Longitudinal Evidence on the Impact of Incarceration on Labour Market Outcomes and General Well-Being*

Malathi Velamuri
Victoria University of Wellington
malathi.velamuri@vuw.ac.nz

Steven Stillman
Motu Economic and Public Policy Research
stillman@motu.org.nz

March 2007

* The authors thanks the New Zealand Ministry of Justice for providing funding for this project and XXX for providing valuable comments. Any views expressed are the sole responsibility of the authors and do not purport to represent those of the Ministry of Justice or Motu Economic and Public Policy Research.

Abstract - Longitudinal Evidence on the Impact of Incarceration on Labour Market Outcomes and General Well-Being

This paper examines the impact of being incarcerated on labour market outcomes and general well-being using longitudinal data from the nationally representative Household Income and Labour Dynamics of Australia survey. We estimate OLS regressions on the entire population, propensity score matching models that examine outcomes for individuals that have been incarcerated compared to outcomes for individuals with similar observable characteristics over the same time period, and fixed effects regression models that examine changes in outcomes for individuals before/after incarceration relative to changes in outcomes over time for non-incarcerated individuals. The second and third types of models allow us to control for characteristics that may simultaneously cause certain individuals to commit crimes and put them at higher risk of poor outcomes. Our results indicate that incarceration has a large negative short-run impact on the likelihood that individuals are employed, but has a positive impact on total income. We also find that being incarcerated has a significant negative impact on mental health and life satisfaction, but leads to an increased satisfaction with family relationships.

Keywords: crime, incarceration, labour market outcomes, well-being, Australia

JEL-Code: K42, J24, I10

I) Introduction

Criminal behaviour can be a serious financial and social burden on a society. Incarceration itself may have long-term effects on labour market outcomes and the general well-being of individuals that have spent time in jail. Previous studies have identified three mechanisms through which incarceration might affect individual employment prospects and earnings (see Western et al. 2001; Waldfogel 1994). First, there may be a stigma associated with being previously jailed that signals a prospective employee's lack of trustworthiness and thus discourages potential employers from hiring them.[1] Second, being incarcerated may erode an individuals' job skills and render them less productive relative to others who have remained in continuous employment. Third, incarceration may erode an individual's social capital, thus weakening the networks that lead to good job opportunities.

The evidence on the effect of incarceration on subsequent labour market outcomes is fairly limited. Western et al. (2001) review this literature. A number of studies examine cross-sectional data (typically, administrative data on criminal offenders or employment data collected by probation officers of federal offenders) and find that being incarcerated or convicted of a crime has a large negative impact on employment and earnings (Witte and Reid 1980; Lott 1990; Western and Beckett 1999; Freeman 1992). However, as discussed in Grogger (1995), it is typically difficult to identify the causal impact of incarceration on outcomes because the events are endogenously determined with the outcomes.[2] For example, poor labour market outcomes for ex-offenders may be a consequence of incarceration or might simply reflect that individuals who are incarcerated differ from other individuals in unobserved aspects that are correlated with poor outcomes.

[1] Evidence from survey data and experiments provides support for there being a stigma associated with being previously jailed. The fact that ex-convicts are not eligible for employment in certain occupations and sectors, at least for a short period of time following their release from prison, is also supportive of the idea that being jailed should be considered a negative signal about an individual's trustworthiness.

[2] Another important issue is that criminal activity is particularly difficult to measure and thus specialised surveys or administrative data are typically used to measure criminal activity. These data rarely also collect

This is an important issue because if the labour market consequences are as big as past cross-sectional estimates suggest, it is difficult to understand the widespread pervasiveness of crime. On the other hand, if a sizeable proportion of the observed cross-sectional effects are due to individuals heterogeneity, it is possible to rationalise the extensive incidence of crime based on a model of optimising behaviour (Grogger 1995). A small number of studies examine longitudinal data with the goal of isolating the causal impact of incarceration by controlling for observable and unobservable characteristics that might be related to the likelihood of an individual being incarcerated.

For example, Grogger (1995) links longitudinal arrest and conviction data for young men in California to official records of unemployment insurance agencies and examines the impact of being arrested on employment and earnings. He finds that being arrested causes a modest decline in post-arrest earnings, but that this effect declines over time and wears off by the 6th quarter following the arrest. Waldfogel (1994) uses longitudinal data on federal offenders in the US to examine the impact of conviction on employment and income for a range of crimes. He finds that being convicted has a negative impact on both employment and earnings.[3] In two separate papers, one using longitudinal data from the US and another from the UK, Nagin and Waldfogel (1993, 1995) find a positive effect of being convicted on income for offenders under the age of 25 and a negative effect for older offenders.[4]

One potential issue with each of these papers besides Nagin and Waldfogel (1995), is that all individuals in each analysis have been arrested at least once during a certain time period and thus either variation arising from the timing of arrests or a comparison of individuals who committed more serious offenses relative to other offenders is used to

---

information on the type of outcomes that social scientists would like to examine.

[3] This paper also finds that impact of being convicted is larger for educated individuals who held positions of trust in their pre-conviction jobs, which the author argues provides support for the stigma of conviction being the main channel through which convictions impact earnings.

[4] The authors argue that this might occur because following an arrest, individuals are less likely to find stable, career-oriented jobs that are characterised by low initial earnings but rising wage profiles over time. Instead,

identify causal impacts. The inability to compare arrested or convicted individuals to non-criminals will lead to biased results if outcomes for individuals that commit minor crimes and/or get arrested in the future are not indicative of what the outcomes for individuals that have already been arrested or convicted would have been if they had not been arrested or convicted (eg if these individuals are not a good counterfactual). For example, it seems likely that individuals from these ‘control’ groups may be engaging in criminal activities for which they have not yet been arrested and thus might be less likely to be formally employed and may have lower reported earnings.

In this paper, we estimate the causal effect of incarceration on labour market outcomes and general well-being using longitudinal data from the nationally representative Household Income and Labour Dynamics of Australia (HILDA) survey. Crucially for our analysis, individuals in HILDA are asked whether they experienced a number of major life events in the previous year including being detained in a jail or a correctional facility. While only a small number of individuals (less than 100) are incarcerated in the three waves of HILDA data used in this paper, the richness of the data allows us to extend the literature in a number of ways.

First, we estimate OLS regression models on the entire population, propensity score matching models that examine outcomes for individuals that have been incarcerated compared to outcomes for non-incarcerated individuals with similar observable characteristics over the same time period, and fixed effects regression models that examine changes in outcomes for individuals before/after incarceration. We can estimate both matching models and fixed effects regressions because HILDA collects longitudinal data on a representative sample of both offenders and non-offenders, unlike the administrative data typically used in this literature. Both the second and third type of models allow us to control

these individuals are more likely to participate in the spot market for labour, which allocates individuals to short-term but relatively high-paying jobs.

for characteristics that may simultaneously cause certain individuals to commit crimes and put them at higher risk of poor outcomes. Second, we estimate the impact on both labour market outcomes and general well-being. There are many reasons why incarceration might have impacts on individuals beyond the labour market. We believe that this is the first quantitative analysis to examine the impact of incarceration on more general measures of human capital and personal well-being.

Our results indicate that incarceration has a large negative short-run impact on the likelihood of employment, a limited impact on hourly wages and a positive impact on the likelihood that individuals are receiving social benefits and on total individual and household income. Perhaps surprisingly, after controlling for observable characteristics, we find little evidence that individuals that are incarcerated in Australia are negatively selected. Turning to the impact on general well-being, we find that being incarcerated has a significant negative impact on mental health, but leads to an increased satisfaction with family relationships.

II) Data

We examine the impact of incarceration using longitudinal data from the nationally representative HILDA survey for the years 2002-2004. This survey began in 2001 and has since been administered annually. HILDA interviews all adult members (aged 15 and over) in over 7,500 sample households and collects information about economic and subjective well-being, labour market dynamics and family dynamics.[5] Individuals in sample households are followed over time regardless to whether they remain in the original households. Four survey instruments are included in HILDA: a Household Form and a Household Questionnaire are completed during a personal interview with one adult member of each household; a Person Questionnaire is administered to all adult household members; and a Self-Completion

[5] The survey utilises a multi-stage sampling approach (sampling households within Census Collection Districts) and is stratified by State and part-of-State.

Questionnaire (SCQ) is provided to all respondents to the Person Questionnaire and is collected at a later date or returned by post.

The SCQ elicits subjective responses to an array of sensitive questions, such as alcohol use and life satisfaction. Starting in the second wave (2002), the following question was added to the SCQ, "We now would like you to think about major events that have happened in your life over the past 12 months. For each statement cross either the NO box or the YES box to indicate whether each event happened during the past 12 months. If you answer "YES", then also cross one box to indicate how long ago the event happened or started." Twenty-one major events are then listed below the question. One of these states, "Detained in a jail or correctional facility", which is the event that we focus on in this paper.[6] Unfortunately, we are provided with no further details about the crime committed, the type of institution in which the individual was detained, whether they undertook job training or apprenticeship programmes while imprisoned, the length of their sentence or the time actually served. This lack of contextual detail is an obvious weakness of using HILDA to examine the impact of incarcerations.

Pooling the three years of survey data provides a sample of 15,088 individuals and 38,177 observations. For obvious reasons, we need to drop all observations in which an individual fails to complete a SCQ (3,396 observations) or fails to answer the question on whether or not they have been incarcerated in the previous year (560 observations). We also drop a small number of observations where individuals fail to report their aboriginal status or education in a particular year (14 observations). This leaves us with a main analysis sample of 14,216 individuals and 34,207 observations. A further 2,742 individuals are in our analysis sample for only one year and thus are dropped when estimating fixed effects regression models.

[6] Other examples include "Pregnancy / pregnancy of partner", "Death of a close friend", and "Promoted at work".

Out of the main analysis sample, 68 individuals (0.48%) report having ever been incarcerated during the three years of the sample, with 64 of these individuals having been incarcerated once and 4 incarcerated twice for a total of 72 events. The probability of being incarcerated declines over the three year period, with 0.23% of individuals (26 people) in 2002 reporting being incarcerated in the previous year, 0.22% (26 people) in 2003, and 0.18% (20 people) in 2004. Table 1 presents summary statistics for individuals, stratified by whether they have ever been incarcerated. The first set of variables comprises the outcomes for which we examine the impact of incarceration. Individuals that have been incarcerated have lower employment rates, hourly wages, individual and household incomes, general and mental health, life satisfaction and satisfaction with family relationships and are more likely to receive social benefits.[7] As discussed above, these differences may merely reflect the fact that individuals that commit crimes and get caught also have observed and unobserved characteristics that are associated with having poor labour market outcomes.

Next, we examine differences in individual and household demographics and local neighbourhood characteristics. Individuals that have been incarcerated are younger, predominantly male, less educated, more likely to be Aboriginal or a Torres Strait Islander, are less likely to be immigrants or to be married, and are more likely to live in lower quality

---

[7] Employment status measures whether the individual is currently employed, (conditional) hours worked measures hours worked in the last week (conditional on being employed), hourly wages are earnings in the last week divided by hours worked in the last week for all wage/salary workers, benefit receipt measures whether an individual receives income from any government benefit, total annual income is the summation of all income sources for the individual in the last year, and total annual household income is the summation of all income sources in the last year for all household members. All wage and income measures are in 2001 Australian dollars. Hours worked are winsorised at the 99th percentile and hourly wages at the 1st and 99th percentile to reduce the impact of obvious measurement error. The SF-36 questionnaire collects data on eight health domains: physical functioning, role-physical, bodily pain, general health, vitality, social functioning, role-emotional, and mental health. Index scores are created for each domain by transforming the appropriate questions from among the thirty-six. These indexes are scored 0-100, with 100 representing perfect health on each index. HILDA asks a general question on each individual's overall life satisfaction. Another question asks how satisfied or dissatisfied each individual is with a number of personal relationships. We calculate the mean response across all of the following relationships which are applicable to the individual: i) their relationship with their partner, ii) their relationship with their children, iii) their partner's relationship with their children, iv) how well the children in the household get along with each other, v) their relationship with their parents, and vi) their relationship with their (most recent) former spouse or partner. Each of these measures is asked on a 0-10 scale with a 0 indicating an individual is totally dissatisfied and a 10 indicating they are totally satisfied with their life

neighbourhoods as measured along a whole host of dimensions.[8] The fact that individuals who have been convicted differ on numerous observable dimensions from those that have never been convicted and that a number of these dimension (for example, being young, Aboriginal or a Torres Strait Islander, being less educated) are associated with worse labour market outcomes suggests that negative selection may explain some/all of the differences in outcomes noted above.[9]

III) Econometric Model

We now turn to estimating an econometric model of the impact of being incarcerated on labour market outcomes and general well-being.[10] Following Grogger (1995), we estimate a distributed lag model, thus allowing incarcerations to impact current and future labour market outcomes and general well-being. The model is specified as follows:

$$y_{it} = \sum_{j=0}^{2} X_{it-j}\beta_j + Z_{it}\delta + e_{it} \tag{1}$$

where y_{it} is one of ten outcome measures for individual i at time t, X_{it} is an indicator variable which equals 1 if individual i reports having been incarcerated in the previous 12 months when interviewed at time t and equals 0 otherwise, Z_{it} is a vector of control variables including age and age-squared, gender, whether the individual is Aboriginal or a Torres Strait Islander, educational status, country of birth, marital status, number of children aged 0-15 and

---

or relationships.

8 HILDA asks individuals a number of questions about their neighbourhood. We examine each individual's opinion on the frequency of the following events in their neighbourhood: traffic noise, noise from airplanes, trains or industry, homes and gardens in bad condition, rubbish and litter lying around, teenagers hanging around, people being hostile and aggressive, vandalism and damage to property, and burglary and theft. These questions are asked on a 1-5 scale with a 1 indicating that an event never happens in their neighbourhood and a 5 indicating that an event is very common.

9 In the next section, multivariate models of the likelihood of ever being incarcerated will be estimated and discussed.

10 Although we interpret the results in terms of the impact of having been incarcerated, it is worth emphasising that we are actually measuring the impact of *reporting* having been incarcerated. Given the nature of the event, it is reasonable to assume that incarcerations may be under-reported. However, our results will be unbiased as long as the likelihood of reporting being incarcerated conditional on having actually been incarcerated is not systematically related to the examined outcomes. It is difficult to know apriori if this is likely to be the case, but it is difficult to tell a compelling story on why the likelihood of reporting this event would be related to any particular outcome.

16-20 in the household, number of adults in the household, and indicator variables for the year, urbanicity, and geographical location of the household, and e_{it} is a random error term. Since only three waves of the data are available, we can only identify the short-term effects of incarceration. In particular, the coefficients β_0, β_1 and β_2 indicate the impact of being incarcerated in the previous year, the impact of having been incarcerated two years ago, and the impact of having been incarcerated three years ago, respectively, on a particular outcome.[11]

We consider three approaches for estimating this regression model. First, we estimate this model using OLS. If the error term, e_{it}, is uncorrelated with the vector of variables indicating whether an individual has been convicted in the current or previous years, X_{it}, then OLS will provide an unbiased estimate of the impact of being convicted on each outcome.[12] In other words, if after controlling linearly for the observable characteristics in Z_{it}, the likelihood of being convicted is uncorrelated with an individual's unobserved characteristics and with a particular outcome, then the estimated OLS impact will be unbiased. One shortcoming of estimating an OLS model is that we need to make strong parametric assumptions about the relationship between the observables and the likelihood of being convicted. A second shortcoming is that it is not transparent whether all individuals who are not convicted in a particular years are, in fact, a good counterfactual for the outcomes that convicted individuals would have had if they have not been convicted.

Next, we follow the program evaluation literature and use a matching methodology approach to construct "control" groups of individuals that have never been incarcerated but have similar characteristics as the individuals that have been incarcerated at some point in the

[11] It is worth noting that the impact of having been incarcerated two years ago is only identified from the outcomes for individuals incarcerated in the previous year in 2002 and 2003 and the impact of having been incarcerated three years ago is only identified from the outcomes for individuals incarcerated in the previous year in 2002. Thus, if the impact of being incarcerated changes over this time period, these results will be confounded. We do not suspect that this is likely to be an issue over such a short time period.

[12] When estimating OLS models and the matching models discussed below, we calculate Huber/White robust

sample (Rubin 1979; Lalonde 1986). Like OLS estimates, matching models also only control for differences in observable characteristics between individuals that have and have not been incarcerated, but, unlike OLS, they only require at most weak parametric assumptions about the relationship between the observables and the likelihood of being incarcerated. In particular, we estimate propensity score matching models that examine outcomes for individuals that have been incarcerated (the "treatment group") compared to outcomes for individuals with similar observable characteristics over the same time period that have never been incarcerated (the "control group"). Here, matching is done on a first-stage estimate of the likelihood of ever being incarcerated (eg. the propensity score). Two useful features of calculating a propensity score is that it maps out the relationship between observables and the likelihood of ever being incarcerated in one summary statistic and shows which individuals in the treatment group have close matches among the individuals in the control group.

We calculate two propensity scores for incarceration based on probit estimates of whether an individual ever reports being incarcerated during the sample period (eg. each individual only contributes one observation to this model). In the first model, we control for all of the demographic and geographic characteristics included in the Z_{it} vector above measured in the first year each individual is in HILDA. In the second model, we also include control variables for the neighbourhood characteristics summarised in Table 1, measured in the first year each individual is in HILDA. Including neighbourhood characteristics increases the explanatory power of the model, but these variables are potentially endogenous to the likelihood of being incarcerated.[13] For example, we should be concerned about including them if criminals cluster together in certain neighbourhoods. As noted in Table 1, no

---

standard errors that account for the fact that each individual is observed multiple times and their error terms are likely to be correlated over time. Thus, the OLS estimates are also efficient.

[13] We also include additional controls for whether individuals report each of the components of neighbourhood quality (eg. whether the variables are missing). A small number of individuals that do not answer the questions about the amount of either other noise or rubbish in their neighbourhood are dropped from the matching model because none of these individuals have ever been jailed (thus, these variables are perfect predictors of never

individuals with Bachelor degrees or higher report being incarcerated in HILDA, thus these individuals are automatically excluded from the control group and dropped from the propensity scores regressions.

The results from these models are presented in Appendix Table 1. Both models fit the data well, with pseudo R-Squares of 0.13 and 0.17, respectively, and pass balancing tests for almost all of the covariates, indicating that the model is well specified (see Dehjia and Wahba 1999).[14] Among the significant results: men, Aboriginals and Torres Strait Islanders, the less educated, and individuals that live in neighbourhoods where people display hostile and aggressive behaviour or where there is a high level of burglary and theft are more likely to ever be incarcerated. Among these, the strongest predictors are gender and ethnicity, with men 0.43-0.52% more likely than women and Aboriginals and Torres Strait Islanders 0.67-1.01% more likely than non-Aboriginals to be incarcerated relative to a mean incarceration rate in the sample of 0.60%.

Figure 1 presents kernel density graphs of the propensity scores for both the treatment and entire potential control group (eg. all individuals that have never been incarcerated and do not have a Bachelor degrees or higher). The top graph is based on the first model specification and the bottom graph on the second. The treatment and potential control group have common support in both models; all ever-jailed individuals are in the same propensity score range as the never-jailed. The noteworthy feature of these density plots is that even though incarceration is very rare for both groups, propensity scores are much higher for the ever-jailed group and the distribution for this group is much more uniform. For example, the mean propensity score for the never-jailed is 0.006 in both models, while the corresponding

being jailed).

[14] Balancing tests test whether covariate means are the same in the matched treatment and control groups when the sample is divided into a number of blocks which ensures that the mean propensity score is not different for the treated and controls in each block. These estimates are done using the user written pscore command in Stata 9. In the first model, two indicator variables for the year of observation and the geographical location fail the balancing test in one block each. In the second model, the indicator variable for being Aboriginal or a Torres

means for the ever-jailed are 0.022 in the first model and 0.035 in the second model (eg. the average individual in the ever-jailed group is estimated to be 4 to 6 times more likely to be ever-jailed than the average individual in the never-jailed group).

Since being incarcerated is a rare event and we have a large potential control group, we use nearest neighbour matching with replacement to match each individual in the ever-jailed group to the four individuals in the never-jailed group that have the most similar propensity score (Dehjia and Wahba 1999). Matching to four people is an arbitrary decision but, based on the sample size, seemed to be a good trade-off between efficiency and bias, both of which increase with the number of matches. We achieve a high quality match using both models, with the mean difference in propensity scores between the ever-jailed individuals and the matching never-jailed individuals less than 0.0005 in both cases. Using the first model, 238 individuals from the control group are matched once to an individual in the treatment group and 17 twice to the 68 ever-jailed individuals. The corresponding figures for the second model are 235 individuals matched once, 17 twice, and 1 thrice. The impact of being incarcerated on each outcome is then determined by estimating equation (1) using OLS, but assigning a weight of 4 to each individual in the treatment group, a weight of 1, 2, or 3 to each matched individual from the control group, and a weight of 0 to all non-matched members of the control group.

Finally, we re-estimate equation (1) including individual fixed effects. We now assume that the error term, e_{it}, can be decomposed into two components:

$$e_{it} = \alpha_i + u_{it}, \tag{2}$$

where α_i is an error-term specific to each individual and invariant over time and u_{it} is a standard white noise error-term. Fixed effects estimation will be unbiased even if α_i is correlated with the X_{it} vector, eg. if fixed unobserved characteristics of the individual are

---

Strait Islander fails the balancing test in one block.

correlated both with the likelihood of being incarcerated and with particular outcomes. The results from this model can be interpreted as measuring changes in outcomes for individuals before/after incarceration relative to changes in outcomes over time for non-incarcerated individuals. While fixed effects regression models are unbiased in the presence of observed and fixed unobserved individual heterogeneity, they are less efficient than OLS and matching models if fixed individual unobserved heterogeneity is not present, and exacerbate measurement error bias, making it increasingly likely that the results will be small in magnitude and statistically insignificant.

IV) Results

We begin by examining whether being incarcerated has an effect on four key labour market outcomes: employment, hours worked in the last week, benefit receipt, and (log) total real annual income in the last year.[15] Table 2 presents estimates of β_0, β_1 and β_2 using OLS, the two propensity score matching models, and fixed effects. First, we examine the OLS results. Controlling for covariates, being incarcerated significantly reduces the likelihood of being employed by 18% and average hours worked by 5 hours in the two subsequent years (relative to a sample average of 23 for the never-jailed); the likelihood of receiving benefits significantly increases by 22% in the first year following incarceration and by 15% in the second; and there is a marginally significant 18% increase in total annual income in the year after incarceration.

The two matching models give similar results; there is a significant decline in employment probabilities for two years following incarceration – between 14-16% in the first year and 17-18% in the following year. The impact on hours worked is of a similar negative magnitude as the OLS estimates in the two subsequent years, but is no longer significant. The

[15] OLS models are estimated for each outcome although employment and benefit receipt are discrete outcomes. We do this so we can estimate the same model for each outcome which is a fairly standard approach in the evaluation literature (Angrist 2001). We also estimated logit and conditional fixed effect logit models for each of these outcomes and we briefly discuss these results below.

probability of receiving benefits increases by a similar amount in the two years following incarceration – between 18-22% in the first year and 17-19% in the second year. The only qualitative change in results occurs for the impact on total income; the matching results indicate that total income increases significantly by 22-30% in the first year after incarceration and 28-35% in the second year.

Finally, we examine the fixed effects estimates. Even controlling for unobserved individual heterogeneity, we find that being incarcerated lowers an individual's likelihood of being employed by 13% in the year after being incarcerated and 20% in the following year and decreases average hours worked by 5-6 hours in these years. However, we no longer find strong evidence that being incarcerated increases the likelihood of receiving benefits, although a marginally significant 13% increase is found two years after incarceration. The impact on total income is of a similar magnitude to that found in the matching models (a 31% increase in the year after incarceration and a 26% increase in the subsequent year), however, only the finding for the year following incarceration is now significant.

Overall, we find strong evidence that incarceration has a large negative short-run impact on the likelihood of employment and a similar magnitude negative impact on average hours worked.[16] These results are similar whether the impact model is estimated using OLS, propensity score matching, or fixed effects, suggesting that Australians who are incarcerated are not negatively selected on either observable or unobservable characteristics which are correlated with the likelihood of being employed.[17] The impact on hours worked relative to the average hours worked of the never-jailed is of similar magnitude as the size of the impact

[16] The negative impact on employment may be even stronger than that reported here. When we re-estimate the impact using a logit model instead of OLS, we find that incarceration leads to a significant 23-24% decline in employment in the year after incarceration and 24-27% decline in the following year, with or without matching. However, when we estimate a fixed effect (conditional) logit model, we only find a significant negative impact two years after incarceration. We do not focus on these results, because it is not possible to calculate simple marginal effects for the conditional logit model, making it difficult to gauge the magnitude of the impacts estimated using this model.

[17] In fact, similar magnitude negative, although marginally significant, impacts on employment and hours worked are found when estimating the OLS model without any covariates besides the incarceration variables.

on the likelihood of employment, suggesting that being jailed mainly affects the extensive margin of labour supply (eg. has a limited impact on hours worked conditional on being employed). We also find evidence that being incarcerated increases the likelihood that individuals receive social benefits. However, there is some evidence that this may occur because individuals who are more likely to receiving benefits are also more likely to be incarcerated.

Interestingly, we find that being incarcerated leads to a large increase in individual income in the two years following incarceration. Here, our matching estimates are larger than the simple OLS estimates (both are positive) suggesting that individuals that have been incarcerated have observable characteristics that are, in general, associated with having lower incomes. Re-estimating the OLS model, excluding all covariates besides the incarceration variables, shows that this is clearly the case – we now find that individuals who are incarcerated have significantly <u>lower</u> incomes in the year after incarceration than the non-incarcerated. Given this apparent negative selection on observables, it is then perhaps surprising that the fixed effects estimates are similar to those from the matching models, indicating that selection on unobservables is unimportant. Since our measure of total income includes benefits, these estimates could simply be picking up the impact of increasing benefit receipt. Another possibility is that return-to-work programs have a positive impact on incomes. Or perhaps, as theorised by Nagin and Waldfogel's (1993, 1995), ex-offenders, following their release, are more likely to participate in the spot market for labour and earn relatively more on short-term jobs. If this is the case, then we should also see that incarceration has a positive effect on wages. Thus, we next estimate the impact of incarceration on wages.

Table 3 presents the results from estimating the impact model with (log) hourly wages for wage/salary employees as the outcome variables. Here, and in the remaining tables in this paper, we only present the results from the matching model including neighbourhood characteristics (for all remaining outcomes, the results are qualitatively similar for either matching model) and the fixed effects model. The first panel of this table presents estimates where individuals are only included in the outcome regression in years where they are working in wage/salary employment (eg. are not self-employed, unemployed, or out of the labour force). The fixed effects estimates here suggest that incarceration has a large negative impact on future wages. However, sample selection is a serious concern for interpreting these results. We have already shown that incarceration leads to a large decrease in the likelihood of being employed. Now, for example, if this reduction in employment mainly occurs among individuals that would have been high wage workers, if they had been working, then we will find that incarceration has a large negative impact on wages even if it does not actually have a causal effect.

We attempt to deal with this selection issue by imputing wages for the non-employed.[18] We take a very simple approach and make the assumption that, on average, individuals who are not employed would be earning "low wages" if they were employed. This seems like a reasonable assumption for individuals that have ever been incarcerated and for never-incarcerated individuals with similar characteristics (eg. the individuals on whom our models focus). We define "low wages" based on the distribution of wages of working individuals. We present the results from using three different definitions of "low wages", either the 5th, 10th or 25th percentile of the observed wage distribution.[19] These estimates suggest that incarceration may have negative impacts on wages on the order of 3-10% in the two years following incarceration. However, only one estimated effect is marginally significant. In all

[18] Another approach is to estimate a heckman selection model of wages conditional on employment. This model is only identified if variables exist that impact the likelihood of being employed and are unrelated to wages. It is not clear that any of these do exist. Furthermore, extending the heckman model to allow for fixed effects is non-trivial.

[19] The corresponding real hourly wages for each imputation are $7.18 for the 5th percentile, $9.34 for the 10th percentile and $13.02 for the 25th percentile.

cases, the point estimates are much lower than the estimated impacts when only examining wage/salary employees, suggesting that selection into employment is an important concern when examining the impact of incarceration on wages in our sample. There is no evidence that incarceration has positive impacts on wages, thus these results imply that the positive impact of incarceration on total income is mainly coming from non-wage sources.

Incarceration may have impacts on individuals beyond the labour market. HILDA also collects data on general and mental health and on how satisfied individuals are with different relationships. We next estimate the impact of incarceration on (log) real total household income, general health, mental health, life satisfaction and overall satisfaction with family relationships. These results are presented in Table 4. Controlling for unobserved heterogeneity, we find that being incarcerated increases household income by 25-28% in the two years following incarceration, reduces mental health by 6.2 points one year after incarceration and 9.0 points in the following year (relative to an average score of 74.2 on a 0-100 scale in the never-jailed sample) and increases average satisfaction with family relationships by 1.4 points three years after incarceration (relative to an average score of 7.9 on a 0-10 scale in the never-jailed sample). The results for household income are sensitive to whether we control for unobserved heterogeneity, with the matching results showing a negative impact of incarceration on household income. This seems plausible given the prevalence of individuals to live with similar people (eg. assortative mating).

Finding that incarceration leads to an increase in household income is consistent both with our finding that incarceration leads to an increase in individual income and with the idea that other household members are likely to increase their labour supply when a family member is incarcerated and then stay in the labour force after they are released (eg. an ‘added-worker’ effect). Our finding of a large impact of incarceration on mental health is particularly striking. This represents a significant *indirect* cost of crime. If individuals suffer a decline in mental health following a stint in prison, this might serve as the channel through which incarceration erodes productivity and skills. Clearly then, this is relevant to the human capital theory explanation for the linkage between crime and labour market outcomes. We are uncertain to what might be causing incarceration to lead to a lagged increase in satisfaction with family relationships; one possible explanation is that individuals that have been incarcerated return home with a new perspective on life and an appreciation for their family, particularly the people who were there waiting for them when they got out of jail. Alternatively, they may have formed new partnerships after getting out of jail, and the results may be reflecting their satisfaction with their new family.

V) Conclusions

This paper examines the causal effect of being incarcerated on labour market outcomes and general well-being using longitudinal data from the nationally representative Household Income and Labour Dynamics of Australia survey. Crucially for our analysis, individuals in HILDA are asked whether they experienced a number of major life events in the previous year including being detained in a jail or a correctional facility. While only a small number of individuals (less than 100) are incarcerated in the three waves of HILDA data used in this paper, the richness of the data allows us to extend the literature in a number of ways.

First, we estimate OLS regression models on the entire population, propensity score matching models that examine outcomes for individuals that have been incarcerated compared to outcomes for non-incarcerated individuals with similar observable characteristics over the same time period, and fixed effects regression models that examine changes in outcomes for individuals before/after incarceration. We can estimate both matching models and fixed effects regressions because HILDA collects longitudinal data on a representative sample of both offenders and non-offenders, unlike the administrative data typically used in this literature. Both the second and third types of models allow us to control for characteristics that may simultaneously cause certain individuals to commit crimes and put them at higher risk of poor outcomes. Second, we estimate the impact on both labour market outcomes and general well-being. There are many reasons why incarceration might have impacts on individuals beyond the labour market. We believe that this is the first quantitative analysis to examine the impact of incarceration on more general measures of human capital and personal well-being.

Our results indicate that incarceration has a large negative short-run impact on the likelihood of employment, a limited impact on hourly wages and a positive impact on the likelihood that individuals are receiving social benefits and on total individual and household income. Perhaps surprisingly, after controlling for observable characteristics, we find little evidence that individuals that are incarcerated in Australia are negatively selected. Turning to the impact on general well-being, we find that being incarcerated has a significant negative impact on mental health, but leads to an increased satisfaction with family relationships.

References

Angrist, Joshua D. (2001), "Estimation of Limited-Dependent Variable Models with Binary Endogenous Regressors: Simple Strategies for Empirical Practice," *Journal of Business and Economic Statistics*, Vol. 19, No. 1.

Australian Institute of Criminology (2006). Australian Crime: Facts and Figures 2005. Canberra, Australia.

Dehjia, R.H. and Wahba, S (1999), "Causal Effects in Nonexperimental Studies: Reevaluating the Evaluation of Training Programs", *Journal of the American Statistical Association*, Vol.94, pp.1053-1062.

Freeman, Richard (1992), "Crime and the Employment of Disadvantaged Youth", *Urban Labor Markets and Job Opportunity*, Washington DC, Urban Institute Press.

Grogger, J (1995), "The Effect of Arrests on the Employment and Earnings of Young Men", *Quarterly Journal of Economics,* Vol.110, No.1, pp.51-71.

Glaeser, Edward, Bruce Sacerdote and Jose Scheinkman (1996), "Crime and Social Interactions", *Quarterly Journal of Economics,* Vol.111, No.2, pp.507-548.

Kling (2006), "Incarceration Length, Employment, and Earnings", *American Economic Review (forthcoming)*

Lalonde R (1986). "Evaluating the Econometric Evaluations of Training Programs." *American Economic Review,* 76: 604–20.

Lott, J R (1990), "The effect of conviction on the legitimate income of criminals", *Economic Letters*, No.34, pp.381-385.

Nagin, Daniel and Joel Waldfogel (1993), "The Effect of Conviction on Income over the Life-Cycle", *NBER Working Paper 4551.*

Nagin, Daniel and Joel Waldfogel (1995), "The Effects of Criminality and Conviction on the Labor Market Status of Young British Offenders", *International Review of Law and Economics,* No.15, pp.109-126.

Pettit, Becky and Bruce Western (2000), "Incarceration and Racial Inequality in Men's Employment", *Industrial and Labor Relations Review*, Vol. 54, No.1.

Rubin D (1979). "Using Multivariate Matched Sampling and Regression Adjustments to Control Bias in Observational Studies." *Journal of the American Statistical Association*, 74: 318–29.

Waldfogel, Joel (1994), "The Effect of Criminal Conviction on Income and the Trust "Reposed in the Workmen"," *Journal of Human Resources*, Vol.29, No.1, pp.62-81.

Western, Bruce, Jeffrey Kling and David Weiman (2001), "The Labor Market Consequences of Incarceration," *Crime and Delinquency* 47:3 (July), 410-427.

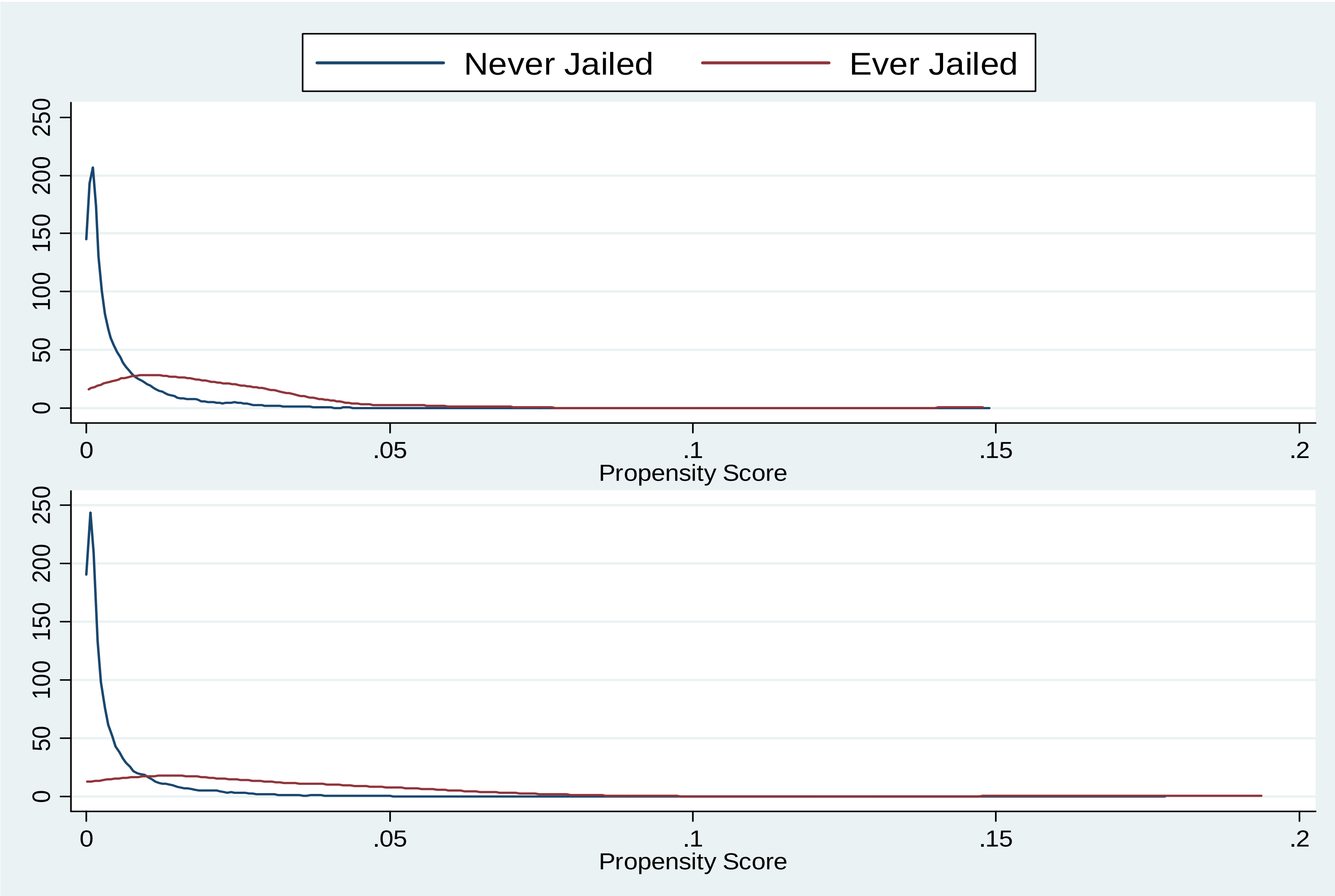
Witte, Ann and Pamela Reid (1980), "An Exploration of the Determinants of Labor Market Performance for Prison Releases", *Journal of Urban Economics,* No.8, Issue 3, pp.313-329.

Table 1: Summary Statistics by Event Status

	No Event	Ever Jailed
Jailed in a Particular Year		49.7%
Is Employed	63%	53%
	(0.00)	(0.04)
Hours Worked in Last Week	22.7	19.9
	(0.12)	(1.77)
Hours Worked if Employed	35.9	37.5
	(0.11)	(1.58)
Real Hourly Wage for Wage/Salary Workers	19.39	14.88
	(0.08)	(0.64)
Receives Social Benefits	33%	51%
	(0.00)	(0.04)
Real Total Annual Income in Last Year	31,727	18,127
	(221)	(1289)
Real Total Annual Household Income in Last Year	69,624	48,698
	(362)	(3412)
SF-36 General Health	69.2	65.5
	(0.12)	(1.89)
SF-36 Mental Health	74.2	63.9
	(0.09)	(1.71)
Life Satisfaction	7.96	7.42
	(0.01)	(0.16)
Satisfaction with Family Relationships	7.92	7.00
	(0.01)	(0.19)
Age	44.0	29.8
Female	53%	19%
Aboriginal or Torres Strait Islander	2%	12%
Australian born	78%	87%
Born in other English speaking country	11%	8%
Born in non-English speaking country	11%	6%
Did not finish high school	38%	61%
Finished Year 12	14%	15%
Has certificate or diploma	28%	23%
Has Bachelor degree or higher	20%	0%
Currently Married/De-Facto	63%	32%
Number of Children 0-15	0.71	0.89
Number of Children 16-20	0.23	0.25
Number of Adults 21+	1.95	1.92
Neighbourhood Traffic Noise	2.94	3.39
Neighbourhood Other Noise	2.49	2.65
Neighbourhood Homes in Bad Condition	2.64	3.07
Neighbourhood Rubbish	2.44	2.79
Neighbourhood Teenagers Hanging Around	2.62	3.25
Neighbourhood People Being Hostile	2.13	2.91
Neighbourhood Vandalism	2.43	3.00
Neighbourhood Burglary	2.63	3.23
Major City	61%	57%
Inner Regional	25%	23%
Outer Regional/Remote/Very Remote	14%	19%
Number of Observations	34,062	145
Number of Individuals	14,148	68
Percent of All Individuals	99.52%	0.48%

Notes: Standard Errors are in parentheses. Real values are in 2001 dollars. Hours worked are winsorised at the 99th percentile and hourly wages at the 1st and 99th percentile.

Figure 1: Propensity Score of Ever Being Jailed



Notes: In the top graph, propensity scores are estimated including demographic characteristics and location as covariates. In the bottom graph, neighbourhood characteristics are also included as covariates. The mean propensity score for the never jailed is .0058 in the top graph and .0058 in the bottom graph while the corresponding mean for the ever jailed is .0206 and .0325.

Table 2: Impact of Being Jailed on Employment, Hours Worked and Total Income

	Currently Employed				Hours Worked in Last Week			
	OLS	Match1	Match2	FE	OLS	Match1	Match2	FE
Jailed								
In Previous Year	-0.178**	-0.158**	-0.139**	-0.132*	-5.325*	-2.810	-3.158	-4.596+
	(0.052)	(0.053)	(0.052)	(0.063)	(2.309)	(2.310)	(2.326)	(2.356)
Two Years Ago	-0.181**	-0.183**	-0.173*	-0.203**	-4.910+	-3.493	-4.725	-6.401*
	(0.067)	(0.068)	(0.067)	(0.074)	(2.720)	(3.037)	(2.901)	(2.744)
Three Years Ago	-0.027	-0.020	-0.014	0.006	-1.347	-0.899	-1.040	-0.200
	(0.105)	(0.098)	(0.099)	(0.096)	(3.966)	(4.127)	(3.999)	(3.563)
R-squared	0.31	0.29	0.35	0.02	0.33	0.31	0.36	0.04
Observations	34,207	1,165	1,145	31,465	34,154	1,165	1,144	31,416
Individuals	14,216	323	321	11,474	14,212	323	321	11,474
	Currently Receives Social Benefits				Log Total Real Annual Income in Previous Year			
	OLS	Match1	Match2	FE	OLS	Match1	Match2	FE
Jailed								
In Previous Year	0.218**	0.220**	0.181**	0.088	0.120	0.301**	0.219*	0.308*
	(0.055)	(0.052)	(0.052)	(0.060)	(0.079)	(0.102)	(0.098)	(0.156)
Two Years Ago	0.148*	0.188**	0.173*	0.132+	0.180+	0.347*	0.278*	0.258
	(0.072)	(0.070)	(0.070)	(0.070)	(0.102)	(0.136)	(0.129)	(0.181)
Three Years Ago	0.155	0.140	0.138	-0.015	0.142	0.343+	0.191	0.160
	(0.126)	(0.127)	(0.130)	(0.091)	(0.160)	(0.189)	(0.187)	(0.238)
R-squared	0.26	0.27	0.24	0.01	0.32	0.46	0.44	0.04
Observations	34,207	1,165	1,145	31,465	33,372	1,101	1,078	30,791
Individuals	14,216	323	321	11,474	13,998	311	302	11,417

Notes: Standard errors that account for individuals providing multiple observations are in parentheses. In the Match 1 specification, propensity scores are estimated including demographic characteristics and location as covariates. In the Match 2 specification, neighbourhood characteristics are also included as covariates.

** 1% Significance, * 5% Significance, + 10% Significance

Table 3: Impact of Being Jailed on Hourly Wages

	Real Log Hourly Wage if Wage/Salary Employee (Imputed Wages for Non-Wage/Salary Workers)							
	Only Years in Wage/Salary		Imputed as 5th Pctile Wage		Imputed as 10th Pctile Wage		Imputed as 25th Pctile Wage	
	Match 2	FE	Match 2	FE	Match 2	FE	Match 2	FE
Jailed								
In Previous Year	0.032	-0.301*	-0.097+	-0.097	-0.065	-0.091	-0.026	-0.084
	(0.064)	(0.122)	(0.050)	(0.079)	(0.042)	(0.070)	(0.036)	(0.064)
Two Years Ago	0.078	-0.316*	-0.032	-0.145	0.000	-0.114	0.040	-0.074
	(0.087)	(0.144)	(0.066)	(0.093)	(0.054)	(0.082)	(0.045)	(0.075)
Three Years Ago	-0.076	-0.254	-0.059	-0.140	-0.052	-0.133	-0.043	-0.124
	(0.078)	(0.181)	(0.084)	(0.124)	(0.060)	(0.110)	(0.040)	(0.100)
R-squared	0.31	0.03	0.24	0.01	0.20	0.01	0.15	0.01
Observations	558	16,391	1,098	29,809	1,098	29,809	1,098	29,809
Individuals	189	7,338	314	11,399	314	11,399	314	11,399

Notes: Standard errors that account for individuals providing multiple observations are in parentheses. In the Match 2 specification, propensity scores are estimated including demographic characteristics, neighbourhood characteristics and location as covariates. The corresponding real hourly wages for each imputation are $7.18 for the 5th percentile, $9.34 for the 10th percentile and $13.02 for the 25th percentile.

** 1% Significance, * 5% Significance, + 10% Significance

Table 4: Impact of Being Jailed on Economic and Social Well-Being

	Log Real Total Household Income		SF-36 General Health		SF-36 Mental Health		Life Satisfaction		Satisfaction with Family Relationships	
	Match 2	FE	Match 2	FE	Match 2	FE	Match 2	FE	Match 2	FE
Jailed										
In Previous Year	-0.158+	0.245*	-3.259	0.013	-8.995**	-6.160*	-0.407	-0.118	-0.421	-0.004
	(0.082)	(0.109)	(2.921)	(2.813)	(2.435)	(2.822)	(0.264)	(0.264)	(0.285)	(0.317)
Two Years Ago	-0.140	0.277*	-5.852	-3.114	-10.304**	-8.983**	-0.216	-0.007	0.066	0.170
	(0.096)	(0.128)	(4.340)	(3.232)	(3.610)	(3.276)	(0.256)	(0.308)	(0.414)	(0.369)
Three Years Ago	-0.312+	0.222	-2.246	-3.043	2.936	-1.212	0.225	0.454	1.304**	1.433**
	(0.178)	(0.165)	(6.029)	(4.161)	(4.456)	(4.250)	(0.340)	(0.400)	(0.447)	(0.479)
R-squared	0.46	0.14	0.08	0.01	0.18	0.00	0.15	0.01	0.16	0.01
Observations	1,136	31,312	1,126	30,943	1,140	31,324	1,145	31,456	1,123	30,569
Individuals	320	11,465	317	11,466	320	11,472	321	11,474	314	11,311

Notes: Standard errors that account for individuals providing multiple observations are in parentheses. In the Match 2 specification, propensity scores are estimated including demographic characteristics, neighbourhood characteristics and location as covariates.

** 1% Significance, * 5% Significance, + 10% Significance