
| Total | 106 | 91 | 197 |

The number of previous imprisonments showed an inverted U-shaped relationship with re-sentencing, where the rate of re-sentencing increased from no previous imprisonments to 3-6 imprisonments then decreased slightly after more than 6 previous imprisonments is reached. There is evidence this is a significant relationship (chi-squared test p-value = 0.0047).

*Table 59: Re-sentencing across the number of previous imprisonments.*

| | | Not re-sentenced | Re-sentenced | Total |
|---|---|---|---|---|
| Number of previous imprisonments | None | 34 (71%) | 14 (29%) | 48 (100%) |
| | 1-2 | 33 (62%) | 20 (38%) | 53 (100%) |
| | 3-6 | 21 (40%) | 32 (60%) | 53 (100%) |
| | More than 6 | 21 (45%) | 26 (55%) | 47 (100%) |
| | Total | 109 | 92 | 201 |

If participants viewed the place they were living at the post-release interview as a home, counter-intuitively they were more likely to be re-sentenced (Table 60). As 7 of the 8 people who were viewed as part of the 'homeless' specifically those who lived 'on the street' called where they were living 'home' this suggests perhaps a greater feeling of safety or comfort from a lack of society, rules and regulation. As discussed in the section '4.9 What Makes a Home' the perception of unstable housing types as a home may have been due to expectations of a lower standard of living, or feelings of freedom from living in less regulated forms of accommodation.

*Table 60: Re-sentencing within a year and the perception of 'home'.*

| | | Re-sentenced | | |
|---|---|---|---|---|
| | | No | Yes | Total |
| Viewed where they were living as a home | No | 28 | 9 | 37 |
| | Unsure | 0 | 1 | 1 |
| | Yes | 20 | 22 | 42 |

| | | | | |
|---|---|---|---|---|
| | Total | 48 | 32 | 80 |

Those who lived in an unstable housing type(s) in the 6 months before prison were more likely than those living in stable housing type(s) to be re-sentenced (chi-squared test p-value = 0.026).

*Table 61: Re-sentencing and housing type prior to prison.*

| | Not re-sentenced | Re-sentenced | Total |
|---|---|---|---|
| In Stable Housing Prior to Prison | 56 (64%) | 32 (36%) | 88 (100%) |
| In Unstable Housing Prior to Prison | 53 (47%) | 60 (53%) | 113 (100%) |
| Total | 109 | 92 | 201 |

Those who were experiencing high residential mobility in the 6 months before prison were also more likely to be re-sentenced compared to those with lower residential mobility (chi-squared test p-value = 0.0051). This is discussed in the 4.4.2 Residential Mobility section. This speaks to the other evidence seen throughout these results, where generally the variables showing disadvantage (lack of education plans, criminal histories, previous unemployment) as well as a history of unstable housing were significantly associated with re-sentencing at the bivariate level.

*Table 62: Re-sentencing and residential mobility prior to prison.*

| | Not re-sentenced | Re-sentenced | Total |
|---|---|---|---|
| Moved 0-1 Times Prior to Prison | 70 (64%) | 40 (36%) | 110 (100%) |
| Moved 2+ Times Prior to Prison | 39 (43%) | 52 (57%) | 91 (100%) |
| Total | 109 | 92 | 201 |

## 4.8 Significant Associations with Re-imprisonment

Once again, there was a significant relationship between recidivism and New Zealand European and Other ethnicity (chi-squared test p-value = 0.012). Participants who identified as having New Zealand European and Other ethnicities were less likely to be re-imprisoned.

*Table 63: Re-imprisonment by New Zealand European and Other ethnicity.*

| | Not re-imprisoned | Re-imprisoned | Total |
|---|---|---|---|
| Not New Zealand European and Other | 86 (68%) | 40 (32%) | 126 (100%) |
| New Zealand European and Other | 64 (85%) | 11 (15%) | 75 (100%) |
| Total | 150 | 51 | 201 |

Conversely, participants who identified as having Māori ethnicity were more likely to be re-imprisoned (chi-squared test p-value = 0.016).

*Table 64: Re-imprisonment by Māori ethnicity.*

| | Not re-imprisoned | Re-imprisoned | Total |
|---|---|---|---|
| Not Māori | 45 (88%) | 6 (12%) | 51 (100%) |
| Māori | 105 (70%) | 45 (30%) | 150 (100%) |
| Total | 150 | 51 | 201 |

Participants who were employed prior to prison were less likely to be re-imprisoned (chi-squared test p-value = 0.011). As seen in the last section employment prior to prison was also significantly associated with re-sentencing. Perhaps owing to greater ties to their communities and the skills from previous employment.

*Table 65: Re-imprisonment and unemployment prior to prison.*

| | Not re-imprisoned | Re-imprisoned | Total |
|---|---|---|---|
| Unemployed prior to prison | 69 (66%) | 35 (34%) | 104 (100%) |
| Employed prior to prison | 79 (83%) | 16 (17%) | 95 (100%) |
| Total | 148 | 51 | 199 |

There was no clear relationship between mental health and re-imprisonment, despite the association being significant (Fisher's exact t-test p-value = 0.034). Although the participants who were identified as 'moderately severe' were the group with the least re-imprisonments, the people who were identified as 'severe' by the PHQ-9 questionnaire in the pre-release interview were all re-imprisoned within a year and those whose PHQ-9 questionnaire identified 'None' to 'Moderate' were all re-imprisoned at similar rates.

*Table 66: Re-imprisonment and PHQ-9 depression score pre-release.*

| | | Not re-imprisoned | Re-imprisoned | Total |
|---|---|---|---|---|
| PHQ-9 depression score in pre-release interview | Severe | 0 (0%) | 3 (100%) | 3 (100%) |
| | Moderately severe | 13 (93%) | 1 (7%) | 14 (100%) |
| | Moderate | 14 (74%) | 5 (26%) | 19 (100%) |
| | Mild | 35 (71%) | 14 (29%) | 49 (100%) |

| | | | | |
|---|---|---|---|---|
| | None | 86 (76%) | 27 (24%) | 113 (100%) |
| | Total | 148 | 50 | 198 |

As seen for re-sentencing, participants who planned on furthering their education after prison were re-imprisoned at a lower rate than those who did not. This relationship shows some evidence of significance (chi-squared test p-value = 0.082). Suggesting perhaps a different, more hopeful mindset about future plans.

*Table 67: Re-imprisonment and planned further education pre-release.*

| | Not re-imprisoned | Re-imprisoned | Total |
|---|---|---|---|
| No planned further education | 93 (70%) | 39 (30%) | 132 (100%) |
| Planned further education | 54 (83%) | 11 (17%) | 65 (100%) |
| Total | 147 | 50 | 197 |

There was very strong evidence of a relationship between the number of previous imprisonments and being re-imprisoned within 12 months (chi-squared test p-value = 0.0024). The number of previous imprisonments showed a positive relationship with re-imprisonment, where the rate of re-sentencing increased with the number of previous imprisonments. This suggests that the participants with more serious criminal histories may be committing more serious crimes within a year of release, or their previous imprisonments are weighing against them to result in a more serious sentence of imprisonment. From the data provided by Corrections, it could be seen that all participants who were re-imprisoned received at least one re-sentencing. Of those who were re-sentenced within a year:

- with no previous imprisonments 43% were re-imprisoned;
- with 1-2 previous imprisonments 40% were re-imprisoned;
- with 3-6 previous imprisonments 59% were re-imprisoned; and
- with more than 6 previous imprisonments 69% were re-imprisoned.

*Table 68: Re-imprisonment by the number of previous imprisonments.*

| | | Not re-imprisoned | Re-imprisoned | Total |
|---|---|---|---|---|
| Number of previous imprisonments | None | 42 (88%) | 6 (13%) | 48 (100%) |
| | 1-2 | 45 (85%) | 8 (15%) | 53 (100%) |
| | 3-6 | 34 (64%) | 19 (36%) | 53 (100%) |
| | More than 6 | 29 (62%) | 18 (38%) | 47 (100%) |
| | Total | 150 | 51 | 201 |

The same unusual relationship observed between perception of home and re-sentencing was present for re-imprisonment (Table 69). If participants viewed the place they were living at the post-release interview as a home, they were re-imprisoned at higher rates than those who did not view it as a home. As discussed in the previous section this may have been due to expectations of a lower standard of living or perhaps increased feelings of autonomy and freedom from living in less stable and regulated forms of accommodation.

*Table 69: Re-imprisonment within a year and the perception of 'home'.*

| | | Re-imprisoned | | |
|---|---|---|---|---|
| | | No | Yes | Total |
| Viewed where they were living as a home | No | 33 | 4 | 37 |
| | Unsure | 0 | 1 | 1 |
| | Yes | 27 | 15 | 42 |
| | Total | 60 | 20 | 80 |

Participants who had housing support were re-imprisoned slightly less than those who did not receive help (Fisher's exact t-test p-value = 0.029). However, the real difference came from those who did not need help at all after release. The most striking factor for avoiding re-imprisonment within a year was not needing housing support in the 6 months after release from prison. This could show a difference due to factors providing an advantage such as: wealth, assets, relationships, or skills available to them.

*Table 70: Re-imprisonment by housing support received post-release.*

| | Not re-imprisoned | Re-imprisoned | Total |
|---|---|---|---|
| Have not needed housing support | 14 (100%) | 0 (0%) | 14 (100%) |
| Did not receive housing support | 28 (67%) | 14 (33%) | 42 (100%) |
| Received housing support | 17 (74%) | 6 (26%) | 23 (100%) |
| Total | 59 | 20 | 79 |

By the post-release interview a new relationship appeared between mental health after release and re-imprisonment. There was some evidence of relationship between mental health after release and re-imprisonment (Fisher's exact t-test p-value = 0.070). There was an inverted U-shaped relationship where the percentages of each group re-imprisoned increased from 'None' to 'Moderate', then decreased until 'Severe'.

*Table 71: Re-imprisonment and the PHQ-9 depression score post-release.*

| | | Not re-imprisoned | Re-imprisoned | Total |
|---|---|---|---|---|
| PHQ-9 depression score in post-release interview | Severe | 3 (100%) | 0 (0%) | 3 (100%) |
| | Moderately severe | 3 (75%) | 1 (25%) | 4 (100%) |
| | Moderate | 5 (50%) | 5 (50%) | 10 (100%) |
| | Mild | 8 (57%) | 6 (43%) | 14 (100%) |
| | None | 41 (84%) | 8 (16%) | 49 (100%) |
| | Total | 60 | 20 | 80 |

There was some evidence that participants who took part in programmes after release were re-imprisoned less than those who did not (chi-squared p-value =0.091). Only 14% of people who participated in programmes after release were re-imprisoned compared to 33% of those who did not.

*Table 72: Re-imprisonment and post-release treatments/programmes.*

| | Not re-imprisoned | Re-imprisoned | Total |
|---|---|---|---|
| Did not participate in programmes/treatments post-release | 30 (67%) | 15 (33%) | 45 (100%) |
| Participated in programmes/treatments post-release | 30 (86%) | 5 (14%) | 35 (100%) |
| Total | 60 | 20 | 80 |

Those who were living in unstable housing type(s) in the 6 months before prison were more likely to be re-imprisoned than those living in stable housing type(s) (chi-squared p-value =0.011).

*Table 73: Re-imprisonment and housing type prior to prison.*

| | Not re-imprisoned | Re-imprisoned | Total |
|---|---|---|---|
| In Stable Housing Prior to Prison | 74 (84%) | 14 (16%) | 88 (100%) |
| In Unstable Housing Prior to Prison | 76 (67%) | 37 (33%) | 113 (100%) |
| Total | 150 | 51 | 201 |

Similarly, those who experienced high residential mobility in the 6 months before prison were more likely to be re-imprisoned (chi-squared p-value =0.016). This again provides evidence that indicators of disadvantage such as lack of education plans, criminal histories, previous unemployment, and a history of unstable housing are significantly associated with re-imprisonment at the bivariate level.

*Table 74: Re-imprisonment and residential mobility prior to prison.*

| | Not re-imprisoned | Re-imprisoned | Total |
|---|---|---|---|
| Moved 0-1 times prior to prison | 90 (82%) | 20 (18%) | 110 (100%) |
| Moved 2+ times prior to prison | 60 (66%) | 31 (34%) | 91 (100%) |
| Total | 150 | 51 | 201 |

## 4.9 What Makes a Home?

Whether the place a participant was living felt like a 'home' was recorded to better understand reintegration into the community. Homeless Link (2011) cited safe accommodation – what might be interpreted as a home – as being crucial to breaking cycles of recidivism. Homeless Link (2011) went on to explain safe accommodation gives a person something to lose, as otherwise prison can seem more appealing than homelessness. In post-release interviews participants were asked whether they thought the place they were living at the time of interview (or prior to returning to prison when re-interviewed in prison) was a 'home'; specifically, participants were asked "Did you consider this to be *your* home? Was it somewhere you feel you can be yourself?". It was of interest as to whether the stable housing measures used were related to stable housing to better assess their adequacy. Moreover, there was an interest in what stable housing may mean to people recently released from prison in New Zealand. The relationship between both stable housing and recidivism measures was unexpected. Further it called into question the relationship between the perception of 'home' to a person who had been to prison and the concept of stable housing.

The research participants answered whether they felt the place they were living felt like a home to them. Stable types of housing and/or low residential mobility may be assumed as a proxy to creating a home environment. Surprisingly though this did not show through from the information collected from this study. Perceptions of home had significant relationships with both residential mobility and stability of housing type (Fisher's exact test p-values of 0.0086 and 0.069 respectively), though not in a manner expected.

Those who moved at least two times in the 6 months after release were significantly more likely to call the place they were staying at the time of interview a home. This could have been due to moving around to end up in a place they found to be a home, or perhaps they may have appreciated a place they were staying more, after having to move many times.

*Table 75: Residential mobility and the perception of 'home'.*

| | | Moved 0-1 times post-release | Moved 2+ times post-release | Total |
|---|---|---|---|---|
| Viewed where they were living as a home | No | 27 | 10 | 37 |
| | Unsure | 0 | 1 | 1 |
| | Yes | 18 | 24 | 42 |
| | Total | 45 | 35 | 80 |

Those who were in unstable housing types were again significantly more likely to call the place they were staying a home if they had stayed in unstable housing types in the 6 months after release. However, the condition of the housing was not recorded, this may have played a large role in the perception of home in cases of participants living in owned, rented, state housing, or Housing First/NGO accommodation. This relationship could also be due to a lack of better accommodations leading participants to have lower expectations of housing. Perhaps the housing types away from past relationships, such as hotels and motels, allow for a freedom regarded as a home without social restrictions.

*Table 76: Housing type and the perception of 'home'.*

| | | Lived in stable housing type(s) post-release | Lived in unstable housing type(s) post-release | Total |
|---|---|---|---|---|
| Viewed where they were living as a home | No | 17 | 20 | 37 |
| | Unsure | 0 | 1 | 1 |
| | Yes | 10 | 32 | 42 |
| | Total | 27 | 53 | 80 |

Perceptions of home also presented unexpected associations with re-sentencing (Table 60) and re-imprisonment (Table 69), which were both statistically significant (Fisher's exact test p-values of 0.012 and 0.0052 respectively). It seems recidivism was not curbed despite participants referring to the place they had been living a home for approximately 6 months after release. This could indicate the restrictions and expectations placed upon people who were living in more stable types of housing meant they did not feel the accommodation was a home but did provide enough stability or re-integration and connection to society to prevent recidivism. The vast majority of people who were living 'on the street' viewed their living situation as a home despite people living 'on the street' being the most likely to be re-sentenced and re-imprisoned.

There was one significant relationship between who participants were living with and whether they viewed the place they were living in as a home. Participants who were living alone were significantly more likely to view the place they were staying as a home (chi-squared p-value = 0.038). Reinforcing the idea of home as a space a person feels they have autonomy over, a space free of societal pressures.

*Table 77: Living alone and the perception of 'home'.*

| | Did not think, or were unsure if the place they were staying was a home | Viewed the place they were staying as a home |
|---|---|---|
| Living alone | 3 | 12 |
| Not living alone | 35 | 30 |

Perceptions of home have made it difficult to draw any hardline conclusions about what makes stable housing based purely upon these measures. It appears the concept of 'stable housing' may not necessarily align with the perception of a living situation as a home. Although perceiving a place a participant was living to be a home may have been influenced quite heavily by the addition to the question suggesting a home to be "somewhere you feel you can be yourself". Homeless Link (2011) emphasised the safety of housing, which may be a stronger influence of home. Further study into what constitutes stable housing and how it relates to the concept of a home, would be invaluable to future research in this area. The research into stable housing would be especially important for Māori due to the large number who return to live with friends/family, where 43% of our research participants planned on living after release.

# 5. Discussion

There was evidence of an association between some of the housing stability measures and recidivism in the bivariate relationship tests. Participants who reported living in an unstable housing type or having high residential mobility approximately 6 months after release were between 1.5 to 4.6 times more likely to reoffend. These associations became stronger when the data was adjusted to better represent non-responders. There was evidence of an association between stable housing type after prison and re-imprisonment in bivariate analysis in both the data available and the data set re-weighted for non-response (chi-squared p-values of 0.020 and 0.0072 respectively). Some evidence was also present of an association between residential mobility and re-sentencing, although the bivariate association was not significant in the data available it was significant in the data set re-weighted for non-response (Chi-squared test p-values of 0.11 and 0.078 respectively).

Despite some evidence of associations between stable housing measures and recidivism in bivariate analysis the association was not significant when other suspected confounding variables were included in the model. There was almost no evidence of a significant causal effect of any stable housing measure on any recidivism measure when confounding was adjusted for. This suggests causal effects of stable housing, if any, may be largely explained away by other suspected confounding variables (Skelly et al., 2012). However, it seems likely this could have resulted from the small sample size that was retained through to the post-release interview not lending enough power to the analysis.

Adjustment for ‘DAG’ and ‘Ideal’ sets of confounding variables resulted in very similar estimated effects of unstable housing types in the 6 months after release and both measures of recidivism. This could mean the ‘DAG’ confounding variable set identified the most significant confounding variables or the variables identified through the DAG acted in similar ways to those in the ‘Ideal’ confounding variable adjustment set. Despite evidence of a relationship between the housing type a participant was living in the 6 months after release and re-imprisonment in bivariate analysis, there was no evidence of a significant relationship after any suspected confounding set was adjusted for. There was no evidence of a significant relationship after suspected confounders were adjusted for as confidence intervals for all estimates straddled zero (Figure 7).

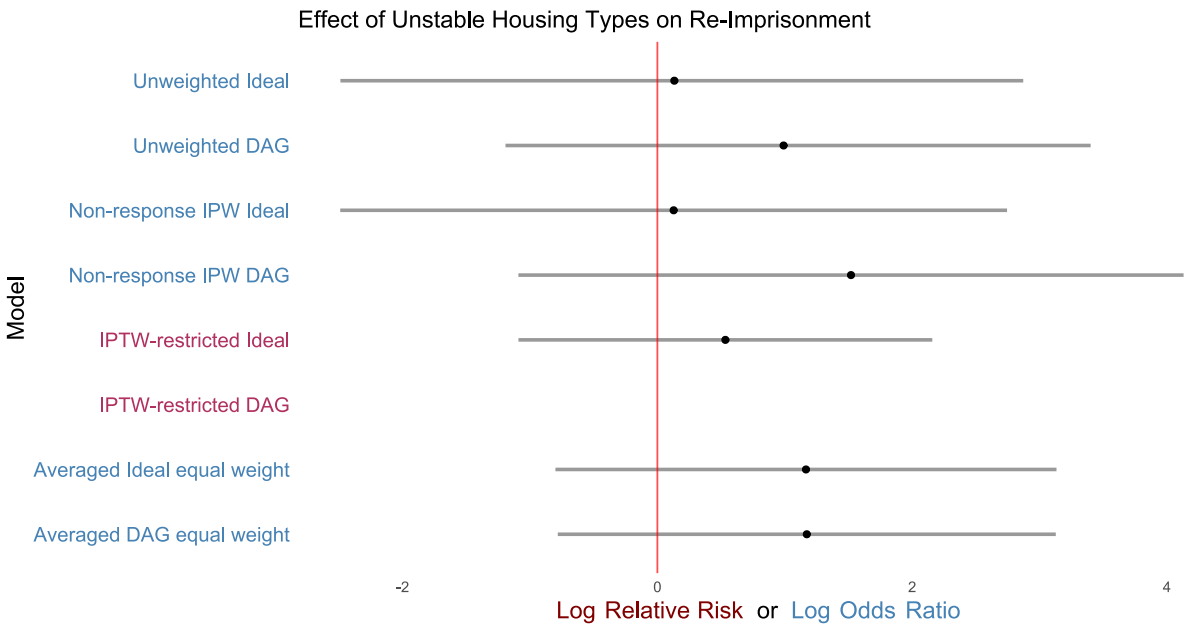


*Figure 7: Estimated effects of unstable housing types and re-imprisonment with their confidence intervals. The IPTW-restricted DAG has been removed due to no satisfactory balance being found across the 'DAG' confounding variable set.*

Similarly, there was no evidence of a significant relationship between unstable housing type and re-sentencing after suspected confounders were adjusted for as confidence intervals for all estimates straddled zero (Figure 8).

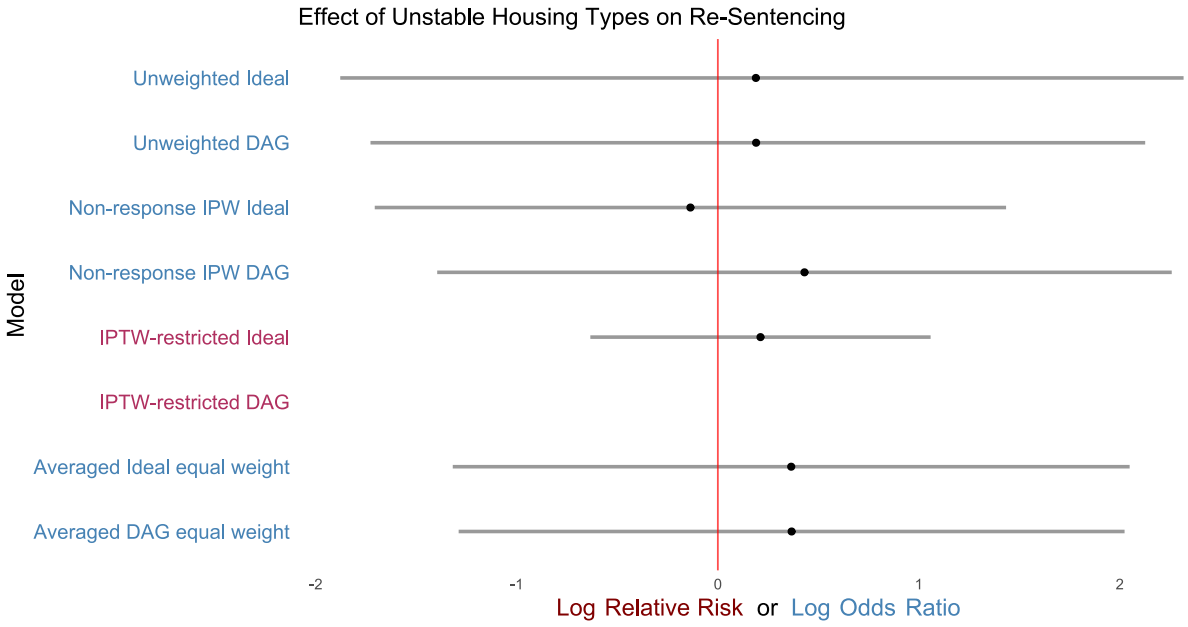


*Figure 8: Estimated effects of unstable housing types and re-sentencing with their confidence intervals. The IPTW-restricted DAG has been removed due to no satisfactory balance being found across the 'DAG' confounding variable set.*

Adjustment for 'DAG' and 'Ideal' sets of confounding variables resulted in slightly different estimated effects of high residential mobility in the 6 months after release and both measures of

recidivism. This could mean the 'DAG' confounding variable set did not identify confounding variable(s), or the variables identified in the 'Ideal' confounding variable adjustment set. The 'Ideal' confounding set could have included many more variables, which may have had many small impacts on the estimated effects and increased the standard error. Despite the set of confounding variables adjusted for, there was very little evidence of a significant relationship after any suspected confounding set was adjusted for. There was no evidence of a significant effect of high residential mobility on re-imprisonment after suspected confounders were adjusted for as confidence intervals for all estimates spanned zero (Figure 9).

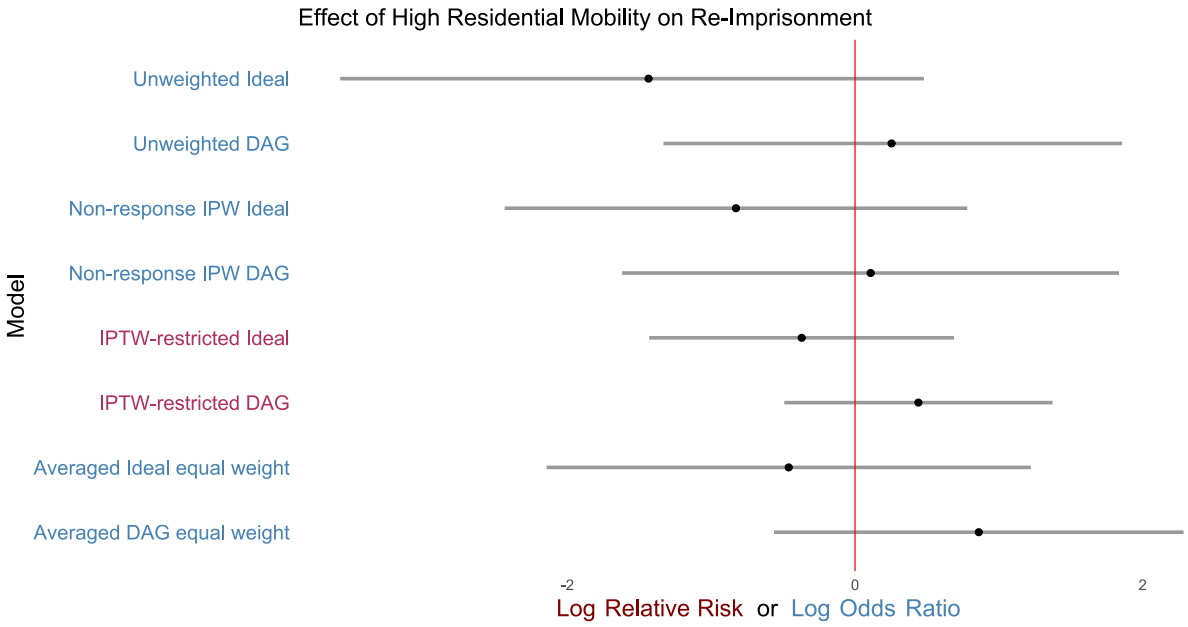


*Figure 9: Estimated effects of high residential mobility and re-imprisonment with their confidence intervals.*

The effect of high residential mobility on re-sentencing was only significant using IPTW when the data set was balanced across suspected confounding variables identified by the DAG (Figure 10). However, the IPTW for data available on less than 100 participants is sub-optimal (Raad et al., 2020) and relies heavily upon correctly having identified and measured confounding variables (Mansournia & Altman, 2016). It is possible the confounding variables identified as having significant bivariate relationships in a DAG (as explained in the section on 2.2.5 Confounding) were not a sufficient set of confounding variables unlike the 'Ideal' set of confounding variables. This result is not far-fetched as the estimated effects of the IPTW model with a DAG identified set of confounding variables overlap with all other model estimates. However, due to the estimated confidence intervals for the effects of high residential mobility overlapping zero for all other models there is little evidence for a significant effect of high residential mobility on re-imprisonment.

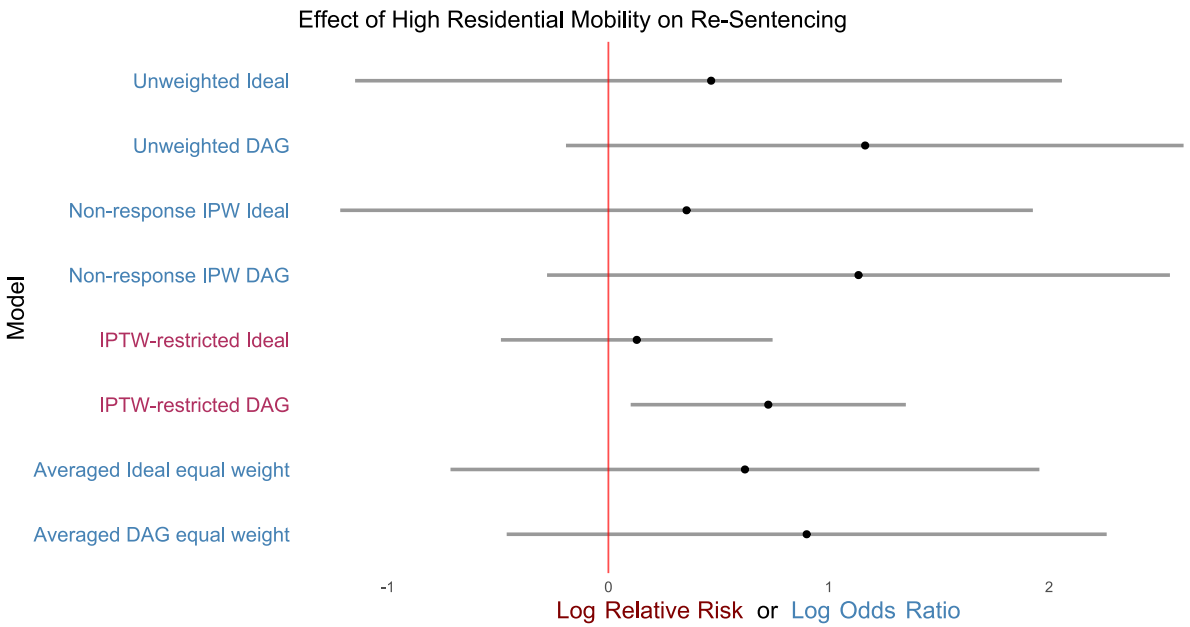


*Figure 10: Estimated effects of high residential mobility and re-sentencing with their confidence intervals.*

Of the New Zealand population Stats NZ estimated over 41,000 to be 'homeless' (2020b). Of the total New Zealand population 0.07%[31] were estimated to be without shelter, while 9% of our sample were without shelter 'on the street' before prison, rising to 10% of those found for the post release interview. Moreover, although homeownership was around 65% in 2018 (Stats NZ, 2020a) only 7% of our sample were living at an owned or partly owned home before their prison sentences. There was no significant evidence of an association between living in unstable housing types in the 6 months prior to prison and in the 6 months post-release.

*Table 78: Housing type prior to prison and housing type 6 months post-release.*

| | Lived in stable housing type(s) post-release | Lived in unstable housing type(s) post-release | Sum |
|---|---|---|---|
| Lived in stable housing type(s) prior to prison | 17 (42.5%) | 23 (57.5%) | 40 (100.0%) |
| Lived in stable housing type(s) prior to prison | 10 (25.0%) | 30 (75.0%) | 40 (100.0%) |
| Sum | 27 (33.8%) | 53 (66.3%) | 80 (100.1%) |

However, there was significant evidence of an association between 'living on the streets' in the 6 months prior to prison and 'living on the streets' in the 6 months post-release (Fishers exact p-value = 0.00026). Of those who were retained for the post-release interview, only 3 of the 7 who had

[31] (3,522/4,300,000*100)

experienced ‘living on the streets’ before prison did not return to ‘living rough’ in the 6 months after release.

*Table 79: Homelessness prior to prison and homelessness 6 months post-release.*

| | Not actually homeless 6 months after | Actually homeless 6 months after | Sum |
|---|---|---|---|
| Not homeless before | 69 (94.5%) | 4 (5.5%) | 73 (100.0%) |
| Homeless before | 3 (42.9%) | 4 (57.1%) | 7 (100.0%) |
| Sum | 72 (90.0%) | 8 (10.0%) | 80 (100.0%) |

There was evidence that ‘living on the streets’ after release was significantly associated with recidivism, as measured by both re-sentencing and re-imprisonment (Fishers exact p-value = 0.054 and 0.021 respectively). Moreover, those who were ‘living rough’ seemed to receive re-imprisonment more often than participants living elsewhere. Of the 6 who were re-sentenced 5 were re-imprisoned, compared to 15 re-imprisoned of the 26 who were re-sentenced who lived elsewhere.

What makes a home? This study suggests supplying ‘stable housing’ does not a home make. The view of ‘stable housing’ and what makes a ‘home’ undoubtably comes from a Eurocentric view. Researchers like Workman (2014) questioned how much Māori and pākehā concepts such as rehabilitation align in more recent times especially when considering whānau. It could be that the same approaches do not fit with Māori perspectives. This could make understanding stable housing as a concept very difficult to describe as a binary data value. As results show in this paper, less stable housing types were more often perceived as a ‘home’ by participants. Despite seeing where they were living as a home, they did not show evidence of a desire to retain these less stable housing types. People living in a place they thought of as a home were instead found to reoffended in higher numbers.

New Zealand Māori were 1.7 times more likely to be in unstable housing types. This will be strongly related to housing with family/friends being categorised as an unstable housing type. Of the 38 people who were recorded as living with family/friends, 30 identified as Māori. Although it could be tempting to put the issue of housing instability for New Zealand Māori down to how unstable housing types were categorised, this trend of instability of housing continues for New Zealand Māori in terms of residential mobility. New Zealand Māori were 2.4 times more likely to have moved at least twice in approximately 6 months after release. Both measures of housing stability, housing type and residential mobility showed greater instability for Māori.

Of the Māori in our pre-release sample 45% planned on living with friends/family after release and 53% of the Māori in the post-release sample had left prison to live with friends/family (compared to 37% and 48% respectively for non-Māori). This suggests a point should be made to support the friends and family taking prison releasees into their homes. The large number of people leaving prison who return to live with family/friends in the 6 months after release could suggest either a lack of options elsewhere for them and/or a greater willingness and community effort to support them after release. Perhaps the focus should be moved from helping the individual to working within the context of a support system of family and friends. Receiving and caring for prison releasees should be focused on as a group effort, as it seems to be, with support applied in the same way (Mills et al. 2021).

Living with friends and family could be a very stable and supportive housing type. However, as this housing type comes with the people living there, this housing type will be dependent on the positive or negative influences of the friends and family they stay with. If this study were to be run again it is suggested to gather more detailed information breaking down the family and friends category, with more information about the type of place they were staying and who they were staying with. Prison releasees may have positive support systems waiting with their family and friends. However, in some cases a split from families may be desirable as intergenerational recidivism may play a part in the difficulty of breaking away from crime (Workman, 2014). Separation from families may even be enforced by the courts in order to separate prison releasees from family members with criminal histories (Mills et al. 2021).

This is not to say that there should not be support for the individual leaving on their own as well. There are times when people leaving prison may wish to break from former toxic relationships or start fresh relationships upon release. Although a family may be supportive, they may be unable to support another person, especially if they are returning home with conditions on their release. Moreover, they may not be able to return home because of these conditions. Other housing options may be limited, especially if they have a history of arson as it may mean anywhere they stay may lose their home insurance (Consumer NZ, 2014). Landlords may not want to rent to people due to their criminal histories. Furthermore, with growing requirements for rental housing applications, such as requiring previous bank statements, it may prevent prison releasees from securing rental housing from any other source than family or friends. The prisoners themselves may even turn down, or be rejected from, housing support programmes if they do not meet, or do not want to accept, the criteria or requirements to partake in the housing available. Furthermore, the releasee may not agree to enforced requirements about how to reconnect with society.

This study has provided some contrary associations to what might have been expected based on other studies. For example, there was no evidence of self-reported substance abuse being significantly associated with either measure of recidivism, contrary to what was found by Baldry et al. (2006). Moreover, although not a significant relationship, men who participated in the post-release interview were more likely than women to have experienced high residential mobility (49% and 32% respectively) and have lived in unstable housing types (70% and 54% respectively). This directly contrasted with the Australian research of Baldry et al. (2002) who found women experienced a harder time finding housing after being released from prison.

The highest rates of re-imprisonment were observed in people who were homeless (Appendix, Table 84), followed by hotels and state housing. However, these were both very small samples. A group with more evidence behind them, people living with friends were also more likely than the overall sample to be re-imprisoned. Of people living with friends and family 29% re-imprisoned, compared to the 25% average for all participants.

The group with the highest rate of re-sentencing were homeless with 6 out of 8 people being re-sentenced (Appendix, Table 85), followed by housing first/NGO, state, and hotel housing. Of people living with friends and family, 42% were re-sentenced, just above the 40% average. The three people living in a hostel/boarding house/backpackers/campground/holiday park were the only ones who were not re-sentenced or re-imprisoned at all. However, this result could be a quirk of a very small sample size.

## 5.1 Study Limitations

As discussed in the Methodology section, this analysis sought to estimate the causal effect of stable housing on recidivism by adjusting for confounding. Ideally, there would have been information available, measured and able to be adjusted for, on all suspected confounders: age, gender, ethnicity, previous housing, housing support/programmes, social support, financial situation, mental health, substance abuse, region, criminal history, sentence length and employment/education. Unfortunately, this study lacked any information on financial situation or region. Moreover, confounders especially typically unobserved ones, such as social support may not have been properly measured by perceived family supportiveness and whether a person planned to live with their family or partner. Although confounding was controlled as best possible,

residual confounding may still be present due to unmeasured variables such as region and financial situation as well as any inadequate measures.

Unfortunately, although poverty was identified in other research as to why prison releasees are unsuccessful in reintegrating into society (Baldry et al., 2002; Lutze et al., 2013; Metraux & Culhane, 2004), no information was collected on financial situation in the interviews. Their financial situations could have contributed to buying or renting a house and repairing or personalising a home. Information on financial status and debts would be desirable in future research of this kind.

Existing research on recidivism and reintegration often focuses on prison releasees who return to more urban areas. Perhaps this is because more intensive programmes have been generally set up in urban regions, with more access to support systems, such as the Washington states re-entry housing pilot program investigated by Lutze et al. (2014). However, rural regions come with their own hurdles such as limited housing stock and a lack of employment opportunities (Groot & Mace, 2016). Information on the region a person lived after release would have been valuable and informative to this study.

Even with only the key sets of suspected confounding variables adjusted for in analysis, more variables were included in the model than would usually be suggested. With the relatively small number of participants who participated in the pre-release and post-release interviews, once all variables from possible confounding variable sets are accounted for, the sample size rule of 10 observations for each independent variable (Harrell, 2010) is violated. In future research if the effect of 'stable housing' is desired it is recommended to have a much larger sample size. This could mean there are enough observations to properly adjust for suspected confounding, even after losses in follow-up interviews.

Due to the causal interest of the research, a holistic view of other factors and how they interact in a temporal nature must be considered. Questions about variables of interest were asked over varying time periods meaning issues arose with what could be used as a confounder due to their temporal relationships. Information about employment for example was gathered for the same time period as stable housing, over the 6 months after release from prison. However, variables of interest such as mental health and substance abuse after release cannot be assumed to affect stable housing as these variables related to a period two weeks and three months prior to interviews rather than approximately 6 months prior to interview. It would be desirable in future studies to survey the

variables of interest for each place a participant lived if a releasee was living with person(s) and had lived in more than one place since release. Furthermore, the amount of time between leaving prison and being interviewed should be considered important and standardised if possible rather than being treated as just as a gap in time between release and the post-release interview.

Ideally all people who were being released from the prisons of interest within the period set aside for interviews would have been available and agreed to interviews. However, at each round of interviews participants could choose to decline to participate in the study and the prisons may deny access to people who were uncontactable, for example if they were in isolation. Not being able to gather information on everyone who left the prisons involved in the study could have resulted in bias in the study in unknown ways.

Unfortunately, the loss of people eligible for the study who declined to participate was unknown. No information was recorded on those of the eligible population who did not agree to participate in the study. The sampling frame for this research was of people in New Zealand prisons who were released in 2019. However, people were only eligible if they were being released within 4 weeks of interviewers visiting a prison. The population of people who are soon to be released was not believed to necessarily be represented by all those who are in prison. There was thought to be no adequate data available on those due for release, as data reported from snapshots of the full prison population may not represent the prison population being released.

The loss to follow-up was high for this study. Despite the best efforts of the study team, only 40% of the sample could be found and interviewed approximately 6 months after release. As a consequence, the power to detect effects and associations was not as high as hoped. Research of ex-offenders by Baldry et al. (2006) and Brunton-Smith and Hopkins (2013) who waited for shorter periods between release from prison and the first interview, reported better retention of the sample. For future research shorter periods between release and follow-up interviews may provide better retention of the sample.

Incorporating a survey design to the analysis which accounted for the clustered sampling nature of the prisons visited was considered for this analysis. However, it seems likely issues would arise due to prisons not being chosen at random but instead chosen under direction from Corrections so prisons did not have an equal chance of being chosen. As a result, the probabilities of a prison being chosen were unknown. Moreover, information was not gathered about the people who turned down the pre-

release interview and so we have no knowledge of the probability of participation within each prison cluster.

The application of survey designs using the "survey" package in R (Lumley, 2020) was considered. However, as the prisons were not chosen at random but rather decided during the planning stage between the PI and Corrections this approach is not considered appropriate. Moreover, it was unknown how likely it may have been for other prisons and people within them to be visited and interviewed respectively. Analyses taking account of the survey design were not pursued.

## 5.2 Future Research

The definition of 'stable housing' appeared inconsistent with perceptions of 'home' in the participants of this study. This could call into question the measures used for stable housing or differing opinions of what home is. Consequently, research that specifically focusses on what might constitute stable housing and how this relates to concepts of home is suggested.

Accommodation with 'family and friends' did not seem a stable housing type as may be expected. This whole category could be better understood if it was examined as two categories, one for 'family' and a separate one for 'friends' to better identify how these relationships may effect recidivism. Perhaps even a framework for better identifying the space an ex-offender inhabited and the social framework available to them could be implemented as in the paper by Corden et al. (1979). Moreover, it would be helpful to investigate released prisoners feelings on going to stay with friends and family as they can be good or bad influences. People can also struggle with returning to friends and family as they may want to dis-establish contact. The effect of displacement after the release could also be gathered to investigate how relocation away from influences of social networks could impact reintegration.

This study found participants who were younger, attended certain prisons, and did not have conditions on where they could go as part of their sentence were less likely to be contactable to respond to post-release interviews. Age had the most significant relationship with non-response to post-release interviews. To minimise non-response in future research it would be desirable to make additional efforts to retain the younger group of people who are leaving prison.

In a bid to capture an engaged larger sample, perhaps new techniques could be applied to ensure participants are retained in higher numbers. If one study were to be run using a prison local to a

researcher, they may be able to gather consistent information about people being released. Shorter periods between follow-up may promote greater engagement with the study. If survival analysis is desired, then ceasing follow-up once a chosen measure of recidivism occurs would allow participants to slowly remove themselves from the study. In future studies for greater retention, if participants who are held at prisons not originally given access to, it would be ideal if an interview sheet could still be sent and requested to be filled out.

## 5.3 Conclusion

There was evidence of an association between some of the housing stability measures and recidivism in bivariate relationship tests. In each case living in unstable housing during the approximately 6 months after release increased recidivism. Participants living in unstable housing were between 1.5 to 4.6 times more likely to reoffend. These associations became stronger when the data was adjusted to better represent non-responders. If these non-responders were well represented by the people who remained in the sample, the bivariate results would suggest retaining more of the original sample would increase the certainty of a bivariate relationship between stable housing increasing recidivism.

Four analytic techniques were implemented that all rely on identification, measurement and adjustment for all confounding. If the confounding variables adjusted for in the models were not correct the estimated effects of stable housing on recidivism may not be correct (Burnham & Anderson, 2002). Further, each technique made different assumptions about the missingness of the participants from the post-release interview:

1. *Logistic regression* with covariate adjustment used complete case analysis. This method requires the missing data from participants who could not be found for post-release interviews to be 'Missing Completely at Random' in order to produce correct effects of stable housing on recidivism.
2. *Model averaging* across covariate adjusted logistic regressions takes into account the large number of suspected confounding variables used in the previously mentioned logistic regression with covariate adjustment. This technique has the same weakness for missing data of research participants. However, it averages the effects of subsets of the full model formula used in logistic regression. This method accounts for uncertainty about the true set of confounding variables by averaging over possible model subsets.
3. *Non-response weighted logistic regression* with covariate adjustment assumes non-response results in 'Missing at Random' data, where missingness can be predicted by other

observed and measured variables. This technique assumes the reasons for non-response can be used to find a probability of response and re-weights the responders to be representative of themselves and those who did not respond. This technique would be correct so long as responders who were weighted to represent others act in similar ways to the people they are supposed to represent. This technique carries out logistic regression with covariate adjustment with a re-weighted data set.

4. *Inverse Probability Treatment Weighting* (IPTW) adjusting for covariate adjustment sets aims to take the data available and re-weight the data set to have a balanced distribution of stable and unstable housing across confounding variables. The re-weighted data, if balanced, should then create a pseudo-randomised data set which may represent what might be expected of a randomised trial. Logistic regression is then used to estimate the effect of stable housing on recidivism. This method makes use of the complete cases that are available. However, as it balances across confounding variables, so long as all confounding variables are included and well measured, the missing data should not be missed if it is 'missing completely at random' or 'missing at random'.

Despite some evidence of significant associations between stable housing measures and recidivism in bivariate analyses the effects of stable housing did not remain significant when other suspected confounding variables were incorporated. This result did not change using any of the other analytic techniques. There was no evidence of a significant causal effect from either stable housing measures (housing type or residential mobility) on either recidivism measures (re-sentencing or re-imprisonment) when confounding was adjusted for in any of the four techniques mentioned above. Across almost all models the confidence interval for the estimate of the effect of stable housing on recidivism spanned the value of zero. This suggests either the sample size was too small to identify significant causal effects of stable housing, or these effects may be explained away by other suspected confounding variables (Skelly et al., 2012).

The models created using the 'Ideal' set of confounding variables were chosen using expert opinion. The 'Ideal' set of confounding variables incorporate variables thought to effect both stable housing and recidivism identified using both scientific research and experience. This set of variables should extend as a generalisation to the wider prison population. However, the models created using the set of 'DAG' identified confounding variables is unlikely to extend to the wider prison population. These variables will only include generalisable confounding variables if the associations between variables remain in the wider prison population.

The similarity between the estimated effects of stable housing measures on recidivism between covariate adjusted logistic regression both unweighted and with non-response weighting suggest little bias was introduced by people who were MAR. There could still be bias introduced that was MNAR. From the information available, assuming people with similar characteristics will have similar outcomes, there was no significant evidence of bias due to people who were not present in the post-release interview and MAR.

IPTW models have consistently smaller confidence intervals despite using the sandwich variance estimator which requires fewer assumptions. Re-weighting resulted in a seemingly larger data set. This gave the appearance of greater surety to the estimated effects of stable housing on recidivism despite the sandwich variance estimator being used. Since sample size initially is not very large, a few people will often represent many in the re-weighted data set. Despite the seemingly greater power of IPTW, these results should be used with caution due to the small sample size (Raad et al., 2020). Moreover, IPTW is sensitive to misspecification of confounding variables used to estimate the propensity scores and can result in biased estimates (Olmos & Govindasamy, 2015). Although our confounding variables were chosen using knowledge of the field (the 'Ideal' set of confounding variables) and knowledge gained from our specific sample (the 'DAG' set of confounding variables), there is no way to be sure all confounding variables were identified, measured well and recorded. Despite this, the confidence intervals provided by IPTW models fitting within those of other analytic techniques is promising.

For now, residential mobility was used as one measure of stable housing and housing type was used as the other. These measures are actionable as the number of moves may be controllable by Corrections with supervision and further, these housing types are some that may be most likely to be provided. However, what actually constituted a 'home' seemed counter-intuitive to what was measured by the 'stable housing' measures. It may be that participants with worse housing histories were more likely to latch on to a lesser housing as a 'home' in our study, or perhaps those in less stable housing types felt they had more freedom and valued different things as home. Although our 'stable housing' measures have good reason to be chosen, questions might be raised about whether/how much 'stable housing' is related to perceptions of 'home'.

# References


Allison, P. (2021, August). *When Can You Safely Ignore Multicollinearity?* Statistical Horizons. Retrieved 5 January 2022, from https://statisticalhorizons.com/multicollinearity

Baldry, E., McDonnell, D., Maplestone, P., & Peeters, M. (2002). *Ex-prisoners and accommodation: what bearing do different forms of housing have on social reintegration for ex-prisoners?* Australian Housing and Urban Research Institute. https://www.ahuri.edu.au/sites/default/files/migration/documents/AHURI_Positioning_Paper_No27_Ex_prisoners_and_accomodation.pdf

Baldry, E., McDonnell, D., Maplestone, P., & Peeters, M. (2003). Australian Prisoners' Post-release Housing. *Current Issues in Criminal Justice*, *15*(2), 155–169. https://doi.org/10.1080/10345329.2003.12036287

Baldry, E., McDonnell, D., Maplestone, P., & Peeters, M. (2006). Ex-Prisoners, Homelessness and the State in Australia. *Australian & New Zealand Journal of Criminology*, *39*(1), 20–33. https://doi.org/10.1375/acri.39.1.20

Boomen, M. & Corrections. (2018). *Where New Zealand stands internationally: A comparison of offence profiles and recidivism rates* (Volume 6 Issue 1: July 2018). https://www.corrections.govt.nz/resources/research/journal/volume_6_issue_1_july_2018/where_new_zealand_stands_internationally_a_comparison_of_offence_profiles_and_recidivism_rates

Bradley, K. H., Oliver, R. B. M., Richardson, N. C., Slayter, E. M., & Community Resources for Justice, Inc. (2001, November). *NO PLACE LIKE HOME: Housing and the Ex-prisoner*. https://www.cjinstitute.org/assets/sites/2/2001/05/54_No_Place_Like_Home.pdf

Brunton-Smith, I., Hopkins, K., & Ministry of Justice UK., (2013, November). *The factors associated with proven re-offending following release from prison: findings from Waves 1 to 3 of SPCR*. https://www.gov.uk/government/publications/results-from-the-surveying-prisoner-crime-reduction-survey

Burnham, K. P., & Anderson, D. R. (2002). *Model Selection and Multimodel Inference: A Practical Information-Theoretic Approach*. Springer.

Carlisle, J. (1996). *The Housing Needs of Ex-Prisoners*. York, University of York Centre for Housing Policy.

Consumer NZ. (2014). *Insurance and convictions news*. Retrieved 5 January 2022, from https://www.consumer.org.nz/articles/insurance-and-convictions-news

Corden, J., Kuipers, J., & Wilson, K. (1979). Accommodation and Homelessness on Release from Prison. *The British Journal of Social Work*, *9*(1), 75–86. https://doi.org/10.1093/oxfordjournals.bjsw.a057060

Corrections. (n.d.). *Types of sentences*. Retrieved 5 January 2022, from https://www.corrections.govt.nz/working_with_offenders/courts_and_pre-sentencing/types_of_sentences

Corrections. (2010, June). *Prison facts and statistics - June 2010*. https://www.corrections.govt.nz/resources/statistics/quarterly_prison_statistics/previous_years_prison_statistics/march_2011

Corrections. (2019). *Annual Report*. https://www.corrections.govt.nz/__data/assets/pdf_file/0008/38852/Annual_Report_2018_2019_Web_Version_Final.pdf

Corrections. (2020a). *Annual Report*. https://www.corrections.govt.nz/__data/assets/pdf_file/0018/42273/Annual_Report_2019_2020.pdf

Corrections. (2020b). *Briefing to the Incoming Minister 2020*. https://www.corrections.govt.nz/__data/assets/pdf_file/0020/42815/Briefing_for_the_Incoming_Minister_2020.PDF

Corrections. (2021). *Annual Report*. https://www.corrections.govt.nz/__data/assets/pdf_file/0010/44398/Annual_Report_2020_2021_Final_Web.pdf

Craney, T. A., & Surles, J. G. (2002). Model-Dependent Variance Inflation Factor Cutoff Values. *Quality Engineering*, *14*(3), 391–403. https://doi.org/10.1081/qen-120001878

Dorne, S., Ministry of Justice, Corrections, & New Zealand Police. (2016, April). *Reintegration Services*. https://www.justice.govt.nz/assets/Documents/Publications/Reintegration-services.pdf

Ellison, M., Fox, C., Gains, A., & Pollock, G. (2013). An evaluation of the effect of housing provision on re-offending. *Safer Communities*, *12*(1), 27–37. https://doi.org/10.1108/17578041311293125

Fox, J., Jr, & Weisberg, S. (2018). *An R Companion to Applied Regression* (3rd ed.). SAGE Publications, Inc.

Groot, S., & Mace, J. (2016). Problem definition: Maori Homelessness in New Zealand. *Parity*, *29*(8), 5–6. https://search-informit-org.ezproxy.auckland.ac.nz/doi/abs/10.3316/informit.451792098323172

Grueber, C. E., Nakagawa, S., Laws, R. J., & Jamieson, I. G. (2011). Multimodel inference in ecology and evolution: challenges and solutions. *Journal of Evolutionary Biology*, *24*(4), 699–711. https://doi.org/10.1111/j.1420-9101.2010.02210.x

Harrell, F. E. (2010). *Regression Modeling Strategies: With Applications to Linear Models, Logistic Regression, and Survival Analysis (Springer Series in Statistics)*. Springer.

Health Navigator NZ. (2021, July). *Patient health questionnaire 9 (PHQ-9)*. Health Navigator New Zealand. Retrieved 5 January 2022, from https://www.healthnavigator.org.nz/tools/p/patient-health-questionnaire-9-phq-9/

Hernán, M. A., & Robins, J. M. (2020). *Causal Inference: What If* (1st ed.). Boca Raton: Chapman & Hall/CRC.

Hess, A. S., & Hess, J. R. (2017). Understanding tests of the association of categorical variables: the Pearson chi-square test and Fisher's exact test. *Transfusion*, *57*(4), 877–879. https://doi.org/10.1111/trf.14057

Hill, A. B. (1965). The Environment and Disease: Association or Causation? *Proceedings of the Royal Society of Medicine*, *58*(5), 295–300. https://doi.org/10.1177/003591576505800503

Homeless Link. (2011). *Better Together, Preventing Reoffending and Homelessness*. Homeless Link, London. https://www.homeless.org.uk/sites/default/files/site-attachments/1.Better%20Together%20-%20summary%20of%20findings.pdf

Hope, A. C. A. (1968). A Simplified Monte Carlo Significance Test Procedure. *Journal of the Royal Statistical Society: Series B (Methodological)*, *30*(3), 582–598. https://doi.org/10.1111/j.2517-6161.1968.tb00759.x

Humeniuk, R. & World Health Organization. (2010). *The Alcohol, Smoking and Substance Involvement Screening Test (ASSIST)*. World Health Organization.

Ikram, M. A. (2019). The disjunctive cause criterion by VanderWeele: An easy solution to a complex problem? *European Journal of Epidemiology*, *34*(3), 223–224. https://doi.org/10.1007/s10654-019-00501-w

King, J., McArthur, J., Paul, J., Harris, M., & Figenshow, S. (2020). *Transformative Housing Policy for Aotearoa New Zealand.* The University of Auckland. https://doi.org/10.17608/k6.auckland.13055441.v1

Kristman, V., Manno, M., & Côté, P. (2003). Loss to Follow-Up in Cohort Studies: How Much is Too Much? *European Journal of Epidemiology*, *19*(8), 751–760. https://doi.org/10.1023/b:ejep.0000036568.02655.f8

Lacey, R. J., Jordan, K. P., & Croft, P. R. (2013). Does Attrition during Follow-Up of a Population Cohort Study Inevitably Lead to Biased Estimates of Health Status? *PLoS ONE*, *8*(12), e83948. https://doi.org/10.1371/journal.pone.0083948

Lavrakas, P. J. (2008). Response Bias. In *Encyclopedia of Survey Research Methods*. Thousand Oaks: SAGE Publications. https://dx.doi.org/10.4135/9781412963947.n486

Little, R. J. A. (1986). Survey Nonresponse Adjustments for Estimates of Means. *International Statistical Review / Revue Internationale de Statistique*, *54*(2), 139–157. https://doi.org/10.2307/1403140

Littman, A. J., Boyko, E. J., Jacobson, I. G., Horton, J., Gackstetter, G. D., Smith, B., Hooper, T., Wells, T. S., Amoroso, P. J., & Smith, T. C. (2010). Assessing nonresponse bias at follow-up in a large prospective cohort of relatively young and mobile military service members. *BMC Medical Research Methodology, 10*(1). https://doi.org/10.1186/1471-2288-10-99

Lumley, T. (2010). *Complex Surveys: A Guide to Analysis Using R* (1st ed.). Wiley.

Lumley, T. (2020). *Survey: analysis of complex survey samples.* (4.0) [Software]. R.

Lumley, T., & Scott, A. (2017). Fitting Regression Models to Survey Data. *Statistical Science*, *32*(2), 265–278. https://doi.org/10.1214/16-sts605

Lutze, F. E., Rosky, J. W., & Hamilton, Z. K. (2014). Homelessness and Reentry. *Criminal Justice and Behavior*, *41*(4), 471–491. https://doi.org/10.1177/0093854813510164

Mack, C., Su, Z., & Westreich, D. (2018, February). *Managing Missing Data in Patient Registries: Addendum to Registries for Evaluating Patient Outcomes: A User's Guide*. National Center for Biotechnology Information. Retrieved 5 January 2022, from https://www.ncbi.nlm.nih.gov/books/NBK493614/

Malatest International. (2020). *Creating positive pathways: Process evaluation 2020*.

Mansournia, M. A., & Altman, D. G. (2016). Inverse probability weighting. *BMJ*, i189. https://doi.org/10.1136/bmj.i189

Mazza, G. L., Enders, C. K., & Ruehlman, L. S. (2015). Addressing Item-Level Missing Data: A Comparison of Proration and Full Information Maximum Likelihood Estimation. *Multivariate Behavioral Research*, *50*(5), 504–519. https://doi.org/10.1080/00273171.2015.1068157

McNeely, J., Strauss, S. M., Wright, S., Rotrosen, J., Khan, R., Lee, J. D., & Gourevitch, M. N. (2014). Test–retest reliability of a self-administered Alcohol, Smoking and Substance Involvement Screening Test (ASSIST) in primary care patients. *Journal of Substance Abuse Treatment*, *47*(1), 93–101. https://doi.org/10.1016/j.jsat.2014.01.007

Metraux, S., & Culhane, D. P. (2004). Homeless Shelter Use and Reincarceration Following Prison Release. *Criminology & Public Policy*, *3*(2), 139–160. https://doi.org/10.1111/j.1745-9133.2004.tb00031.x

Mills, A., Gojkovic, D., Meek, R., & Mullins, D. (2013). Housing ex-prisoners: the role of the third sector. *Safer Communities*, *12*(1), 38–49. https://doi.org/10.1108/17578041311293134

Mills, A., Latimer, C. L., Gordon, G., Groot, S., & Milne, B. (2021). More than a roof?: A critical review of post-prison housing provision in Aotearoa/New Zealand. *New Zealand Sociology*, *36*(1), 1–24. https://search-informit-org.ezproxy.auckland.ac.nz/doi/abs/10.3316/informit.850185230729940

Mills, A., Terry, A., Latimer, C. L., Milne, B., & Groot, S. (2022). *Going Straight Home? The role of stable housing in reducing reoffending by people who have left prison*.

Ministry of Housing and Urban Development. (2019). *Cabinet Paper Preventing and Reducing Homelessness in New Zealand*. https://www.hud.govt.nz/assets/News-and-Resources/Proactive-Releases/f41acf93b7/Cabinet-Paper-Preventing-and-Reducing-Homelessness-in-New-Zealand.pdf

Ministry of Justice. (2020). *Justice Sector Prison Population Projections 2019 - 2029*. https://www.justice.govt.nz/assets/Documents/Publications/3.-JS-Prison-Projection-Report-v2.1.pdf

Ministry of Justice. (2021). *Te Tāhū o te Ture Annual Report*. https://www.justice.govt.nz/assets/Documents/Publications/Ministry-of-Justice-Annual-Report-2020-21.pdf

Ministry of Social Development. (n.d.). *Work out how much it could cost to live - StudyLink*. Retrieved 5 January 2022, from https://www.studylink.govt.nz/starting-study/thinking-about-study/cost-of-living.html

Moik, F., Riedl, J. M., Winder, T., Terbuch, A., Rossmann, C. H., Szkandera, J., Bauernhofer, T., Kasparek, A. K., Schaberl-Moser, R., Reicher, A., Prinz, F., Pichler, M., Stöger, H., Stotz, M., Gerger, A., & Posch, F. (2019). Benefit of second-line systemic chemotherapy for advanced biliary tract cancer: A propensity score analysis. *Scientific Reports*, *9*(1). https://doi.org/10.1038/s41598-019-42069-1

Morral, A. (2000). Hardcore drug users claim to be occasional users: drug use frequency underreporting. *Drug and Alcohol Dependence*, *57*(3), 193–202. https://doi.org/10.1016/s0376-8716(99)00048-4

Morrison, B., & Bowman, J. (2017). What happens beyond the gate? Findings from the post-release employment study. *Practice: The New Zealand Corrections Journal*, *5*(1). https://www.corrections.govt.nz/resources/research/journal/volume_5_issue_1_july_2017

Nadesu, A. & Corrections. (2009, March). *Reconviction patterns of released prisoners: A 60-months follow-up analysis*. https://www.corrections.govt.nz/__data/assets/pdf_file/0015/10716/Complete-Recidivism-Report-2009-DOC.pdf

Olmos, A., & Govindasamy, P. (2015). A Practical Guide for Using Propensity Score Weighting in R. *Practical Assessment, Research & Evaluation*, *20*(13). http://www.math.umd.edu/~slud/s818M-MissingData/PropensityScoreWeightingR.pdf

Pleggenkuhle, B., Huebner, B. M., & Kras, K. R. (2015). Solid Start: supportive housing, social support, and reentry transitions. *Journal of Crime and Justice*, *39*(3), 380–397. https://doi.org/10.1080/0735648x.2015.1047465

Pourhoseingholi, M. A., Baghestani, A. R., & Vahedi, M. (2012). How to control confounding effects by statistical analysis. *Gastroenterology and Hepatology from Bed to Bench*, *5*(2), 79–83. https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4017459/

Raad, H., Cornelius, V., Chan, S., Williamson, E., & Cro, S. (2020). An evaluation of inverse probability weighting using the propensity score for baseline covariate adjustment in smaller population randomised controlled trials with a continuous outcome. *BMC Medical Research Methodology*, *20*(1). https://doi.org/10.1186/s12874-020-00947-7

Rubin, D. B. (2006). The design versus the analysis of observational studies for causal effects: parallels with the design of randomized trials. *Statistics in Medicine*, *26*(1), 20–36. https://doi.org/10.1002/sim.2739

Seaman, S. R., & White, I. R. (2011). Review of inverse probability weighting for dealing with missing data. *Statistical Methods in Medical Research*, *22*(3), 278–295. https://doi.org/10.1177/0962280210395740

Sedgwick, P. (2014). Bias in observational study designs: prospective cohort studies. *British Medical Journal*, *349*(dec19 2), g7731. https://doi.org/10.1136/bmj.g7731

Skelly, A., Dettori, J., & Brodt, E. (2012). Assessing bias: the importance of considering confounding. *Evidence-Based Spine-Care Journal*, *3*(01), 9–12. https://doi.org/10.1055/s-0031-1298595

Stats NZ. (2015). *New Zealand definition of homelessness: 2015 update*. https://www.stats.govt.nz/methods/new-zealand-definition-of-homelessness-2015-update

Stats NZ. (2020a). *Housing in Aotearoa: 2020*. https://www.stats.govt.nz/reports/housing-in-aotearoa-2020

Stats NZ. (2020b). *The state of housing in Aotearoa New Zealand*. Retrieved 5 January 2022, from https://www.stats.govt.nz/infographics/the-state-of-housing-in-aotearoa-new-zealand

Stewart, M. (2018, February 21). Prisons under ‘immense pressure’ with only enough space for 300 more inmates. *Stuff*. https://www.stuff.co.nz/national/crime/101634611/prisons-under-immense-pressure-with-only-enough-space-for-300-more-inmates

Stürmer, T., Wyss, R., Glynn, R. J., & Brookhart, M. A. (2014). Propensity scores for confounder adjustment when assessing the effects of medical interventions using nonexperimental study designs. *Journal of Internal Medicine*, *275*(6), 570–580. https://doi.org/10.1111/joim.12197

Textor, J., van der Zander, B., Gilthorpe, M. S., Liśkiewicz, M., Ellison, G. T. H., & International Journal of Epidemiology. (2017). *Robust causal inference using directed acyclic graphs: the R package 'dagitty.'* [Software]. R.

Therneau, T. M. (2022). A Package for Survival Analysis in R (R package version 3.3-1) [Software]. https://CRAN.R-project.org/package=survival

Thomas, L., Li, F., & Pencina, M. (2020). Using Propensity Score Methods to Create Target Populations in Observational Clinical Research. *JAMA*, *323*(5). https://doi.org/10.1001/jama.2019.21558

Tissera, N. (2019). New Zealand's Six Pillar Model of reintegration and international reintegrative models: A review of the literature. *Practice: The New Zealand Corrections Journal*, *Volume 7*(Issue 1). https://www.corrections.govt.nz/resources/research/journal/volume_7_issue_1_july_2019/new_zealands_six_pillar_model_of_reintegration_and_international_reintegrative_models_a_review_of_the_literature

Venables, W. N., & Ripley, B. D. (2002). *Modern Applied Statistics with S (Statistics and Computing)* (4th ed.). Springer.

Vu, D., Muttaqi, K., & Agalgaonkar, A. (2015). A variance inflation factor and backward elimination based robust regression model for forecasting monthly electricity demand using climatic variables. *Applied Energy*, *140*, 385–394. https://doi.org/10.1016/j.apenergy.2014.12.011

Wang, J. J. J., Bartlett, M., & Ryan, L. (2017). On the impact of nonresponse in logistic regression: application to the 45 and Up study. *BMC Medical Research Methodology*, *17*(1). https://doi.org/10.1186/s12874-017-0355-z

Workman, K. (2014). The social integration of Maori prisoners. *Aotearoa New Zealand Social Work, 26*(1), 39–46.

Workman, K., & McIntosh, T. (2013). Crime, Imprisonment and Poverty. In M. Rashbrooke (Ed.), *Inequality: A New Zealand Crisis* (1st ed., pp. 120–133). Bridget Williams Books.

Yang, D., & Dalton, J. E. (2012). A unified approach to measuring the effect size between two groups using SAS. Paper 335–2012.

Zhang, Z., Kim, H. J., Lonjon, G., & Zhu, Y. (2019). Balance diagnostics after propensity score matching. *Annals of Translational Medicine*, *7*(1), 16. https://doi.org/10.21037/atm.2018.12.10

# Appendix A – Tables

*Table 80: The type of housing prior to prison compared to the number of moves in the 6 months prior to prison*

| | Number of moves before prison | |
|---|---|---|
| | 0-1 | 2+ |
| Owned | 12 | 3 |
| Rented | 37 | 12 |
| State housing | 12 | 3 |
| Housing First/NGO Provider | 7 | 2 |
| With friends/family | 40 | 46 |
| Hotel | 0 | 5 |
| In a hostel/boarding house/backpackers/campground/holiday park | 2 | 2 |
| On the street | 0 | 18 |

*Table 81: Housing support offered by prison.*

| | | Auckland Women's | Christchurch Wens | Hawkes Bay | Northland | Spring Hill | Waikeria |
|---|---|---|---|---|---|---|---|
| Had housing support offered while in prison | Have not needed | 3 | 3 | 6 | 6 | 0 | 0 |
| | No | 22 | 12 | 33 | 19 | 10 | 22 |
| | Yes | 9 | 14 | 16 | 12 | 0 | 11 |

*Table 82: Housing support received after release from prison by gender.*

| | | Female | Male | Sum |
|---|---|---|---|---|
| Received support after prison | Have not needed | 3 | 11 | 14 |
| | No | 9 | 33 | 42 |
| | Yes | 0 | 22 | 22 |
| | Sum | 12 | 66 | 78 |

*Table 83: Re-imprisonment within a year and housing support received after release from prison.*

| | | Re-imprisoned | | |
|---|---|---|---|---|
| | | No | Yes | Sum |
| Received support after prison | Have not needed | 14 | 0 | 14 |
| | No | 28 | 14 | 42 |
| | Yes | 17 | 6 | 23 |
| | Sum | 59 | 20 | 79 |

*Table 84: Re-imprisonment and all housing types.*

| Re-imprisoned | No | Yes |
|---|---|---|
| Owned | 3 | 1 |
| Rented | 16 | 0 |
| State housing | 1 | 1 |
| Housing First/NGO Provider | 5 | 0 |
| With friends/family | 27 | 11 |
| Hotel | 2 | 2 |
| In a hostel/boarding house/backpackers/campground/holiday park | 3 | 0 |
| Homeless | 3 | 5 |
| Elsewhere | 0 | 0 |

*Table 85: Re-sentencing and all housing types.*

| Re-sentenced | No | Yes |
|---|---|---|
| Owned | 3 | 1 |
| Rented | 13 | 3 |
| State housing | 1 | 1 |
| Housing First/NGO Provider | 2 | 3 |
| With friends/family | 22 | 16 |
| Hotel | 2 | 2 |
| In a hostel/boarding house/backpackers/campground/holiday park | 3 | 0 |
| Homeless | 2 | 6 |
| Elsewhere | 0 | 0 |