Empirical Analyses on the Economics of Criminal Justice

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A dissertation submitted to the Department of Economcis in partial fulfilment of the requiremenes for the degree of

Doctor of Philosophy

Univeristy College London

January 2018

Declaration

I, Keith Lai, confirm that the work presented in thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in this thesis.

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Abstract

This thesis covers three empirical analyses on the economics of criminal justice, completed using a new micro-dataset that links up the administrative criminal, employment and benefits records of offenders in England and Wales.

The first analysis considers the effectiveness of post-custody supervision in reducing recidivism and improving labour market outcomes. It employs a regression discontinuity design and to exploit an age cut-off point in the compulsory provision of post-custody supervision, and finds that there are no effects on recidivism, employment or benefits outcomes, contrary to the belief that lead to a recent policy change.

The second analysis considers the labour market effect, or scarring, of criminal convictions. It employs a distributed lag model with fixed effects to estimate the potential damage to earnings and employment likelihood of a criminal conviction. It finds evidence that contrary to the popular belief (and simple OLS results), once individual fixed effects are controlled for, a criminal conviction even in the event where the punishment is i mprisonment is only associated with moderate damages.

The third analysis considers the effect of prison sentences on later outcomes. After the England riots in 2011, judges in riot areas were statistically handing out more prison sentences to offenders who had nothing to do with riots than judges in non-riot areas. This creates a valid instrument for testing the effect of imprisonment (at least on non-rioters). It shows that once self-selection is controlled for, prison sentences can in fact induce reduction in recidivism, likely through specific deterrence, but the effect dies out after 6 months and gives way to criminogenic factors. There are no statistically significant effects on employment, at least not within one year, though somewhat surprisingly the estimates tend to be positive rather than negative.

Acknowledgement

First and foremost, I would like to thank my supervisors Professor Stephen Machin and Professor Imran Rasul for being role models of distinguished researchers and for their continued guidance and help, esepcially Steve's, throughout the degree - it has been a most enlightening experience. Then, there are my PhD batchmates, especially Rui Costa, whose company from the very early days of the degree made the last five years a lot more bearable than it apparently ought to be. Next, acknowledgement goes to Osama Rahman, the former chief economist at the Ministry of Justice, for without his approval and blessing, I would not have had access to the micro-dataset that I based my entire thesis on. I know he has gone out of his way to make it happen, and I am truly grateful. I must also thank my family and in-laws for their trust and unwavering support during what have been probably the most turbulent years of my life yet. Last but not least, words cannot express the gratitude I have towards my best friend and wife Jaycee - this achievement would not have been possible without her encouragement, understanding, patience and love.

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Chapter 1

Introduction

This thesis is concerned with the economics of criminal justice.

The seminal paper by Becker (1968) in which the first economic model of crime was proposed kickstarted economists' contribution to this important public policy area that was traditionally studied by physchologists and criminologists. Yet, to this date there remains considerable evidence and knowledge gaps, particularly around the empirical implication of the complicated interaction and feedback loop between crime and other outcomes, and in general the effectiveness of criminal justice punishment in reducing recidivism. This is somewhat reflected in the variance observed between different countries' justice policies - for example the United States have adopted a much more punitive approach and system than in Europe, with Scandinavia on the opposite side of the spectrum. The literature on the determinants of crime is vast, as will be touched on in chapter 2, and in the face of inter-country differences on so many relevant levels, it is hard to simply infer what differences in criminal justice system can make to those complicated interactions. The problem is further complicated by the fact that experiments in this policy area would be considered unethical or putting public safety at risk (say assignment to custody is by a random lottery so some petty crime offenders would be over punished or serious offenders would be on the loose). Observational data are the only source to generate evidence. Early empirical research was based on aggregated cohort data, which is fine to a point but there are obvious limits. For most part the lack of good quality micro-data has been a stumbling block to progress in the empirical literature. But this is starting to change. For example, Norway is particularly good at making use of its existing administrative micro-datasets and linking them up to allow hollistic analyses of many policy areas including crime and criminal justice. This is evident in its emerging status as the hotbed for social policy empirical analyses.

In this thesis I take advantage of a new data-sharing initiative between the justice and labour departments in the UK government, in which a new micro-dataset encompassing crime, benefits and labour market outcomes of offenders in the UK is created for the first time, to shed light on three topics that are part of the current policy debate but without empirical consensus. Each topic is discussed in a chapter, following an overview of the literature on the economics of crime and criminal justice in chapter 2, and a description of the aforementioned new dataset in chapter 3.

My first empirical contribution, discussed in chapter 4, is to the debate on the effectiveness of post-custody supervision in influencing outcomes. I use Regression Discontinuity Design to exploit a previous policy feature in the England and Wales criminal justice system in identifying the treatment effect of a 3-month post-custody supervision period on later outcomes. According to the law before 2015, within the group of adult offenders sentenced to less than 12 months in custody, the allocation to post-custody supervision is completely determined by age: only those under 21 at the time of conviction are on licence upon release. Using the new micro-dataset I find that the 3-month supervision period, during which offenders have to comply with conditions and undertake programmes aimed at reducing their recidivism, has no impact on 1-, 2- and 3-year recidivism, benefit claim and employment outcomes. Results from Duration Analysis applied in a Regression Discontinuity spirit further reveal that not even very short term impact can be detected, such as specific deterrence or incapacitation effect during the 3 months on licence. The robustness checks all return satisfactory results and point towards a very strong RD design, endowing my estimates with very high validity. In 2015, the law was changed such that all adult offenders sentenced to custody, regardless of sentence length and age, shall be supervised on a mandatory basis for a minimum of 12 months. The implication of my results for the current policy debate is that an expansion of the provision of post-custody supervision in its previous form to simply more offenders may not be a cost-effective measure in preventing recidivism and facilitating offenders' social re-integration.

My second empirical contribution, discussed in chapter 5, is on the effect of criminal convictions on labour market outcomes. The popular belief, as evident from a quick internet search, appears to be that a criminal record would cause long term and potentially irreversible damage to employability and hence earnings. This is consistent with the observation one can get from studying the cross-sectional differences between groups of offenders and non-offenders. However, cross-sectional differences do not infer causality, but merely correlation. Utilising the panel structure within said dataset, I estimate the relationship between criminal record and labour market outcomes by using a distributed lag model with fixed effects. I find mild negative effects of convictions on employment likelihood that persist for at least 10 quarters, and some evidence that the damage on earnings dies out after 10 quarters. This is in line with a hypothesis of statistical discrimination combined with employer learning. I estimate the effect by punishment type, and find consistent with the existing literature that a prison spell has the largest and most persistent negative effect, while less severe punishments like fines and police cautions have smaller effects. There is little evidence that crime types matter. I test another hypothesis whereby employment experience to date becomes a more useful signal of true productivity in the presence of a criminal record and find mixed support. I carry out the analysis separately for two periods, before and after the recent great economic crisis which I define as started in 2008.[1]I find that the results are somewhat different and inconsistent. The difference can be explained by a compositional change in the productivity of offenders, and anecdotal evidence suggests this could be the case. The policy implication is that the labour market effect of a criminal conviction may not be as severe as many, including policymakers, fear. More important drivers for the poor labour market outcomes of people with criminal records may lie with other channels that require policy interventions of different type at a different stage in offenders lives, such as education.

My third empirical contribution, discussed in chapter 6, is on the effect of custody on recidivism and employment outcome. This is a popular area of research with numerous recent contributions, largely due to availability of new datasets around the world that

[1] The technical start date of the crisis is debatable as first sign of stress in the financial system surfaced in August 2007 when the French bank PNB Paribus shut down two of its investment funds. Nonetheless, it was not until 2008 that official UK statistics confirmed the beginning of a sustained decreased in GDP. Also, the crisis only fully sank in (and was characterised) when American investment bank Lehman Brothers failed in September 2008 and British banks RBS, HBOS and Lloyds TSB were rescued by the UK Government.

follow offenders' journey through the criminal justice system and link across multiple outcomes. By far the most common strategy in circumventing the self-selection into custody issue has been to exploit the randomness in allocating court cases to judges who inherently exhibit different level of harshness as a source of exogenous variation. While this is a popular strategy, it is not problem-free. Court cases are often complex and what is observed as innate harshness to the econometrician may in fact capture features that are observed outside hard data and endogeneity may result. In this thesis, I take a different approach to the literature and exploit the England riots of 2011 as a natural experiment for exogenous variation in punishment disposal. The riots broke out on a scale that was unseen in the UK for several decades and it is well documented that the criminal justice response was very swift and judges were particularly harsh towards rioters during sentencing to "send a message" - for example a teenager was sentenced to 10 months in custody for stealing two left-footed trainers in Wolverhampton, England. I demonstrate that this extra harshness spilled over to non-rioters who committed similar offences, but only in the riot-affected areas. Effectively, non-rioting offenders who happened to be trialled in the riot areas after the riots faced an exogenous 10% hike in their odds of being sentenced to custody. Using the riot as an instrument, I find that incarceration induces very short-lived specific deterrence effect but it fades away after 6 months and gives way to criminogenic factors. There is no significant effect on employment at least within one year. The analysis also shows that prior employment record explains quite a lot of the variation in post-custody outcomes, again suggesting fixed effects at the point of prison entry are important.

In the final chapter I provide a discussion of the avenues for further research.

Chapter 2

Literature Review

2.1 Introduction

In this chapter I provide a review of both the theoretical and empirical literature on the economics of crime and criminal justice, with the aim of understanding the current state of knowledge and the existing gaps that may be plugged with the new micro-dataset in the UK, which I describe in more detail in the next chapter.

Economists have long contributed to answering the important policy question of criminal justice, but there is not always consensus, both in the theoretical and empirical literature. While economic modelling of criminal behaviour and responses to criminal justice have in general progressed from a simple static perspective to a more sophisticated dynamic perspective, the extra analytical demand that comes with the latter, combined with a lack of individual datasets on behaviour, means that empirical testing of a structural dynamic model of crime has so far proven to be difficult. Even for simpler kinds of testing of particular structural or reduced-form parameters, there are numerous empirical challenges such as heterogeneity, effect of unobservable traits, selection, and simultaneity to name a few.

2.2 Modelling punishment and crime

Criminal behaviour has long been studied by psychologists, criminologists and social researchers. Economists only joined the field rather recently, after Becker's seminal paper in 1968 on a simple rational model of crime. Since then, more sophisticated models have been developed and the literature has grown quickly. In this section, I describe and discuss the three main classes of model used in the literature to explain criminal behaviour: static, dynamic and behavioural. Apart from a few exceptions, I use the same notation for common variables across models. I also identify the implications of punishment on crime in these models, which is an useful exercise for the next section, where I review the empirical literature, link the empirical findings to the models and contemplate whether any reduced form or structural parameters related to the effect of punishment on crime have been identified to this date.

2.2.1 Static model: Becker (1968)

The first model of crime was developed by Becker (1968). In this simple model, an individual is rational and choose to commit crime if the expected utility from it is greater the expected utility from not committing crime. More formally, let $W_{c,i}$ be the monetary plus psychological income for agent i from committing the crime he is faced with, $W_{w,i}$ be his income from work (outside option), p_i be the probability of conviction (in a simple model this is assumed to be exogenous and constant across i), F_i be the punishment and U_i be the utility function, then an individual choose to commit an offence if:

$$p_i U_i(W_{c,i} - F_i) + (1 - p_i) U_i(W_{c,i}) > U_i(W_{w,i}). \tag{2.1}$$

Let O_i be the offence function for person i (one can think of it either as total number of offences committed by i or the propensity of committing an offence). Then it is clear that O_i is a function of p_i, $W_{c,i}$, F_i and $W_{w,i}$:

$$O_i = O(p_i, W_{c,i}, F_i, W_{w,i}). \tag{2.2}$$

From equation (1), assuming utility is increasing in W_i and decreasing in F_i, we can see the following predictions of marginal effects on offence:

$$\frac{\partial O_i}{\partial p_i} < 0, \ \frac{\partial O_i}{\partial W_{c,i}} > 0, \ \frac{\partial O_i}{\partial F_i} < 0, \ \frac{\partial O_i}{\partial W_{w,i}} < 0.$$

This very simple model powerfully predicts that the number of offence increases in returns to crime, and decreases in probability of conviction, severity of punishment and returns to legitimate work (outside option). Note that these predictions do not rely on the sign of the second derivative of the utility function, ie preference to risk. Whether an individual is risk-averse or risk-loving does not affect the direction of the effects, but the relative magnitude. A risk-loving person has convex utility and so would react more to a unit change in p_i, the risk factor, than to a unit change in F_i, the negative return to crime if caught. If it is assumed that criminals are risk-lovers, then this would imply policies that raise the probabilities of conviction are more effective than those that increase punishment in combating crime. To some extent, the argument that criminals are risk preferrers is supported by the empirical literature, which I will discuss in section 3. These predictions form the basis of a lot of the empirical work, and also the rationale behind traditional crime-control policies such as increasing the presence of police, building new prisons and handing down tougher sentences. Becker's model is simple to understand, but has several important limitations to its usefulness for drawing policy implications. Firstly, it assumes that participations in legal and illegal activities are mutually exclusive, which is not supported by data. Second, it has no dynamic structure. Third, the offence function should be endogenous in its determinants. For example, probability of arrest is likely to change as a person commits a crime repeatedly, or the legitimate income that one gets is likely to be partly driven by the number of offence he commits. Subsequent models represent attempts to rectify some of these issues.

2.2.2 Static model: Ehrlich (1973)

The other prominent static model of crime is that of Ehrlich (1973), which is an extension to the simple rational choice model of Becker. Instead of modelling the choice of committing crime or staying legitimate as a one-off expected utility comparison across uncertain states, this model allows for simultaneous earning of both legal and illegal (crime) income through a time-allocation set up that is also based on expected utility maximization. This model improves from the Becker model by capturing quite rightly the notion that many crimes are in fact carried out by individuals who also have legitimate earnings.

The essence of the formal set-up is as follows (from now I suppress i subscript for ease of notation). An individual can allocate his time within a period to leisure, legal work and crime participation, denoted by t_l, t_w and t_c. Income from legal work W_w is now a monotonic function in t_w, and income from crime W_c is a monotonic function in t_c. Income from legal work is certain, while that from illegal work depends upon the realization of for example the two states of world: state a, where the individual is caught and convicted with probability p at the end of the period, or state b, where the individual gets away with crime with probability $1-p$. If caught, the individual suffers a loss F_i, which is a function of t_c. Finally, utility within the period is a function of leisure, and of total earnings X_a or X_b depending on the realized state. We can write

$$X_a = W_w(t_w) + W_c(t_c) - F(t_c) \tag{2.3}$$

and

$$X_b = W_w(t_w) + W_c(t_c). \tag{2.4}$$

The individual then makes a decision on time allocation between leisure, legal and illegal work by maximizing expected utility,

$$\mathbb{E}U(X,\, t_l) = pU(X_a,\, t_l) + (1-p)U(X_b,\, t_l), \tag{2.5}$$

with respect to t_c, t_w and t_l, subject to various time, resource and nonnegativity constraints. By taking Kuhn-Tucker first order conditions, it is easy to see that an interior solution for allocation between t_w and t_c must satisfy the following equality:

$$-\frac{(w_c - w_w)}{(w_c - f - w_w)} = \frac{pU'(X_a)}{(1-p)U'(X_b)} \tag{2.6}$$

where the small letters w and f are the first derivatives of W and F with respect to their arguments. From equation (6), we can identify the factors determining allocation of time to legal and illegal activities as risk attitude, marginal expected return to crime and work, and marginal penalty. Note that the extreme allocation is that of total specialization in either crime or work, and this could happen, taking specialization in crime as an example, as a result of constant marginal wage and marginal penalty combined with preference for risky returns. This model can also be generalized easily to accommodate more than two uncertain states of the world, for example incorporating unemployment probabilities in legitimate work.

In terms of comparative statics, the implications of this model are similar to the simple Becker model. An increase in either p or f would, holding other variables constant, reduce at the margin incentives to take part in crime. The relative magnitude of the two, again as in the Becker model, depends on the risk attitude, with risk preferrer reacting more to p. Similarly, a ceteris paribus increase in the legal-illegal income differential would at the margin reduce incentives to allocate time to illegal activities. Ehrlich's more general model of choice has also given new insights that are consistent with stylized facts, such as the extent of participation in crime is important in determining response to p or f. Suppose an individual specializes in crime, ie the solution to his utility maximization results in a corner solution, then his allocation is unlikely to be affected by small changes to p, f, or the income differential. This gives an important policy implication - that 'hardcore' criminals may require different policy treatment to deter.

There are other static models in the literature such as Grogger (1998), but as they give similar insights to the above models, particularly in terms of the effect of punishment, I will move on now. While static models are certainly useful starting points for thinking about criminal behaviour and corresponding crime-deterring policy, the simplifications that individuals only consider one-period utility when making choices, and policy tools such as p and f only affect decisions and payoffs within the period during which the criminal opportunity arises, are inherently unrealistic. Also, in these models deterrence is the only effect that punishment has on offenders. In reality, the effect of punishment is more than that and could last well beyond the sentence length. For example, a criminal record can alter one's future legitimate opportunity and foster the propensity to reoffend in the future. Also, time spent in prison may present a chance for the criminal to learn tricks from other inmates, thus improving his expected future illegal returns. These together may offset any dynamic deterrence of punishment, if the individual values the resulting increased expected illegal returns more than the legitimate income (subdued by having a criminal record) loss in the future.

Thinking in a dynamic framework would open up new policy insights, such as human capital investment (education) early in life may reduce incentives for crime participation later. Clearly, static models, no matter how sophisticated, are not enough to capture the incentives mechanism fully, and not so amenable to counterfactual policy experiment. To this end, various efforts have been made in the literature to put a dynamic structure on criminal behaviour. Below I describe three such dynamic models.

2.2.3 Dynamic model: Imai and Krishna (2004)

The first model I discuss is that of Imai and Krishna (2004). It is one of the early papers that takes a dynamic structural approach in modelling crime, employment and deterrence. Their approach allows current crime participation choices to be affected by both past arrests and future consequences (wage and employment opportunities) of today's actions. Its spirit is close to a dynamic extension of Becker's seminal model.

The choice set of a person is simple: to commit a crime or not. If he gets caught, his high school graduation, employment, wages can all be affected, which in turn affect his choice. To do this, past criminal record is allowed to affect the probability of graduation, employment and wage distribution draws. The model also allows for unobserved heterogeneity.

From now on, I use t to denote point in time, rather than time resource. The state space of the model S_t at any time contains the following variables: time t age, criminal record, high school attendance, high school graduation, unemployment and wage. Criminal records depreciate at rate δ_{CR}. Each period, either 1 or 0 is added to the depreciated criminal record carried over from the previous period, depending on if the individual commits a crime and gets caught or not. The probability of unemployment takes standard logit form and is affected by age, high school graduation, criminal record in previous period and unemployment status in previous period. High school attendance is exogenous, but the probability of high school graduation takes standard logit form and is endogenous in criminal record at graduation age. The starting wage follows lognormal distribution, with the mean of the distribution affected by criminal record. The growth of wage is also assumed to follow lognormal distribution, with the mean affected by age and criminal record.

The utility of not committing a crime $U_n(S_t)$ is interpreted also as the utility of not getting caught. It depends on age (flexibly, with change of intercept and slope at key ages such as 17 and 18), unemployment status, wages, high school graduation status and criminal record. The utility of committing a crime $U_c(S_t)$ is taken to be the direct gain and depends on age, unemployment status and criminal record.

The value of not committing a crime is

$$V_{n,t}(S_t) = U_n(S_t) + \beta\mathbb{E}[V_{t+1}(S_{t+1})\Big| S_t,\ \textit{not arrested in } t] + \epsilon_{n,t} \tag{2.7}$$

and the value of committing a crime is

$$\begin{aligned} V_{c,t}(S_t) = U_c(S_t) + p\beta\mathbb{E}[V_{t+1}(S_{t+1})\Big| S_t\ , \textit{arrested in } t]+ \\ (1-p)\{U_n(S_t) + \beta\mathbb{E}[V_{t+1}(S_{t+1})\Big| S_t\ , \textit{not arrested in } t] + \epsilon_{c.t} \end{aligned} \tag{2.8}$$

where the ϵ's are i.i.d extreme valued distributed utility shocks, β is discount rate and p is the probability of getting caught. The value of committing contains $U_n(S_t)$ because it is assumed that if he does not get caught after committing a crime, then he enjoys the benefits from crime as well as the benefits of an otherwise normal life. An individual enters the period knowing the state space vector S_t. After the realization of ϵ's occur, he makes the decision to commit a crime or not. The value function is hence:

$$V_t(S_t) = Max V_{n,t}(S_t),\, V_{c,t}(S_t). \tag{2.9}$$

The above is an outline of the essence of the model. As mentioned, it is a dynamic treatment of Becker's model, and considers effects of punishment outside pure deterrence in the traditional sense, ie a one-off disutility. That it allows dynamic and endogenous relationships between crime, employment and graduation makes it a valuable contribution to the literature. The main implication for effect of punishment is that, as the future state space, ie status of graduation, employment and wage, is negatively affected by today's punishment (arrest), a dynamic deterrence mechanism is created through these channels. However, there are several drawbacks to it. First, it does not capture the effect of imprisonment appropriately. It does not allow multiple period or severity of punishment, which is important for thinking about the incapacitation and specific deterrent effect in a dynamic model. Also, the time t disutility of punishment is modelled as giving up $U_n(S_t)$ within the period, but in general should be more than that because, for example, of the unpleasant experience in prison and potential ramifications on future state space. In pursuit of simplicity, the model has forgone an important policy instrument in f. For these reasons, the model is perhaps not realistic and does not allow simulation of any policy that increases the severity or length of sentence. Second, it is a complicated model driven by a lot of parametric assumptions that are difficult to fully justify. Although the authors brought the model to data and generated some Maximum Likelihood estimates, the results may change if different assumptions are used. The complexity of the model also makes it unfriendly for testing using other data. Third, it does not have the more realistic time allocation feature as in Ehrlich (1973). Fourth, it assumes a long time horizon in the offender's evaluation of the value function, which may be too strong an assumption for this group of individuals.

2.2.4 Dynamic model: Sickles and Williams (2008)

Instead of modelling crime as a binary yes-or-no decision, Sickles and Williams (2008) present a dynamic treatment of the Ehrlich (1973) time allocation model, augmented with a 'social' capital accumulation perspective.

Social capital K_t represents reputation and status in society. Such capital naturally depreciates at rate δ_K, but individuals can also accumulate it through spending time ω_t on work, or reduce it through spending time C_t on committing a crime and consequently getting caught. Recall that a denote the state in which the individual is arrested and b the state where he is not, then

$$K_{a,t+1} = \delta_K K_t - \alpha C_t K_t, \tag{2.10}$$

and

$$K_{b,t+1} = \delta_K K_t + \gamma \omega_t, \tag{2.11}$$

where α transforms time spent in crime into stigma, and γ transforms time in labour market into social capital (eg building up network and reputation). Notice in this model that having a higher level of social capital affects positively the cost of punishment. This captures the notion quite appropriately that people with higher social status, such as a public figure, often suffer more damage to his reputation and opportunities than a normal member of the public for the same criminal justice punishment. The individual's within-period utility depends on current level of time spent on leisure l_t, composite consumption good Z_t and K_t,

$$U_t = U(l_t,\, Z_t,\, K_t). \tag{2.12}$$

Social capital also determines positively the legitimate earnings W_ω that the individual receives, along with time allocated to employment. This is captured in the intertemporal budget constraint:

$$A_{t+1} = (1+r)[A_t + W_\omega(\omega_t,\, K_t) + W_C(C_t) - Z_t], \tag{2.13}$$

where r is the interest rate, W_C is illegal income as before, and A_t can be interpreted as physical capital. Another interesting innovation of the model is that probability of arrest p is no longer exogenous to the individual, but he can affect it through his intensity of committing crimes. Governments can also influence this probability by spending more resources R_t on law enforcement:

$$p_t = p(C_t,\, R_t). \tag{2.14}$$

Note that The effect of R_t on p_t is expected to be positive, but the effect of C_t is less clear. On one hand, being involved in more crimes during a single period would raise the chance of arrest. However, the individual may also improve his criminal skills by virtue of practice makes perfect, and become better at avoiding punishment.

The individual's problem is then to maximize his expected discounted utility. The Bellman's equation of his dynamic programming problem, as characterized by the value function $V(A_t,\, K_t)$ in period t, is:

$$V(A_t, K_t) = \max_{Z_t, \omega_t, C_t} U(l_t,\, Z_t,\, K_t) + \beta \left\{ p(C_t,\, R_t) V(A_{t+1},\, K_{a,t+1}) + [1 - p(C_t,\, R_t)] V(A_{t+1},\, K_{b,t+1}) \right\}, \tag{2.15}$$

subject to time constraint and equations (10),(11),(13). This model incorporates many ideal elements of a model of crime, such as the endogenous probability of arrest. The innovation of social capital is also an interesting contribution to the literature. Compared to Imai and Krishna (2004), the dynamic effect of punishment on labour market outcomes works via damage to social capital, rather than criminal record directly entering employment equation. Within this model it is possible after punishment to accumulate social capital quickly by investing time in work and catch up with the capital level of non-criminals, whereas in the previous model the effect of punishment on wage and employment is more persistent if the depreciation of criminal record is slow. Consequently, the authors suggest that an effective policy to prevent individuals from pursuing a lifetime of crime would be to foster the social capital of the disadvantaged. This is consistent with the current policy momentum on rehabilitation during sanction, which can credibly be modelled as boosting social capital. Despite the model having a lot of interesting features, it has several weaknesses. Similar to Imai and Krishna (2004), it assume long time horizon in individual's dynamic optimization, and it does not allow multi-period punishment (notice however, while explicitly it lacks f, the αC_t here serves similar modelling purpose). Also, while the authors demonstrate an estimation algorithm for the structural parameters that involves calibration of parameters, simulation techniques then simulated method of moments, in general the model is not friendly for empirical work.

2.2.5 Dynamic Model: McCrary (2010)

The final model I study is that of McCrary (2010). The main improvement of his model over others in the literature is that it allows for punishment lasting longer than one period, which is a useful modelling assumption. The author argues that this is important because typically some offences are punished with long prison sentences, so the notion of punishment being a single period utility loss is inappropriate. The model is also developed so that it can easily be tested with commonly found longitudinal data on arrest. This gives it an advantage over other dynamic models, which typically require richer and harder-to-find datasets. I now take a closer look at the model, before discussing its strengths and weaknesses.

Suppose infinitely-lived agents face the same problem in every period of committing a crime or not. The benefit of crime W_c in each period is drawn randomly, following some distribution $F(w_c)$ with density $f(w_c)$. Notice the slight change of notation here - W_c is a random variable and w_c is the realization (instead of first derivative). If an agent commits a crime, the probability of getting punishment is p. The punishment is imprisonment for J periods, where J is a random variable taking on values $j = 1, 2, 3$, etc., with probability π_j. After criminal benefit w_c is drawn at the beginning of the period and the agent makes a decision, his utility in that period can then take on 3 values, depending on his decision and the uncertain arrest outcome. If he commits a crime and gets away, then he receives utility flow $w_w + w_c$, ie outside option plus benefit from crime. If he does not commit a crime, then utility flow is w_w. If he commits a crime and gets caught, then the utility flow is w_w - f for each of the j periods that he is incapacitated. It is assumed that $w_w + w_c > w_w > w_w - f$. The agent's objective is to maximize the sum of current and expected future utility flows, discounted at constant rate β, by choosing to commit the crime or not. Time homogeneity is assumed, so the agents will not obtain additional information in the future that he does not have access to in current period.

If the agent stays away from crime, he receives payoff $w_w + \beta\mathbb{E}[V(W_c)]$, where $V(W_c)$ is the value of being free next period and presented with a future opportunity to commit a crime with value W_c (this is uncertain in current period but follows the same distribution over time, hence the expectation operator without time index). If he commits the crime with value w_c this period and gets away, the payoff is $w_w + w_c + \beta\mathbb{E}[V(W_c)]$. If he is caught and has to face imprisonment for j periods, then his payoff is

$$(w_w - f)(1 + \beta + \beta^2 + ... + \beta^{j-1}) + \beta^j \mathbb{E}[V(Wc)]. \tag{2.16}$$

Therefore, the value of being free and being presented a criminal opportunity worth $W_c = w_c$ is

$$\begin{aligned} V(w_c) = max\{&w_w + \beta\mathbb{E}[V(W_c)], \\ &(1-p)(w_w + w_c + \mathbb{E}[V(W_c)]) + p\sum_j \pi_j[(w_w - f)\frac{(1-\beta^j)}{(1-\beta)} + \beta^j \mathbb{E}V(W_c)]\} \end{aligned} \tag{2.17}$$

In this model, the optimal strategy is to have a reservation value $w_c{}^*$ and only commit crime if the realization of W_c is above it. At $w_c{}^*$, the two arguments inside the maximum operator is equalized, and it is possible to solve for $w_c{}^*$ analytically to study comparative statics. Note that an increase in $w_c{}^*$ implies lower ex-ante probability of crime and so fewer crime in the population. This is be seen from the resulting increase in $F(w_c^*)$.

The model gives unambiguous prediction that crime can be reduced ($w_c{}^*$ can be increased) by increases in p, f and β. These are similar predictions to other models in the literature, dynamic or static. Crime can also be reduced in the model by a shift in the probability of sentence length draw towards longer sentences, ie an increase in $\mathbb{E}[J]$, as long as the agent cares about the future and does not have $\beta = 0$. This is an interesting and important point often overlooked in the literature, as the author pointed out. Most dynamic models invoke the assumption of long time horizon during the individual's maximization problem, but evidence has shown that this assumption may be inappropriate because many crimes are committed when offenders are experiencing diminished capacity due to drug, alcohol or overwhelming emotional impulsion (McCrary, 2010). This has policy implication for governments, specifically that longer sentences may not at all have an deterrent effect. The last interesting comparative statics, despite not so relevant to the effect of punishment, is that of the effect of an outward shift in the distribution of criminal benefits, or in other words crime becoming more profitable on average. The model prediction is ambiguous, which may be counter-intuitive and a departure from other models. The reason is that while crime has become more profitable and attractive in the current period, the opportunity cost of crime commission is also higher now that the agent has to risk imprisonment and not being able to take advantage of more valuable crime in the future. In the extreme case, the future opportunity can so good that agents never want to commit crime today and risk imprisonment. The two effects work in opposite directions, hence the ambiguous prediction.

Assumptions around time homogeneity can be relaxed to allow for more general modelling. Despite the model's advantages, there are several drawbacks. Unlike the previous two dynamic models, labour market outcomes (outside option) and probability arrest are exogenous here, which is an unrealistic assumption. Also, the binary choice set of crime commission is also undesirable.

While I have presented three dynamic models which offer different but equally valuable perspectives, there are also several other important contributions which I will not discuss here, including Huang et al. (2004), Imrohoroglu et al. (2004), Burdett et al. (2004) and Lochner (2004). It should be clear that to this date, dynamic modelling of punishment and crime remains a very difficult branch of work, with a lot of ongoing debates and a lot more to be understood. In my view, it will be interesting for future models to push on in two directions, despite the cost of higher complexity. First, it will be useful, especially for policy simulation, to improve assumptions on available policy instruments. It is too simplistic to assume governments only having p or f at disposal. There are many dimensions of punishment not captured so far, for example a prison sentence is not merely an increase in f to a community sentence, and many argue those dimensions do interact with outcomes. Putting a formal structure around those dimensions and their effects on crime will enrich the modelling of punishment and perhaps give new insights to both academics and policymakers. Second, crime is a phenomena closely related to outcomes in labour market, accommodation, health, marriage, etc., and future models can look at incorporating more of these dynamic relationships. This will allow better understanding of the effect of different types of interventions on preventing recidivism.

2.2.6 Behavioural model

Recently, there are developments in drawing lessons from behavioural economics to model crime. The main argument to using a behavioural economic approach as opposed to the 'classical' approaches outlined above is that, while a expected utility/rational framework is useful for gaining insights into how incentives may affect behaviour, people may not behave as predicted by expected utility theory (Kahneman and Tversky, 1979). Garoupa (2003) provides a useful summary of the debate in relation to criminal behvaiour. The criticism is three-fold. First, in the rational approach it is assumed that individuals have preferences about each possible state of the world before taking an action (ie deciding on the 'lottery' to pick). However, it has been argued that some individuals become criminals because of isolation from anti-criminal patterns during upbringing and so instead of having rational preferences over all states, rather they are constrained by the incomplete information set, shaped by the contact they have with criminal patterns, that they have. Second, the assumption that criminals choose an action based on comparison of marginal costs and benefits and maximization of utility may not be appropriate. Garoupa (2003) point out inaccuracies, or even contradictions, between predictions from an expected utility model and observed actual behaviours. Explanations for such limited rationality include task complexity (too costly to compare costs, benefits or calculate risks), manipulation of beliefs (for example, overconfidence), ambiguity of risk of apprehension (classical expected utility theory only takes into account risk, but not ambiguity), and limited opportunism (that individuals care about costs and benefits of others under some circumstances). Third, it has been argued that individuals respond to perceived rather than actual changes, and the discrepancy between the two leads to inability of the classical approach in accurately predicting response to changes in policy instruments. Reasons for this include ignored moral costs on the part of criminals in the model, over-simplistic modelling of enforcement decisions and the criminal market arguably being a different kind of market to those typically considered in economics.

A behavioural crime model (van Winden and Ash, 2012) would instead treat people as having limited rationality, and their criminal behaviour motivated by both cognition and emotions. I now discuss the most relevant cognitive and emotional biases. In most cases below, a behavioural model may only require tweaks, instead of overhaul, to the classical approach.

2.2.6.1 Emotional factors

The recognition of emotions as an important part of economic behaviour (Elster, 1998; Loewenstein, 2000; van Winden 2007) is growing, but there is no formal behavioural economic model to this date that incorporates them. For this reason, I do not discuss emotional factors in great length, but only list the most relevant ones for criminal behaviour as identified in the literature: anger, altruistic punishment, shame, guilty, social norms, empathy, sympathy and social ties. While the behavioural implications on crime of the effect of emotions are still very much a matter of debate both theoretically and empirically, some 'classic' models have these features partly built-in. For example, Sickles and Williams (2008) introduces social capital, which can be seen as a proxy for social ties. Until further research, it is not clear whether simply extensions to the rational approach will be enough to accurately capture the effect of emotions and their interplay with other determinants of criminal behaviour.

2.2.6.2 Cognitive factor: risk attitude

Behavioural economists argue that prospect theory, where outcomes are evaluated against a reference point and probabilities are transformed into decision weights with more weights on small probabilities, should replace expected utility theory in criminal modelling. Such a claim is supported by experimental evidence that shows non-linear risk attitudes amongst subjects (Tversky and Kahneman, 1992). The policy implication under prospect theory is that, since individuals are risk averse towards prospective gains but risk loving towards losses, and punishments are losses, so governments should make the punishment as predictable as possible. This is a different way of modelling but consistent with findings from simple rational model assuming preference for risk. The characteristics of the prospect theory prediction can be incorporated into a classical expected utility approach by reweighting the probabilities of the uncertain states accordingly.

2.2.6.3 Cognitive factor: loss aversion

Experimental evidence further shows that losses and gains are perceived asymmetrically, that losses loom larger (Tversky and Kahneman, 1991). This means that within the prospect theory framework, the marginal utility is steeper in the negative than the positive domain as defined by the reference point. The policy implication is that punishment should impose a bigger deterrent effect on the population than predicted by the classical approach. Loss aversion can also be easily incorporated into a classical approach by introducing non-linear marginal utilities.

2.2.6.4 Cognitive factor: time preferences

The classical dynamic approach typically assumes exponential discounting. That is, each additional period is discounted by the same factor β. This would conveniently lead to time consistency in prediction. For example, preferences over risking arrest or not remain the same over time. However, critics point to experimental evidence that subjects demonstrate stronger preference for immediate gains over future gains (Ainsley and Haslam, 1992), and often criminals start to regret their actions at the point of sanction, showing time inconsistency in their decision-making process (Jolls, Sunstein and Thaler 1998). They argue for hyperbolic discounting to replace the classical exponential discounting. This gives new insights such as swift justice may be important as a crime deterrent. Incorporating hyperbolic discounting in a classical approach can be done, in its simplest form, by having non-constant discount rate β_t, which starts low for low t, but increases over time.

Insights from behavioural economics can no doubt add to the traditional rational approach of modelling criminal behaviour. Experimental evidence has shown that people do not necessarily behave as the classical rational approach predicts, and so policy simulations may perform better by involving lessons from a behavioural approach to crime. However, the main drawback to this approach is that it lacks a unifying theory on why those deviations from expected utility theory exist, or how and if those cognitive and emotional factors are interlinked. Policy recommendations based on a behavioural approach to reduce crime, such as having large, prominent, and gaudy parking tickets (Jolls, 2005), may seem ad-hoc and difficult to comprehend within a general structure or model of behaviour, as opposed to policy instruments such as changing the severity of punishment, which has more solid theoretical groundings. In my view, at this stage research should actively review if some of the classical assumptions do fail and by how much, and at the same time exercise caution when adding elements of behavioural economics to the modelling of crime.

2.3 Empirical evidence

There is a large body of empirical research dedicated to verifying the relationship between crime and various variables and policy instruments. Some of the investigations are motivated directly by the predictions of existing theoretical models, including those discussed in the above section. The parameters estimated in this kind of research typically have some structural meaning to them in relation to the models used. Other studies are more focussed on estimating reduced form parameters. They are close to studies in the treatment effect literature in spirit. In this section, I concentrate on summarizing the empirical findings related to crime and punishment, the main relationship of concern to this dissertation. Before that, for completeness I also briefly describe the research on crime and non-punishment variables.

2.3.1 Crime and non-punishment variables

One of the major predictions of the simple rational model of Becker (1968) and most subsequent models is that criminal behaviour can be affected by legal labour market conditions, or more specifically, wages and unemployment (expected wages). A selection of distinguished papers that have investigated this relationship (or its variants) include Witt et al. (1998), Grogger (1998), Doyle et al. (1999), Gould et al. (2002) and Machin and Meghir (2004). They generally find evidence for unemployment or lower relative wages in explaining the rise of criminal behaviour in particular groups of offenders. However, in a dynamic setting Imai and Krishna (2004) find that lower unemployment can instead induce more crime, due to the lower expected cost of incarceration in terms of difficulty in finding a job afterwards. This is an interesting proposition and is consistent with theoretical prediction, but within the context of previous discussion on the limitations of their model and results from other empirical studies using panel or time series data, one should perhaps not place too much weight on their findings.

There are also studies that consider business cycles and poverty and inequality as determinants of crime. Cook & Zarkin (1985) find that a 1% point increase in unemployment from the long term trend is associated with a 2.3% point increase in robbery and 1.6% point increase in burglary, but not associated with homicide. Hsieh & Pugh (1993) conclude after a meta-analysis that poverty and income inequality are moderately and positively associated with violent crime. Kelly (2000) also find that inequality has robust impact on violent crime, and property crime only associated with poverty. The former is attributed to strain and social disorganization theory, while the latter is consistent with standard economic theory.

Lochner's (2004) human capital model of crime predicts that crime participation is negatively correlated with human capital, which is accrued through education and experience (in other words, age). The crime-age relationship is well documented and long been studied by sociologists/criminologists, see for example Hirschi & Gottfredson (1983). The crime-education relationship has not been studied as much as crime-age, but recently the empirical literature on that is growing. For example, Machin et al. (2011) find, using regression discontinuity design and exploiting an exogenous policy change in compulsory schooling age, a negative effect of education on crime.

Other variables that have been studied by economists, albeit more in a reduced form manner and less motivated by proper models of criminal behaviour, include drug use (Grogger & Willis 2000, Levitt 2004) and legalized abortion (Levitt 2004). These empirical studies found evidence for the drug use increasing crime commission and abortion reducing it.

2.3.2 Crime and punishment variables

Hypotheses developed in the theoretical literature have generated a set of predicted effects that the criminal justice punishment can have on crime, at both micro and macro levels. We have already seen some of them in section 2. First of all, there is the general deterrent effect. As probability of apprehension or sentence length goes up, expected ex-ante utility from crime decreases for all individuals at the margin and so does crime participation. Similarly, according to the specific deterrence hypothesis (eg Smith & Gartin 1989), contact with the criminal justice system, for example a period of imprisonment, can reinforce an offender's perception of the likelihood of arrest or the severity of punishment, thus deterring him from recidivism. Apart from deterrence, Ehrlich (1981) point out that criminal justice punishment can also reduce crime through the incapacitation effect, ie physically prohibiting criminals to commit crime, but it may be offset in the other direction by the replacement effect, whereby the criminal opportunities not taken up by the incarcerated offenders are simply taken up by new entrants to the criminal markets. Ehrlich (1981) also suggests the existence of a rehabilitative effect on criminal behaviour for programs that criminals have to undertake during punishment. In contrast, Bayer et al. (2009) argues instead that prison can act as a "school for criminals" where inmates learn from each other and become better and more likely reoffenders in the future. From a capital accumulation perspective, contacts with criminal justice system may depreciate human capital (Ehrlich 1981) or social capital (Sickles and Williams 2008), thus reinforcing criminal behaviour in the future. As observed by Cameron (1988) and Frey (2009), the causal relationship between punishment and crime is highly complicated. Unsurprisingly, it is difficult to disentangle and estimate the many effects. Nonetheless, reasonable progress has been made in the empirical literature.

General deterrent effect, especially of more effective crime detection in the form of increased police numbers, has been studied relatively extensively and the evidence is in general supportive. Using the number of fire-fighters to instrument police numbers, Levitt (1997) finds a significant and positive relationship between police deployment and crime reduction. Applying quasi-experimental econometric technique to the terrorist attacks in Bueno Aires in July 1994, and in London in July 2005, Di Tella and Schargrodsky (2004) and Draca et al. (2011) respectively find similarly strong evidence on the deterrent effect of police. It is harder to identify the general deterrent effect of longer sentence length because it generally is observed together with the incapacitation effect. Nonetheless, Helland and Tabarrok (2007) manage to estimate it with a credible empirical strategy utilizing the "three strike" legislation in California. Lee and McCracy (2009) also estimate the deterrent effect using a quasi-experimental design which separates out incapacitation, and remark that the elasticity is significant but very small. Drago et al. (2009) find using the a natural experiment in Italy that the elasticity of average recidivism with respect to the expected punishment equal to 0.74 for a 7-month period. In general the evidence here suggests that the response to an increase in crime detection is stronger than to an increase in severity of punishment, confirming Becker's (1968) early conjecture that criminals are risk-lovers. The general deterrent effect of capital punishment is unclear, as Nagin et al. (2012) conclude after a comprehensive survey of existing research. They claim that the literature has failed to validly identify the marginal effect of death penalty over an alternative lengthy prison sentence, and that there is no plausible models of murderers' perceptions of and response to capital punishment.

There is mixed evidence on the effect of incapacitation. On one hand, Levitt (1998) and Kessler & Levitt (1999) find that the size and direction are similar to that of deterrence. On the other, sociologists Blokland & Nieuwbeerta (2007) conclude after a review of evidence in their field that, although the estimated imprisonment elasticity of crime rate range from 0% to 2.2% reduction, most of the studies do not adequately control for the simultaneity between crime rate and imprisonment. Apart from this, I am also not aware of research that explicitly distinguish between the incapacitation and displacement effects. If crime opportunities left behind by the incarcerated criminals are taken up by new entrants, then not taking into account this displacement will under-estimate the true effect of incapacitation. The effect may also be over-estimated if the displacement is in time, that is if individuals simply delay crime commission into the future. Overall, while existing studies all point towards a small but significant positive impact of incapacitation on crime reduction, there are in my views very few credible estimates. This is a gap in the literature, and one which is probably quite hard to fill for the reasons mentioned.

With regards to the rehabilitative effect of criminal justice punishment, the evidence is sparse at best, at least in the economics literature. Levitt & Miles (2007) remark that there are very few economic studies contributing to this area of research. Sociologists have attempted to evaluate the effect of various rehabilitative programs but their designs tend to suffer from attrition and selection biases, rendering their estimates incredible.

On the effect of punishment on human or social capital, we can refer to the relatively large empirical literature on punishment and labour market outcomes. Waldfogel (1994) finds a strong negative effect of imprisonment on the likelihood of employment and wages. Grogger (1995) using fixed effect models similarly finds a negative, albeit short-lived, effect of arrest on wages and employment. Kling (2006) finds that longer sentence lengths are not correlated with more negative labour outcomes. To summarize, as Freeman (1999) conclude in his wide-ranging review, the empirical research tends to find a negative impact of punishment on individuals' labour market outcomes. The evidence seems to suggest that this 'scarring' effect is at its greatest at the first entry to the criminal justice system, with much smaller marginal effect for further contact. Given the prediction from an economic model of crime that labour market outcome is negatively related to criminal behaviour, the empirical findings here would suggest that punishment can be criminogenic in this sense. A more in-depth discussion on this topic is given in chapter 5.

So far, the empirical studies reviewed in this section focus more on identifying parameters that have structural meanings in an economic model of crime. Within their neighbourhood in the literature, there are also works on identifying reduced forms parameters instead. Structural parameters are invariant to the economic conditions, so the studies reviewed so far are informative with respect to giving policy predictions or simulations under alternative environment. On the other hand, research on estimating reduced-form parameters, which I review below, offers direct evidence on the effectiveness of policies in a programme evaluation sense. It does not require a firm theoretical model to motivate empirical testing, and in concentrating on identifying the aggregate policy effect, it serves a different but equally useful purpose to structural parameters estimation. It also faces different difficulties, ones that are akin to those in the treatment effect literature, ie selection on unobservables and heterogeneity.

Both sociologists and economist have contributed to this literature, but using different research designs. Sociologists Nagin et al. (2009) conclude after reviewing existing evidence in sociology that the effect of imprisonment on subsequent criminal behaviour appears to be null or criminogenic, rather than a preventative one. They remark that the majority of sociological research in this area uses matching design in one form or another as the identification strategy. Weisburd et al. (1995), using propensity score matching, find no negative effect of imprisonment on re-arrest rates over a period of 10 years for white-collar crime offenders. Nieuwbeerta et al. (2009) combine trajectory modelling with propensity score matching, and find that first-time imprisonment is associated with more criminal activities in the 3 years following release. Wermink et al. (2010) also use propensity score matching to compare offenders sentenced to custody and community punishment, and conclude that offenders on the latter are less likely to reoffend. One should exercise caution, however, when interpreting results from matching designs, because unless selection is entirely based on observable variables the estimates are likely to suffer from selection bias. For example, if criminals are sentenced to custody rather than community punishment due to the perceived risk of reoffending associated with his personality traits and this information is not available in the dataset, then matching designs will likely over-estimate the apparently criminogenic effect of imprisonment. There are other attempts by sociologists to use randomized experimental designs, such as Killias et al. (2000) and Green & Winik (2010). Their results agree that the marginal impact of imprisonment over less severe sanction, such as community punishment, is crime-inducing on the individuals.

Economists' take on estimating the treatment effects of interest tends to be more creative, often by exploiting exogenous features of the system as sources of identification. Kuziemko (2012) applies Regression Discontinuity design to the cut-off parole rules in Georgia, USA to estimate the effect of additional time served in custody on recidivism. She finds a large negative effect of an extra month in prison on recidivism rate. In contrast, Marie (2009) also applies Regression Discontinuity to the Home Detention Curfew scheme in the UK justice system, where prisoners sentenced between 3 months to 4 years for relatively minor crime types are released early, and finds that offenders sentenced to 3 months in custody who are eligible for early release have lower re-offending rates by up to 5% than those who are sentenced to just under 3 months and spend more actual time in prison. While the estimates of effect of additional time in custody from the two studies seem to contradict each other, it is worth bearing in mind that the discontinuities in the two systems occur at different locations along the sentence length spectrum, and so it is entirely possible that along it the local effects are very different and have opposing signs. In another economic study, Di Tella & Schargrodsky (2013) make use of the random assignment of offenders to judges in Argentine justice system, and estimate by OLS/IV that the recidivism rates of offenders sentenced to electronic monitoring is 9% lower than those sentenced to custody. Nagin and Snodgrass (2013) also make use of randomization of cases to judges in Pennsylvania to find that incarceration has little effect on reoffending behaviour from 1 year up to 10 years after release. Mueller-Smith (2015) argues that the popular approach of using judge randomisation as instrument suffers from the assumption of monotonicity and exclusion, and shows that bias can result if they are violated. He proposes an improved estimation procedure that takes into account of this and finds that prisons are criminogenic instead of having no effects. There does not seem to be any clear consensus in the reduced-form literature, particularly in the economics literature, about the effect of sanction on future criminal behaviour, but this is not so surprising. While structural parameters are invariant to the economic conditions, reduced form parameters are not. For example, the effects identified in each of the aforementioned studies are likely to be different types of treatment effect, ie Regression Discontinuity designs identify local treatment effects, while matching typically identifies average treatment effect on the treated. It is certainly useful in the future to develop a structural model of recidivism, for instance, to understand better if and why local treatment effects along the sentence length spectrum may be different, and connect these separate studies to form a full picture of recidivism behaviour.

2.4 Concluding remarks

Looking across the literature it is clear that the amount of work carried out using micro-datasets that span across crime and other outcomes is rather lacking. This is unsurprisingly due to the lack of suitable datasets as well as often the sensitivity and difficulty involved with joining up individual-level data. Evidence for the UK is especially lacking. The new micro-dataset that I have access to, which I describe in more detail in the next chapter, goes a long way in providing the necessary material to answer many of the cross-cutting questions about crime, labour market and benefits.

In this thesis I choose to focus on three topics that I have touched on above already that are without clear consensus: the rehabilitative effect of post-custody supervision program, the labour market effect of criminal conviction, and the effect of custody on recidivism and labour market outcomes. In the final chapter I discuss further areas of research not provided in this thesis that are possible with similar micro-datasets and should be addressed by the literature in the future.

Chapter 3

Data

The empirical analyses in the rest of this thesis are all carried out using some combinations of the following four micro-datasets in the UK: the Police National Computer (PNC), the National Benefits Database (NBD), P45 Employment database and P14 Earnings database. When datasets with personal information are administered by different government departments, they are typiclly not shared nor linked to each other due to data sharing legal restrictions. This was the case with the four datasets Ise in this thesis. The PNC extract is held by the Ministry of Justice (MoJ), NBD by the Department for Work and Pensions (DWP), and P14 and P45 databases by Her Majesty's Revenue and Customs (HMRC). Fortunately a breakthrough arrived in 2011, when the MoJ reached agreement with the DWP and HMRC to enter a data share of these four datasets. The intention is that the arrangement would enable hollistic analyses of the interaction between criminal, benefit and employment outcomes to inform better policymaking in criminal justice and reoffending reduction.

In this chapter I provide general descriptions for all of them. The technicalities of applying the datasets to the different research designs, such as time period and sample will be discussed later in the relevant chapters.

3.1 Police National Computer

The PNC is the administrative IT system managed by the Home Office of the UK Government and is used by all police forces in England and Wales. The PNC covers all offences that are punishable by imprisonment plus many of the serious summary offences (ie recordable offences) and contains offender level information. It generally does not cover less serious offences that most likely attract fines as punishment, such as TV license evasion, careless driving, driving without insurance, reproducing British currency notes, etc. Across all police cautions and disposals dealt with by courts including custody, probation, fines, discharge, 55% are recorded on the PNC. Coverage across all sentence types is very high except fines, where just less than a fifth is recorded (Ministry of Justice 2014). Despite the PNC not covering non-recordable offences which make up a significant part of overall crime and which explains the big difference between the PNC and the British Crime Survey crime numbers, arguably leaving them out does not affect my analyses. This is because the punishment of interest in this thesis is typically custodial sentence, and also the fact that non-recorded crimes do not show up in criminal checks hence unlikely to affect employment outcomes as much as recordable crimes. This point will be further discussed in the relevant chapters. I use the extract of PNC that the Ministry of Justice (MoJ) holds, and use in particular the variables on information about the offence, conviction, punishment and offender characteristics (age, gender and ethnicity). The timeframe of the extract that has been matched to external datasets is between 2000 and 2013.

3.2 National Benefits Database

The extract of NBD that I have access to contains information on claims to all DWP benefits made by offenders who can be matched to the PNC between 2000 - 2013. Note that not all benefits are recorded on the National Benefits Database. For example, child, housing and council benefits are recorded outside the NBD since they are administered by other government departments outside DWP. There are in total twelve types of benefits available in the data: Attendance Allowance, Bereavement Benefit, Disability Living Allowance, Employment Suport Allowance, Incapacity Benefit, Carers Allowance, Income Support, Jobseekers Allowance, Pension Credit, Passported Incapacity Benefit, Retirement Pension, Sever Disablement Benefit and Widows Benefit. Out-of-work benefits are most relevant to this thesis in terms of proxying whether offenders require state help, and they are all included in the data. The main information from this data that I exploit in the analysis is the start and end date of benefit claims.

3.3 P45 Employment

The extract of P45 Employment that I have access to, contains employment date information for offenders who can be matched to the PNC between 2000 - 2013. In the British system, P45 is the reference code of a multi-part form officially titled 'Details of Employees Leaving Work', issued by the employer when an employee leaves. Part of it is submitted to HMRC for individual's tax record purposes, and it is the employment start and end date information on there that provide information about period of employment of offenders. One note of caution on P45 employment is that it does not cover all employment. For a start, self-employment and cash-in-hand jobs are not recorded in the data. Also, for my period of analysis employers were only required to submit P45 forms for periods of employment that are above the Lower Earnings Limit (around £100 per week). Hence, P45 employment under-estimates total employment, and this may be particularly problematic for offenders, as one may view them as more likely to take up self-employment, cash-in-hand or lower paid jobs. Despite this, trends and differences in P45 employment between groups of offenders should to a large degree reflect trends and differences in true employment between the same groups, which are the outcomes I am ultimately interested in. Also, there are no alternative adiministrative datasets anyway that capture self-employment or cash-in-hand jobs. For these reasons I argue that P45 employment is a good proxy for offenders' true employment.

3.4 P14 Earnings

The P14 Earnings database covers income information derived from P14 forms that employers sent to HMRC. At the end of each tax year, employers normally complete a three-part form for each of their employees, regardless of the length or mode of employment, about their taxable income and deduction through income tax and National Insurance contributions. The first two parts are sent to tax offices and form the P14, while the third part is issued to the employee and commonly known as the P60 End of Year Certificate. Note that employers are not required to submit the P14 for all workers – only those with earnings above the Lower Earnings Limit just like in the P45 Emplyoment dataset. Despite this, in reality I still observe some entries with stated pay lower than the threshold, showing some employers would report anyway. I am not able to determine the coverage of the P14 dataset in the low pay region, but I assume that reporting there is a random event. Note also that due to the nature of P14, income from self-employment or cash-in-hand jobs is not included. I assume, like above, that trends and differences in P14 earnings between groups of offenders reflect those in true earnings. From the P14 database I observe and use variables on the start and end dates of employment spells and the corresponding pay. Wages are normalized to 2008 level using the Office of National Statistics GDP deflator.

From the discussion of the P45 and P14 databases so far, clearly it is possible to derive employment spells for individuals from either data sources. The results from both are, however, not always consistent. There are employment spells that appear in one but not the other, though there is an overlap of over 90%. This is a common issue with administrative datasets. Because of the inconsistency, I will only use either P45 or P14 within the same analysis when considering employment outcomes and never in conjunction. There is not enough information to judge which administrative dataset is better in terms of accuracy so the default choice within an analysis is simply driven by whether earnings are considered as an outcome, in which case P14 will be used as seen in the chapter 5. This is so because the structure of raw information held wthin the P14 dataset is more complicated to analyse - so unless required, I work with the P45 instead.

3.5 Matching between datasets

The matching between PNC, NBD, P45 and P14 was done by MoJ and DWP using a quality-assured methodology (see annex A of Ministry of Justice, 2014a). Over 80% of the 5.2 million PNC records since 2000 were successfully matched and the MoJ had conducted tests to ensure the representativeness of the match. The only differences in the distribution of key variables between the matched and unmatched data are in ethnicity (slightly lower proportion for ethnic minority) and disposal category (high number of cautions in the unmatched data). Problems as such are not uncommon for matching across multiple micro-datasets and for most part I am going to take the quality of the data as granted and fixed. Where relevant, issues stemming from the imperfect matching and data imputation will be discussed in the next three empirical chapters.

Chapter 4

The Effect of Post-Custody Supervision on Recidivism and Other Outcomes

4.1 Introduction

A key aim of the criminal justice system is to reduce recidivism. That is, to prevent the offenders who have been brought to justice from offending again. A quick glance of the UK official statistics suggest that on average 25% to 27% of offenders would commit a re-offence within a year. Juvenile offenders typically have a higher rate than adult offenders, about 38% and 24% respectively. The numbers are higher for offenders released from custodial sentence, which currently stand at 43% for adults and 69% for juveniles. And they get worse if we focus on short prison sentences, ie ones that are less than a year. According to the National Audit Office (2010), the annual economic and social cost of reoffences committed by short term prisoners alone is estimated to be between £7 billion to £10 billion. It is therefore no surprise that rehabilitation and recidivism reduction is an important agenda for policymakers in the UK - one of the most important policy shifts took place in 2014 when the government decided that all short term prisoners would be subject to one year compulsory supervision after release.

So is post-custody supervision all good for reducing recidivism? The literature does not lend unanimous support. Theoretically, the impact of an extra period of supervision is multifold and ambiguous on the whole. First, under the specific deterrence hypothesis (Smith and Gartin 1989), offenders on licence are expected to commit fewer crimes due to their experience with the criminal justice system and knowing they are more likely to detected and punished for reoffences that they commit. Second, offenders on licence are usually restricted in movement during the time they meet their supervisors or undertake required activities. They may be further restricted if the supervision comes with a curfew order. Such incapacitation (Ehrlich 1981) may reduce the amount of crime being committed. Third, supervision often incorporates some degree of rehabilitation aimed at enhancing offenders' ability to re-integrate into society, such as improving their employability, substance abuse, mental health and accomodation. It should have a beneficial influence (Ehrlich 1981, also see the review of Levitt and Miles 2007). On the other hand there may be negative scarring effect of the extra supervision on labour market outcomes, which are linked to recividism under any static or dynamic models of criminal behaviour (Becker 1968, Ehrlich 1973, Imai and Krishna 2004, McCrary 2010, etc). Grogger (1995) finds that arrest has a negative short term impact on employment rate. This may be true also for an extra period of supervision. Finally, under the "school of criminal" hypothesis of Bayer et al. (2009), there may be negative peer effects operating among offenders on licence, leading to higher recidivism rate.

A selection of treatment effect studies that have attempted to estimate the effect of time spent within the criminal justice system on offenders' outcomes includes Marie (2009), Di Tella and Schargrodsky (2009), Kuziemko (2013) and Huttunen et. al (2014). Amongst them, there is no agreed direction of impact. This may be due to the coexisting opposite effects as mentioned above, but may also be due to "time spent" being slightly different objects and involving different activities in the researches.

In this chapter, I use the new UK micro-dataset described in chapter 3, which encompasses criminal, labour market and benefit histories of offenders, to estimate the effect of a 3-month period of post-custody supervision on recidivism, benefit claim and employment outcomes. I do so by applying Regression Discontinuity Design (RDD) to an age cut-off rule in the English law that prior to 2015 determined completely the allocation to treatment. Specifically, among adult offenders who are sentenced to a short custodial sentence, those aged under 21 are supervised by the Probation Service for 3 months upon release, while those aged 21 or above are not. I find that, contrary to the policy belief that underpinned the UK government's expansion of the provision of post-custody supervision to all short term prisoners from 2015, post-custody supervision has no detectable effect on recidivism, benefit claim and employment outcomes from the time of release up till 3 years afterwards. While RDD treatment effect estimates have an inherently local interpretation restricted only to persons near the cut-off of age 21, we know from the well documented age-crime profile (Hirschi & Gottfredson 1983) that this is also the age where criminal activities peak. There are important policy implications here as my results suggest that more of the same services under the new policy landscape may not be a cost-effective measure in preventing recidivism and facilitating offenders' social re-integration.

The rest of the chapter is organised as follows. I outline the policy situation and the empirical research design in section 2. I then describe the data in section 3. I discuss and interpret my results in section 4. Section 5 concludes the chapter.

4.2 Research Design

4.2.1 Policy situation

In England and Wales, adult prisoners are automatically released at around the halfway point of their custodial sentence. Prior to the commencement of the Offender Rehabilitation Act 2014, under the Criminal Justice Act 1991 and Power of Criminal Courts (Sentencing) Act 2000, if the custodial sentence length is less than 12 months and the offender is at least 18 but under 21 at the time of conviction, upon prison release he/she is to be supervised by the Probation Service (also known as "being on licence") for a fixed term of three months. While on licence, the offender has to comply with standard conditions such as keeping in touch with probation officers, undertaking rehabilitation programmes, doing supervised unpaid work, observing any curfew orders, and most importantly, not committing any offence. Recall to prison procedures are enforceable at court if the offender is proven to have commited crime during the supervision period. On the other hand, if the custodial length is less than 12 months but the offender is at least 21 at the time of conviction, he/she is released without further conditions[1]. This is the discontuity that I exploit in this research. To complete the policy picture, all prisoners sentenced to 12 months or longer, regardless of age, are supervised upon automatic release until the end of their sentence.

Licence exists to serve several purposes. Firstly, the under 21 offenders (and those sentenced to 12 months or longer) are generally considered to be more prone to falling back to crime. In monitoring their activities upon release, it is a public safety measure to reduce the risk that their recidivism may pose to society. Secondly, the licence may act as a short-term deterrent against recidivism, as committing a crime during the supervision period would lead to a return to prison with possible time addition for the new offence. Finally, some activities that offenders undertake while on licence are designed to facilitate reintegration into society and/or rehabilitation of substance misuse. They aim to reduce the long term recidivism of this group of high risk offenders in the process.

From the stated policy purposes, one may expect post-custody supervision to have a ceteris paribus positive impact on recidivism reduction in both short and long term, as well as positive effects on reintegration outcomes such as employment afterwards. If I treat benefit outcome as a measure of offenders voluntarily seeking state help, since ideally the policy should prepare them better for life after custody, I should see a negative effect on benefit claim. However, the scarring or "school of criminal" hypothesis (Grogger 1995; Bayer, Hjalmarsson, and Pozen 2009) would instead predict a criminogenic effect, if being supervised reduces employability or if offenders have negative peer effects on each other while being supervised in the same Probation office. It is under the belief that the beneficial effects of licence would outweigh the harms that a new licence policy applying to England and Wales would be introduced by the Ministry of Justice (MoJ) in late 2014 under a new agenda termed Transforming Rehabilitation. Under the new policy, all prisoners sentenced to less than 12 months in custody, regardless of age, would receive 12 months of supervision. This obviously represents a huge increase in resource

[1] Technically, there is another subtle difference - under 21 adult offenders are required by law to be detained in Young Offender Institutions (YOI), intended for offenders aged 15 - 20, rather than adult prisons. However, the regime of YOI for offenders aged 18 - 20 is in practice much the same as that of adult prisons. In some cases, under 21s are merely detained in designated YOI cells within an adult prison block. After discussion with officials and practitioners, I decide that it is appropriate to assume the effect of this difference is negligible.

requirement, as it means the treatment group will receive an additional 9 months of supervision, and the control group an additional 12 months. In section 4, I present my analysis and demonstrate to what extent the current policy of a 3-month supervision is effective, which shall shed some light on the likely impact under the new policy.

The rest of the research design discussion will only consider individuals who are relevant for the licence policy in question, ie adult (>18) prisoners sentenced to custody for less than 12 months. I also restrict the analysis to male offenders, as over 90% of the prison releases are attributed to male and criminological research (Steffensmeier and Allan 1996; Uggen and Kruttschnitt 1998; Painter and Farrington 2004) shows male and female criminal behaviours are different.

4.2.2 Sharp Regression Discontinuity Design

It is clear from the above policy description that a Sharp Regression Discontinuity Design (SRD) can be set up to study the effect of post-custody supervision on various outcomes. Letting L_i be a binary indicator of individual i being on licence or not (1 if positive, 0 otherwise), $Y_i(0)$ and $Y_i(1)$ be the pair of counterfactual outcomes for i under the two treatment status, Y_i be the observed outcome and X_i be the age at conviction (in years) for i, I can formally express the situation as follows:

$$Y_i = (1 - L_i) \cdot Y_i(0) + L_i \cdot Y_i(1) \tag{4.1}$$

$$L_i = \mathbf{1}[X_i < 21] \tag{4.2}$$

The outcomes that I consider as Y_i are 1-, 2- and 3-year binary rates of recidivism, benefit claim and employment. Further let Z_i denote a vector of covariates of i, such as demographic characteristics and criminal history. For an individual, the quadruple (Y_i, L_i, X_i, Z_i) are observed by the econometrician. While there are no "hard" administrative data to demonstrate the sharpness of the design as modelled in (2), such as record of reception into Probation Service upon custodial release, I take confidence from the UK legal system that the reality is that of a perfect SRD case and there is no voluntary opt-in or opt-out.

The conditions required for identifying the treatment effect in a SRD setting are well understood and discussed in greater detail in Hahn, Todd, and Van der Klaauw (2001), Lee (2008) and Imbens and Lemieux (2008). Formally, only one continuity assumption is required for identification, and in this policy context it reads:

Assumption 1. $\mathbb{E}[Y(0)|X=21]$ *and* $\mathbb{E}[Y(1)|X=21]$ *are continuous.*

It is straight forward to see under this assumption that

$$\mathbb{E}[Y(0)|X=21] = \lim_{X\downarrow 21} \mathbb{E}[Y|X=21] \tag{4.3}$$

and,

$$\mathbb{E}[Y(1)|X=21] = \lim_{X\uparrow 21} \mathbb{E}[Y|X=21]. \tag{4.4}$$

Let β denote the average treatment effect at $X=21$. Then, β can be identified as

$$\beta = \lim_{X\uparrow 21} \mathbb{E}[Y|X=21] - \lim_{X\downarrow 21} \mathbb{E}[Y|X=21]. \tag{4.5}$$

Assumption 1 is the minimal condition with which SRD will work. Sometimes one may wish to impose the stronger (but not necessary) assumption that the pair of expected counterfactuals is continuous anywhere along X, rather than just at the threshold value, to increase validity. I show in section 4 that assumption 1 is likely to be satisfied, and given the feature of the UK criminal justice system, I believe the stronger version of the continuity assumption is also likely to hold for $X > 18$.

Based on (5), there are different viable econometric specifications to estimate β from the data. The essence is captured in the following general model:

$$\begin{gathered} Y_i = \alpha + L_i\beta + K_n(D_i)'\gamma + L_i \cdot K_n(D_i)'\delta + u_i, \\ for\ 21 - d \leq X_i \leq 21 + d, \end{gathered} \tag{4.6}$$

where D_i is the absolute distance of i's age at conviction from the threshold of 21 years, $K_n(D_i)$ is a function denoting the sum of powers in D_i up to power n, and d is a length of time corresponding to the choice of bandwidth for sample selection on either side of the age threshold.

One would typically also include the vector of other control variables Z_i in the econometric model, to reduce the potential bias induced by including observations not so close to the threshold, as well as to improve the precision of treatment effect estimates if Z is correlated with Y (Imbens and Lemieux 2008). In a valid RD design, however, the inclusion of Z_i should not matter. This is because under assumption 1, if the counterfactual outcomes are continuous at the threshold, it indirectly implies that other covariates should also be continuous. Hence, the inclusion of Z_i should not make a significant difference to the estimation of β. I show that this is true in my analysis in section 4.

The choice of n is typically positively related to the choice of d. In my analysis, I choose $n = 1$ (ie nonparametric local linear regressions around the threshold) for d equalling 3 and 6 months, and $n = 2$ (quadratic polynomial) and 3 (cubic polynomial) for d equalling 1 year. I can take more confidence in the results if they are not sensitive to the choice of specification. I do not include in the analysis observations that have an absolute difference in age at conviction of more than 1 year away from the threshold (ie at most I only include 20 - 22 years old). This is to ensure I do not consider observations that are likely to be systemically different from the ones near the discontinuity.

One factor that may undermine the validity of using SRD here to identify average treatment effect (conditional at $X = 21$) is the potential manipulation of allocation to treatment, leading to dissimilar offenders located across the age threshold. This can happen if some offenders give weight to the displeasure of an extra 3 months of supervision when they consider the consequence of their actions, and subsequently decide to delay offending until after turning 21. However, this is unlikely to be part of their decision mechanism given the existing evidence on young offenders being myopic (Lee and McCrary 2005). Allocation may also be unnaturally manipulated by judges during sentencing if they respond in a way that will result in just over 21-year-olds being more likely to be sentenced to custody for 12 months or more and hence receive post-custody supervision that a shorter sentence would otherwise not entail. I show strong evidence from my robustness checks in section 4 that there is no suggestion at all of manipulation across the threshold by both offenders and judges.

4.2.3 Duration Analysis in a Regression Discontinuity spirit

The structure of my data allows me to formulate the outcomes as duration variables, such as time elapsed until the individual commits his first reoffence. I can apply tools from the Duration Analysis literature to gain important insights about the dynamics of outcomes for different groups, such as the degree of duration dependence, in addition to the dynamic effect of licence that I am primarily interested in. Wooldridge (2002) provides a good textbook summary of the topic. The literature is slightly out of fashion but there are numerous early papers on for example recidivism (Witte and Schmidt 1979; Schmidt and Witte 1989; Chung, Schmidt, and Witte 1991) and unemployment (Lancaster 1979; Lancaster and Nickell 1980). In this paper, I combine Duration Analysis with SRD to mitigate the risk of unobserved heterogeneity to identifying the effect of licence.

Let T be the duration (say, in days) of an offender remaining in an initial state after release from prison. The initial states that I consider in this analysis, and an offender typically finds himself in at first, are being a non-reoffender, non-benefit-claimant and non-worker. The first object of interest is the "dropout function", $F(t)$, which is the cumulative distribution function of T and captures the probability of having dropped out of the initial state by time t:

$$F(t) \equiv \Pr(T \leq t). \tag{4.7}$$

This is the complement of the more popular "survival function". I do not consider re-entry into the initial states, so an individual who was employed for some time but sacked afterwards at time s and remained unemployed in the future would still be modelled as having dropped out of the initial state of unemployment by time s.

The second object of interest is the "hazard rate", $\lambda(t)$, which captures the instantaneous likelihood of leaving the initial state at time t, conditional on having not exited already:

$$\lambda(t) = \lim_{h \to 0} \frac{\Pr(t \leq T \leq t + h | T \geq t)}{h} \tag{4.8}$$

Let $f(t)$ be the first derivative of $F(t)$. It can easily be shown that

$$\lambda(t) = \frac{f(t)}{1 - F(t)} \tag{4.9}$$

Rearranging the above, we can link $F(t)$ and $\lambda(t)$ directly by the following expression:

$$F(t) = 1 - \exp\left[-\int_0^t \lambda(s)ds\right]. \tag{4.10}$$

I hypothesise that being on licence has an effect on the hazard rate, and so the dropout function. I assume the specification of piecewise exponential hazard function (see Wooldridge (2002) for more detail), which is a very flexible specification within the class of proportional hazard model that does not, for example, a priori assume any shape about state dependence ($\partial\lambda(t)/\partial t$) as the popular Weibull hazard function does. This specification allows me to estimate the effect of licence at as many different points in time after release as I like. I choose to use months as the unit of time in my analysis. Formally, for number of months $m = 1, \ldots, M$ since release, I specify the hazard function as:

$$\lambda(t; D, L, \theta) \;=\; \exp\left[L\beta_m + K_n(D)'\gamma + L \cdot K_n(D)'\delta\right]\lambda_m, \quad a_{m-1} \;\leq\; t \;\leq\; a_m \tag{4.11}$$

or after taking natural logs,

$$\ln\lambda(t; D, L, \theta) \;=\; L\beta_m + K_n(D)'\gamma + L \cdot K_n(D)'\delta + \ln\lambda_m, \;\; a_{m-1} \;\leq\; t \;\leq\; a_m \tag{4.12}$$

where X, L, K_n, D are as previously defined, λ_m is the constant baseline hazard rate specific to m, and a_m is the number of days corresponding to m months. In this specification, the baseline hazards are restricted to be constant for t within the same time segment specific to m, but allowed to differ between different segments. For the choice of M, I restrict it to be 36 in the analysis since 3 years after release is usually a long enough period to study interesting differences in behaviour.

The set of β_m is the treatment effect. It is assumed that licence has a constant scale effect of $exp(\beta_m)$ on $\lambda(t)$. β_m are m-specific and capture the potentially time-variant effect of supervision on reoffending, benefit claim and employment hazards. For example, one may hypothesise that under specific deterrence, the impact may be greater during the first 3 months when the offenders are on licence. Alternatively, if the supervision is effective in making fundamental changes to how offenders view costs and benefits of their actions in the future and hence creating a positive spiral, we may see the effect of licence to increase over time. This specification can reveal any interesting dynamic effects of treatment that may be useful for future modelling work.

$\theta \equiv (\beta_m, \gamma, \delta, \lambda_m)$ is the vector of parameters to be estimated. This can be accomplished using Maximum Likelihood, given the piecewise exponential hazard function that I specify. The log likelihood function for observation i can be written as:

$$\sum_{h=1}^{m_i-1} \ln\left[\alpha_h(\theta; L_i, D_i)\right] + d_i \ln\left[1 - \alpha_{m_i}(\theta; L_i, D_i)\right] \tag{4.13}$$

where m_i is the month since release by which i has left the initial state, d_i is a dummy variable indicating if i's duration is uncensored (so equalling one only when i has not left the initial state by the end of the measurement period of 36 months) and $\alpha_m(\theta; L_i, D_i)$ is a function capturing the likelihood of survival during m and $m-1$ months:

$$\alpha_m(\theta; L_i, D_i) \equiv \exp\left\{-\exp\left[L_i\beta_m + K_n(D_i)'\gamma + L_i \cdot K_n(D_i)'\delta\right]\lambda_m(a_{m_i} - a_{m_i-1})\right\} \tag{4.14}$$

General discussion of the derivation of the likelihood function under piecewise constant exponential hazard specification can be found in Wooldridge (2002). The log likelihood for the entire sample is obtained by summing expression (14) across all $i = 1, ..., I$.

Similar to the situation where a simple OLS regression would suffer from bias in treatment effect estimation due to unobserved heterogeneity, a simple duration analysis would suffer similar consequences. To overcome this, I propose implementing my MLE procedure in a RD spirit. My proposal is to restrict the sample to a subset with age at conviction of about 21, and choose an appropriate $K_n(D_i)$ correspondingly. Consistent with the choices I make in RD analysis, I choose to couple a 1-year bandwidth on either side of the threshold with $n = 2$ or 3, and couple a 6-month and 3-month bandwidth with $n = 1$. Thinking of each $\lambda(t)$ as an outcome of interest in a RD framework, the assumption required here for identification is a familiar one of continuity:

Assumption 2. The counterfactuals of λ for any t are continuous at the age at conviction threshold of 21.

Before presenting the results, I turn my attention first to describing the data in the next section, and discuss some data limitation issues and my proposed solutions.

4.3 Data

This paper draws on the new linked data created by the Ministry of Justice (MoJ) in partnership with Department for Work and Pensions (DWP) and Her Majesty's Revenue and Customs. My analysis dataset is made up of three component datasets: the Police National Computer (PNC), National Benefits Database (NBD) and P45 Employment. General descriptions about them are provided in chapter 3. In this section I discuss the sample for this analysis and specific data issues that are relevant. As mentioned in the previous chapter, while the process of matching carried out by MoJ/DWP is demonstratably robust, some analytical issues are impossible to avoid when matching across multiple agencies and departments, all with different data systems and recording practices.

4.3.1 Sample

I include in the analysis sample offenders in England and Wales who were released from prison between 2002 and 2008. Whilst the earliest matched PNC data is available from 2000, I have chosen to begin the analysis at 2002 because there are quality concerns with the 2001 data (it is often omitted from official reoffending analysis). The latter cutoff of 2008 is chosen because at the time of undertaking this analysis, 2008 is the latest cohort for which I can observe the 3-year reoffending rate. Applying the sample selection criteria set up in sectoin 2.1, there are in total 263,146 prison releases of 157,100 unique offenders identified during the analytical timeframe. An offender is recorded to have reoffended within one year, two years and three years if he has committed at least one offence during this period that is subsequently proven through caution and court conviction. This PNC sample is then linked to its counterpart NBD and P45 Employment records for analysis of benefit and employment outcomes.

4.3.2 Analytical Data Issues

The MoJ/DWP data matching process is highly rigorous (MoJ, 2014a) and has overcome major practical matching issues in creating the database. However, there still remain analytical issues stemming from imperfect matching between datasets and measurement error that must be considered carefully when using the data for analysis.

PNC individuals missing on NBD/P45

There are two reasons why individuals appearing in PNC may not be found on NBD and/or P45 Employment. The first one is that they have never claimed benefits and/or never been employed within the time period that the extracts of NBD and P45 data cover, both of which going back to before 2000 and lasting until early 2013. This is a natural cause and the not-found reflects the behaviour that I am interested in. The second reason is recording error, where basic identifying information about the same individuals (such as name or date of birth) has been recorded differently on different datasets, leading to a match not being established. The two types of not-found are of different nature and in analysis would require different handling. Unfortunately, the matching process cannot distinguish between the two. If I simply code all unfound PNC/Reoffending Cohort individuals as never claimed benefit and never been employed, set up the outcome variables accordingly and run the analysis on the entire sample, the estimated treatment effects are going to be biased. This is because for the fraction of benefit claimants and workers suffering from the recording error, I will be coding them wrongly as non-claimants and non-workers. On the other hand, if I drop all unfound individuals and use only those who can be found in the analysis, this can potentially lead to a non-representative sample if the found and not-found groups display different characteristics. The resulting treatment effects, while still can be internally valid if the RD assumption is upheld, are only specific to a strangely defined subgroup of the population, restricting the already limited external validity of the RD estimates.

One simple solution is to assume away the effect of the differences in the found and not-found group on outcomes (by far the major one is the proportion of UK nationals, 94% vs 79% for NBD, 93% vs 86% for P45), and simply use the found sample without further adjustment. A more sophisticated approach is to generate separate propensity scores of being found in NBD and P45, and apply the scores as inverse weights to the observations when analysing the Reoffending/NBD matched sample and the Reoffending/P45 matched sample. The intuition here is to re-create within the found samples the representation in the full Reoffending sample, by weighing up observations with characteristics more associated with not being found and weighing down others. This can overcome the issue of misrepresentation when using the found samples, but at the cost of less precision in estimation. I later present results of both approaches. The use of weight appears not to affect the estimates, suggesting representativeness of the found samples is not much an issue, at least for the subgroup of offenders that I consider in the analysis.

Random Start/End Dates in P45

It appears to be a common problem in the P45 data that the start and/or end dates are not properly filled out. To complete the dataset anyway, the DWP's approach is to flag the problematic employment spells and randomly assign dates within the tax years (known to be accurate) that the records belong. MoJ and DWP further adjust these random dates so that benefit spells do not overlap with prison spells. Out of the 105,172 unique offenders from my analysis sample who can be matched to P45, 60% of them have had at least one randomised start dates during 1998 - 2013. Of those who had had at least one randomised start date, the average times of randomisation is 2.6. Obviously, inaccuracy in recording the start and end dates affects the validity of the employment outcome and history variables that I create for the analysis.

Upon closer inspection, there are three reasons to believe that the impact of this is not detrimental to the analysis. Firstly, the randomisation is not completely wild and is still accurate to the tax year. It may not make a difference at all to the RD employment outcome variables of ever employed within 1, 2 and 3 years, and even if it does, the difference that can be made is limited as time goes on. For example, if the randomised start date is within one year after custody but the true date is outside, I still know for sure that the true date must be within two years after custody, as a consequence of the tax year restriction. In such a case, the two-year outcome must be correct even if the one-year one was wrong. Secondly, I am not interested in all the P45 records. I formulate my outcome variables as whether the individual was ever employed in the 1, 2 and 3 years after custody, and the history variable as whether he was ever employed in the 1 year prior custody, so I need to only consider the P45 records immediately before and after a prison spell. In my sample, of the 171,102 prison spells that can be associated with a P45 record before or after, just under 10% have a subsequent P45 spell with a randomised start date, and less than 7% have a preceding P45 spell with a randomised end date. The extent of the problem is limited as I focus on the relevant records for analysis. Thirdly, I show in the results section that keeping or dropping the affected records do not make a difference to the estimates in both RD and duration analyses, suggesting the randomised records do not bias results and the allocation to randomisation is random.

4.4 Results

Between 2002 and 2008 in England and Wales, in the data I can identify a total of 263,146 releases of 157,100 unique adult male offenders sentenced to less than 12 months in custody. Roughly 15% (39,003) of the releases were for offenders of age at conviction less than 21, the threshold at which the likelihood of receiving post-custody supervision jumps sharply from one to zero. The descriptive statistics can be found in table 1. UK nationals make up over 90% of the observations, and white 84%. For nearly 80% of the releases, the offenders can be found on the NBD. The found rate for P45 is lower at 64%.

4.4.1 Regression Discontinuity Analysis

Reoffending outcomes

Figure 1 shows simple plots of smoothed local linear polynomial fit (over a 1 month window) of 1-year, 2-year and 3-year reoffending rates against age at conviction, with a break at the red line denoting the licence eligibility threshold of age 21. Any jump at the threshold can loosely be interpreted as the local treatment effect. Supervision appears to have a negative effect on the 1-year reoffending rate but positive effects on the 2- and

3-year rates. However, the jumps are all of minor magnitudes (less than 2% point in absolute) and none appears to be statistically significant. We can see that in general over 60% of these young offenders reoffend within one year, and this figure rises to over 80% by 3 years after release.

Table 2 shows more accurate regression results. It confirms the simple findings from inspecting the figure. The first two columns report estimated treatment effects on 1-year, 2-year and 3-year reoffending outcomes from a specification of equation (6), with a linear functional form and a bandwidth of 3 months on either side of the age threshold, with and without control variables. The next two columns report results from a similar specification but with a 6-month bandwidth. The last two pairs of columns report results from a quadratic and a cubic specification respectively over a 1-year bandwidth. The estimates are all close to zero and not statistically significant at even the 10% level. The fact that they are similar with or without controls and regardless of the functional form is a strong indication that the RD assumption is credible.

Benefit outcomes

Figure 2 shows plots of 1-, 2- and 3-year benefit claim rates against age at conviction. In general, over 90% of the this group of prisoners have claimed benefit and actively sought financial help from the state by 3 years after release, and licence appears to have a weak negative impact. Table 3 presents the results of RD benefit analysis using the NBD matched sample, a subset of the sample from the reoffending analysis. Though I do not report estimates from specifications without control covariates (available on request), they are very similar to the specifications with controls. This is indicative that the RD continuity assumption is upheld despite using a subsample. The first two columns in table 3 report the estimates from the same specification, specifically the one with linear functional form fitted over a bandwidth of 3 months using the NBD matched sample, but first one is without propensity score weighting and the second one with. The next 3 pairs of columns are for estimates from specifications with 6 months bandwidth/linear, 1 year/quadratic and 1 year/cubic. The estimates from all specifications are largely the same - they all suggest a weak negative effect of licence on benefit claim of about 1-3% point, but none of them are significant at the 5% level. There is only one set of significant estimates at the 10% level, coming from the specification with a bandwidth of 1 year and cubic functional form in age.

I notice from the table that the application of propensity score inverse weighting appears to have almost no effect, suggesting the matched sample is representative of the overall sample and does not require sample rebalancing.

Employment outcomes

Figure 3 shows plots of 1-, 2- and 3-year employment rates against age at conviction. There is almost no visible discontinuity and treatment effect at the threshold. Over 50% of the offenders in the data had been in P45 employmen for at least once by 3 years after release. Table 4 presents the results of RD employment analysis using the P45 matched sample, excluding the observations associated with a subsequent randomised P45 start date. The table structure follows that under benefit analysis. Again, I do not report estimates from specifications without control variables, but they are available on request and are highly similar to the specifications with controls. We notice as before that the propensity score weighting does not make a significant difference, and no estimates are significant at the 10% level, confirming the findings from graphical inspection.

I present table 5 to demonstrate that the issue of random dates is not of significance to the analysis. The table shows estimates from the same specifications as in table 4, but using the entire P45 sample without discarding the randomised P45 spells. We can see the estimates are very similar to those in table 4, confirming that the issue of randomised dates is of minor importance.

4.4.2 Duration Analysis

I use tools from the Duration Analysis literature to dig deeper into the dynamics of the effect of supervision on outcomes, as well as the outcomes themselves.

Reoffending outcome

Each graph in figure 4 shows two plots of the hazard $\lambda(t)$ of dropping into recidivism, for the control group (green) and treatment group (red). The difference between them is plotted as the dashed line, and can be interpreted as the time-variant effect of supervision. The hazard functions are generated by applying MLE procedure to the sample likelihood function, which is the sum of equation (13) across i. Each graph in the panel corresponds to a choice in the combinations of sample selection bandwidth around threshold and functional form of the sentence length variable during estimation. Figure 5 shows the corresponding dropout functions. Table 6 presents MLE estimates of the treatment effect, $\hat{\beta_m}$. Each column again corresponds to a specification with a particular combination of bandwidth and functional form. They are all estimated without control covariates Z, as RD analysis already demonstrates that the inclusion of Z does not matter. Also, the size of this vector (about 30 variables) means that inclusion of it would considerably increase the computational demand, for little value in return.

The first thing we notice from the table and the graphs is that there are no significant patterns of effects of supervision on the hazard rates (and therefore the dropout functions) anywhere along the time horizon. This is perhaps surprising, as one may have expected to see a difference in recidivism hazard not least at the very beginning due to specific deterrence and incapacitation. Even though offenders on licence are restricted in movement and exposed to the certainty of recall to custody if they are caught commiting a reoffence, they do not behave differently to offenders who are not exposed to supervision. We also notice from the graph that the hazard functions demonstrate negative duration dependence, ie the longer an offender stays free of crime, the less likely he is to fall back to crime. This is consistent with previous recidivism research using duration analysis techniques, such as Chung et al. (1991).

Benefit outcome

Figures 6 and 7 show plots of the benefit hazard and dropout functions. The definition of the initial state here is not having claimed benefit once after prison release, and so the interpretation of the benefit hazard is, perhaps unnaturally, the probability of claiming benefit at time t, having not claimed benefit since release, and similarly the dropout function refers to the probability of having claimed benefit at least once by time t.

The results are not sensitive to the choice of specification. We can see that the hazard for both groups take a very similar and striking shape. The hazard is particularly high in the first month after custody, then drops sharply after 30 days and remains flat from there onwards. This reveals an interesting behavioural response to being punished in custody, that benefit is mostly claimed right away, but if it was not then the likelihood of claiming benefit later is much lower. While this is not relevant for the discussion on the marginal impact of supervision, it is a useful stylised fact for further behavioural modelling work on individual's interaction with the criminal justice system. Back to the effect of supervision, we see that it has no impact on the shape or scale of the hazard function, although a small insigificant decrease about 1% does show up in the dropout functions over time. This is consistent with the findings in benefit RD analysis.

Employment outcome

Figures 8, 9 and table 8 show the corresponding graphs and treatment effect estimates for employment outcome. For both treatment and control groups, the employment hazard, defined as the probability of being employed at a particular time having not been employed once since prison release, exhibits a trend of negative duration dependence. This is consistent with a hypothesis of reduced employability the longer individuals stay out of work. On the difference between the treatment and control groups, there are more significant treatment effect estimates, but overall they are not economically significant and there are no particular emerging patterns. The close to null effect could be due to rehabilitative effect and scarring effect offsetting each other, or that being on licence has genuinely little effect.

4.4.3 Robustness checks

The validity of results in RD and Duration Analysis is dependent on assumptions 1 and 2 being satisfied, but as they involve counterfactuals they are not directly testable. They may be called into question if allocation to supervision can be unnaturally manipulated by the offenders or the judges. I present five robustness checks here to show that there is no hint of any manipulation and my main analysis results are highly robust.

Covariates balance

The continuity assumption refers to the idea that if everyone was treated the same away, there would not be a jump in the outcome across the threshold. This would require the observables (at least those that would correlate with the outcome anyway) and unobservables to be continuous at the threshold. If the treatment and control groups near the threshold are similar over a large set of observable characteristics bar the licence status, it is strongly indicative that the unobservables are likely to be balanced and that the continuity assumption is plausible. We have seen from the RD analysis results that the treatment effect estimates are not sensitive to the inclusion of the set of control covariates.

This is a good sign that the covariates are balanced. To confirm this, I run regressions based on equation (6), but replacing the outcome with control variables. The idea is to detect whether there is a discontinuous jump in the variable across the threshold. We can see from table 9 that across the 31 control variables encompassing nationality, ethnicity, index offence type, criminal histories, benefit histories and employment histories, only 2 of them are statistically significant at the 5% level. Specifically, it appears that the just under 21 group has a higher proportion of violent offenders by 3% point, and fewer drink driving offenders by 1.5% point. However, these are not economically significant differences. Also, as for a fact there is no difference in sentencing guidelines across the age threshold for these offences, I take the view that the differences are anomalies. Overall, the comparability between treatment and control groups is very strong.

Density of forcing variable

The continuity assumption is more plausible if agents cannot manipulate allocation to treatment. This is the intuition behind the McCrary (2008) test – density of the forcing variable (age at conviction) should be continuous across the threshold if agents cannot manipulate which side of the threshold to be on. In my case, if there is manipulation, I expect it to be in the form that just under 21 offenders would wait until they turn 21 to commit the same crime in order to avoid the additional 3 months of supervision. Previous research by Lee an McCrary (2005), which shows young offenders are myopic and do not respond to increased sentence length, suggests this is unlikely to be true, but if it is, I should see a dip in density as as age approaches 21 from underneath, and a discontinuous upward jump in density at 21. Visual inspection of figure 10, which shows a finely gridded histogram of age at conviction for all male offenders sentenced to less than 12 months in custody and released between 2002 and 2008, suggests that there is no discontinuity at 21. In fact, this figure is highly reminiscent of the well-documented age-crime profile from criminological studies such as Hirschi & Gottfredson (1983), with a peak of criminal activities at around age 20.

Figure 11 shows the result of applying the McCrary continuous density test. The estimated jump in density at the threshold is statistically significant, but the test has picked up a downward jump in density that contradicts the intuition outlined above. I conjecture that this is due to the test not performing well at a cutoff near the peak of density where there is a sharp change in gradient from positive to negative. As a counter check, I apply the McCrary test to a placebo cutoff at age 20 instead of 21 in figure 12, and it again returns a significant discontinuous downward jump. There is no reason to suspect unnatural manipulation into crime around the age of 20. It adds weight to the conjecture that McCrary test may not perform well near a sharp turning point. Based on the results of visual inspection and the placebo test, I conclude there is nothing unnatural in the density of the forcing variable at the allocation threshold, and there is no hint of manipulation on the offenders' part.

Continuity of the ratio of long vs. short custody

Judges may react to the licence age cut-off during sentencing in a way that invalidate the continuity assumption. As mentioned under policy situation in section 2, all adult offenders sentenced to 12 months or more in custody are supervised upon release, regardless of age. It is hence a conceivable situation that when deciding on the sentence for an offender just over 21, the judge may be inclined to giving out a 12 months or more custodial sentence over a under 12 months one so that the offender would still receive post-custody support, something that he would have had under a short custodial sentence had he arrived at the court just before he turned 21. Such counter-balancing act against the age cutoff on the judges' behalf would undermine the continuity assumption, as the 21 or above offenders sentenced to less than 12 months in custody would belong to a group that is deemed to have lower recidivism risks and lower needs for supervision. In my design, this would underestimate any positive effect of supervision on recidivism reduction.

Figure 13 shows a smoothed line denoting the ratio of 12 - 14 months custody to 9 - 11 months custody sentence by age at conviction. It is clear that there is not a discontinuous jump in the ratio at the age threshold, suggesting judges do not counter-balance the age cutoff by sentencing more 21 years old to 12 months or more in custody in order to provide them with supervision.

Sensitivity to sentence length

On the same line of thought as the previous robustness check, if judges counter-balance the age cutoff by reserving the just under 12 months custodial sentences for offenders 21 or above that have lower perceived needs for supervision, then my analysis results may be sensitive to the inclusion of prison spells with custodial length between 9-11 months. From table 10, we can see that excluding those prison spells does not make an impact on the estimates. This confirms that the comparison between under and over 21-year-olds is not sensitive to being near the 12 months sentence length threshold, over which all adults offenders would receive post-custody supervision.

Sensitivity to criminal history

The last check I present is an external validity robustness check, unlike the previous ones. It is reasonable to hypothesize that offenders with different criminal histories may react differently to supervision. For example, new offenders may react most positively to the specific deterrence element of the supervision. I show that in table 11 there is no emerging pattern as I restrict the analysis to offenders with 0, 1 and >1 previous convictions. If anything, supervision appears to have a weak but not statistically significant (at 5% level) criminogenic effect on offenders with multiple previous convictions. I conclude that my finding of null supervision effect on recidivism, benefit claim and employment in not sensitive to types of offenders (in terms of criminal histories) being considered and is a general result applicable to the all types.

Overall, results of the robustness checks all point towards a very strong and highly credible RD design.

4.5 Conclusion

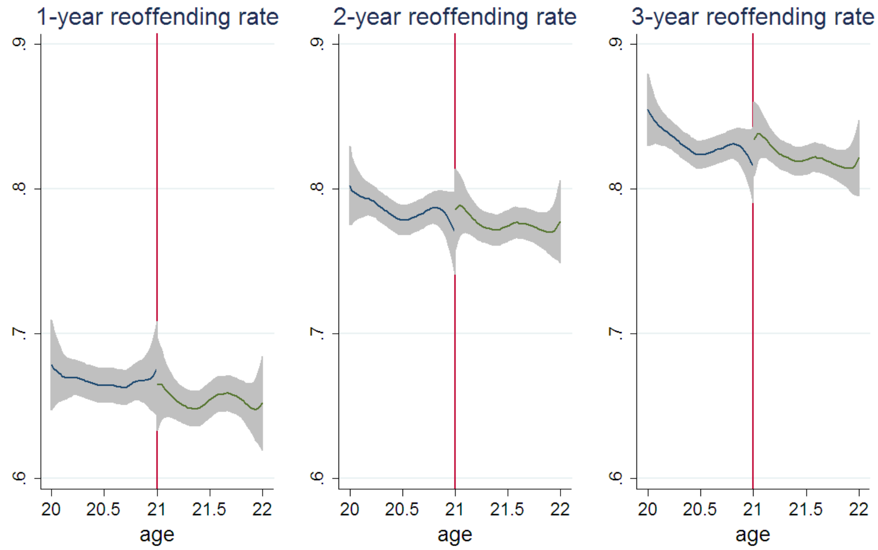
In this chapter, I use Regression Discontinuity Design to exploit a previous policy feature in the England and Wales criminal justice system in identifying the treatment effect of a 3-month post-custody supervision period on later outcomes. According to the law before 2015, within the group of adult offenders sentenced to less than 12 months in custody, the allocation to post-custody supervision is completely determined by age: only those under 21 at the time of conviction are on licence upon release. Using a new micro-dataset produced under the joint effort of MoJ and DWP covering information on criminal, benefit and employment histories, I find that the 3-month supervision period, during which offenders have to observe licence conditions and undergo various programmes aimed at reducing reoffending and enhancing re-integration into society, has no impact on 1-, 2- and 3-year recidivism, benefit claim and employment outcomes. Results from

Duration Analysis applied in a RD spirit further reveal that not even very short term impact can be detected, such as any specific deterrence or incapacitation effect during the 3 months on licence. The robustness checks all return satisfactory results and point towards a very strong RD design, endowing my estimates with very high validity.

This paper adds evidence to the growing empirical literature on the effect of criminal justice punishment on outcomes. The null effects estimated here are somewhat consistent with the ambiguous theoretical prediction on the direction of impact, as well as the mixed results from previous similar research on time spent within the criminal justice system. The majority of existing research only provides treatment effects on recidivism and does not shed light on the mechanism behind the observed pattern. I am able to incorporate into the analysis labour market and benefit (not least as a proxy for offenders' self assessment of whether state financial help is required) outcomes and provide a fuller picture of the effect of being on licence.

Inherent in any RD studies, the estimated treatment effects are only valid for the sample of individuals near the threshold. In my case, they are male offenders sentenced to under 1 year in custody and aged around 21 at the time of conviction. The results cannot be generalized with confidence to, for example, 18- and 30 years-olds, who are located on very different parts in the age-crime profile. Despite this limitation, the results should still be highly relevant and useful for policymaker. This is because, as a matter of fact, on average offenders of about 21 years old are near the peak of activities during their criminal careers. For all the good intention of and the resource spent on the current licence policy, it is perhaps surprising from a UK policymaker's point of view that it is not an effective means of reducing reoffending and facilitating social re-integration. This is also an alarming result within the context of the new MoJ Transforming Rehabilitation agenda, which came into effect in late 2014. Under the new licence policy, all adult offenders sentenced to less than 12 months in custody will receive post-custody supervision lasting 1 year. While it remains to be seen whether a 12-month supervision period under the new policy is going to be more effective than a 3-month period, found in this paper to be ineffective at least for young offenders, results here should provide an opportunity for policymakers to consider and explore what the other more effective and efficient measures may be in achieving the aims of safeguarding the public, reducing reoffending and enhacing social re-integration.

Figure 4.1: Reoffending outcomes of the analysis sample



Shaded region denotes 95% confidence interval

Figure 4.2: Benefit outcomes of the analysis sample

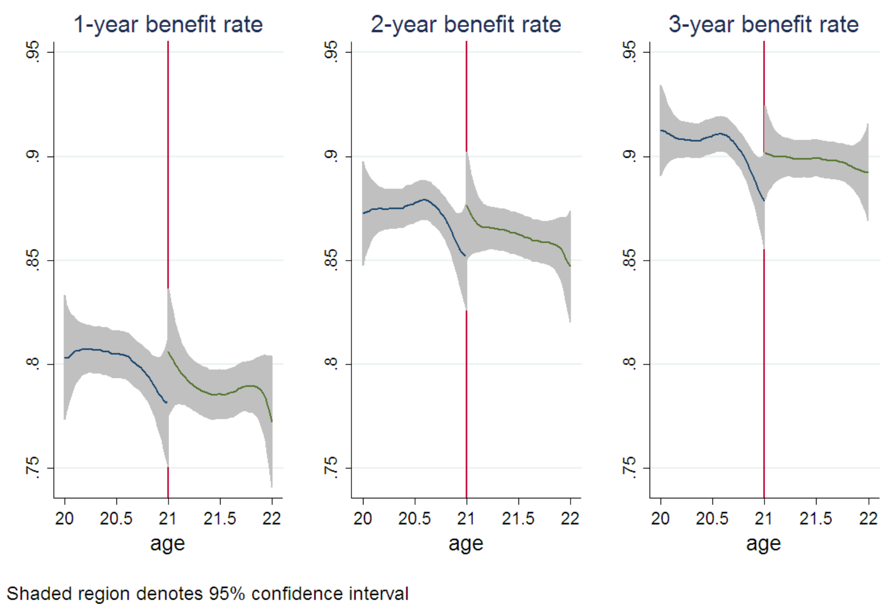


Figure 4.3: Employment outcomes of the analysis sample

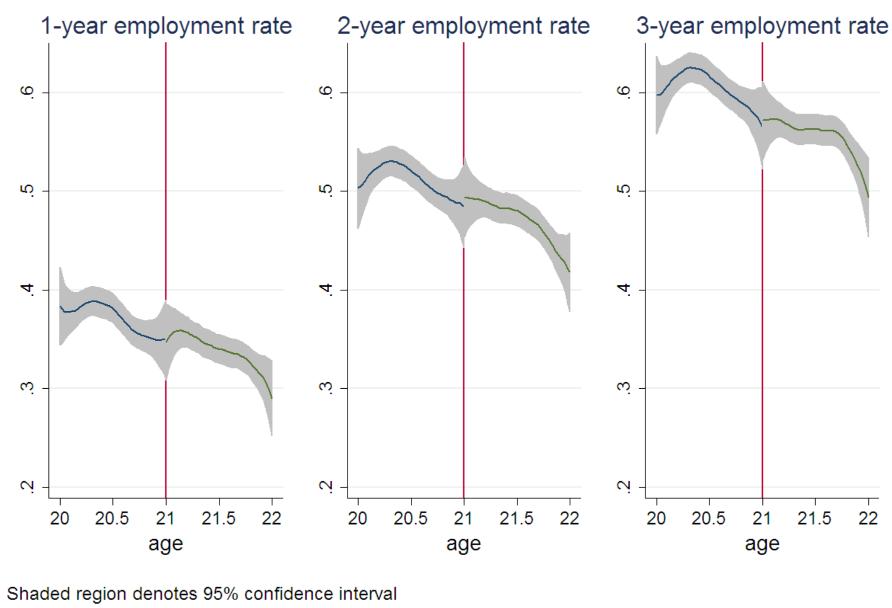


Figure 4.4: Reoffending hazard functions

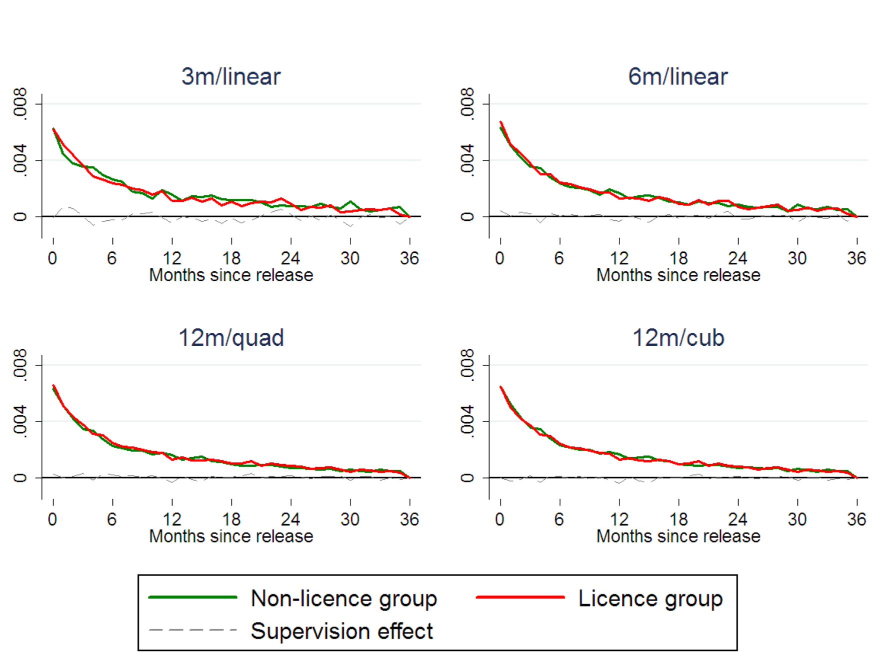


Figure 4.5: Reoffending dropout functions

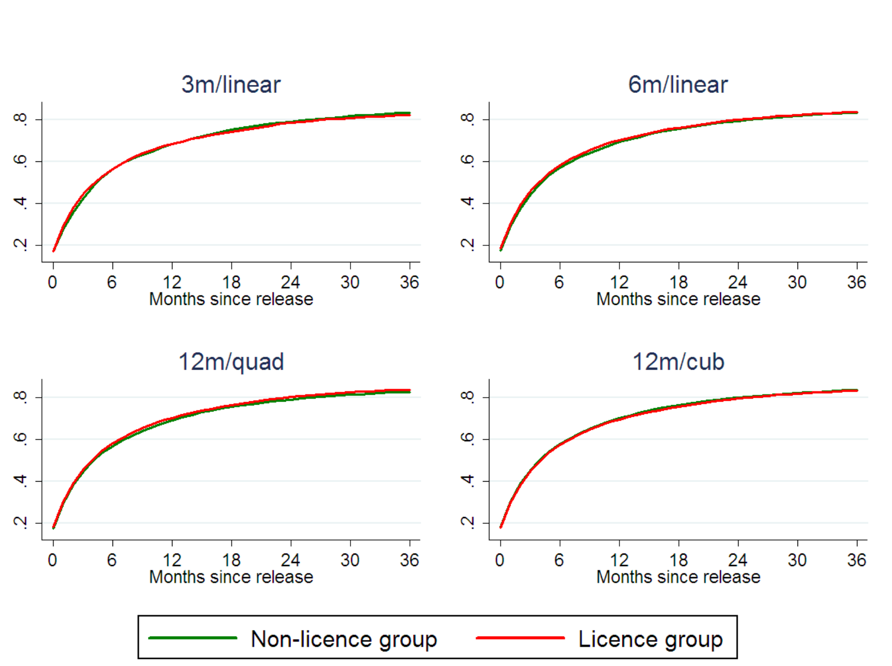


Figure 4.6: Benefit hazard functions

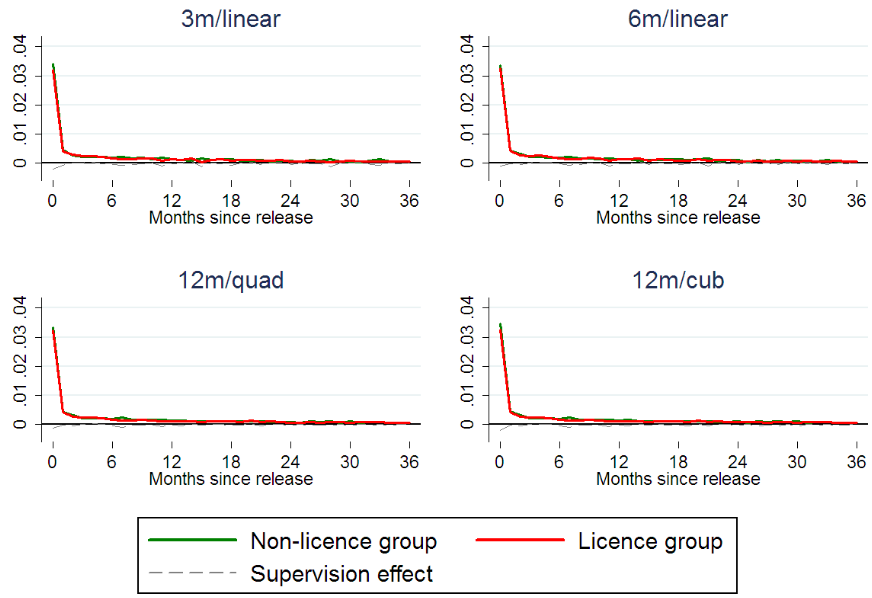


Figure 4.7: Benefit dropout functions

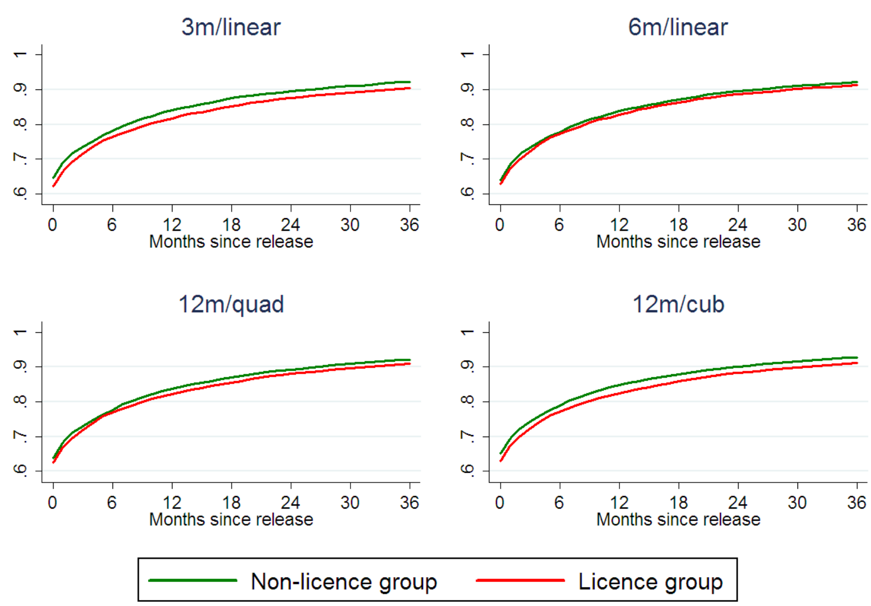


Figure 4.8: Employment hazard functions

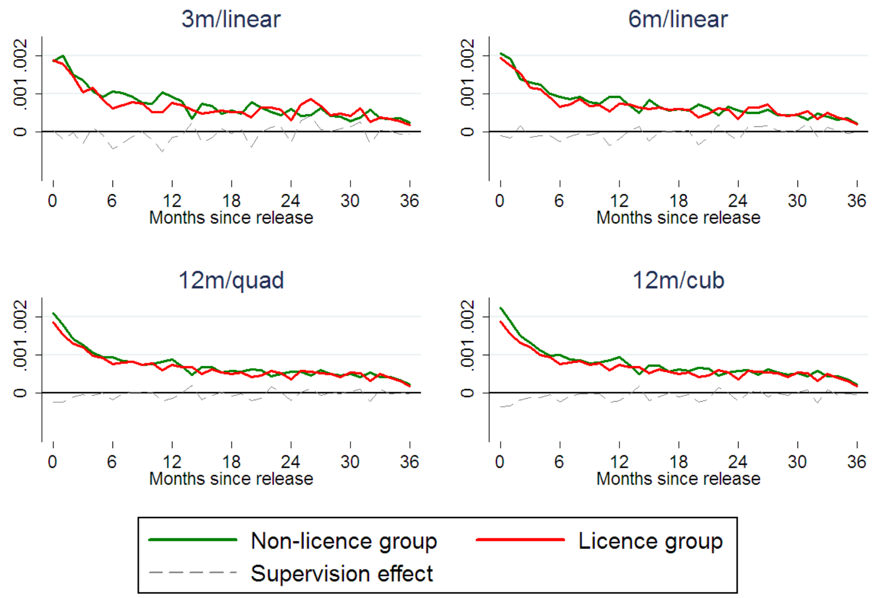


Figure 4.9: Employment dropout functions

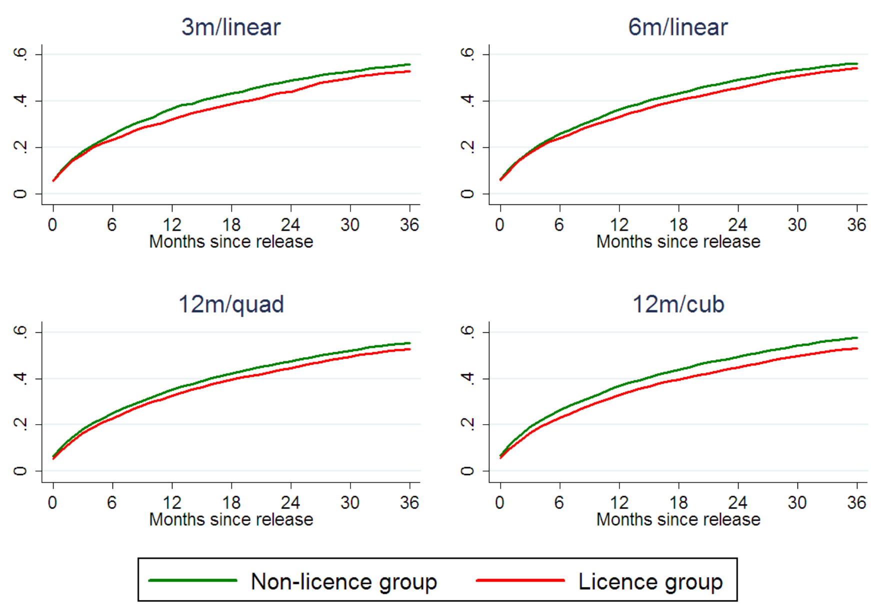


Figure 4.10: Distribution of age at conviction

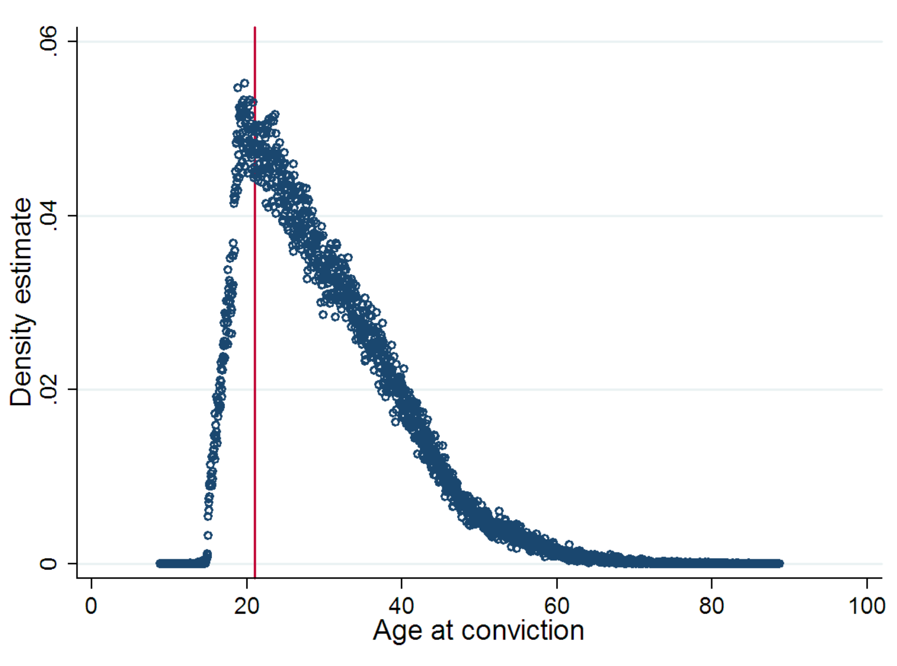
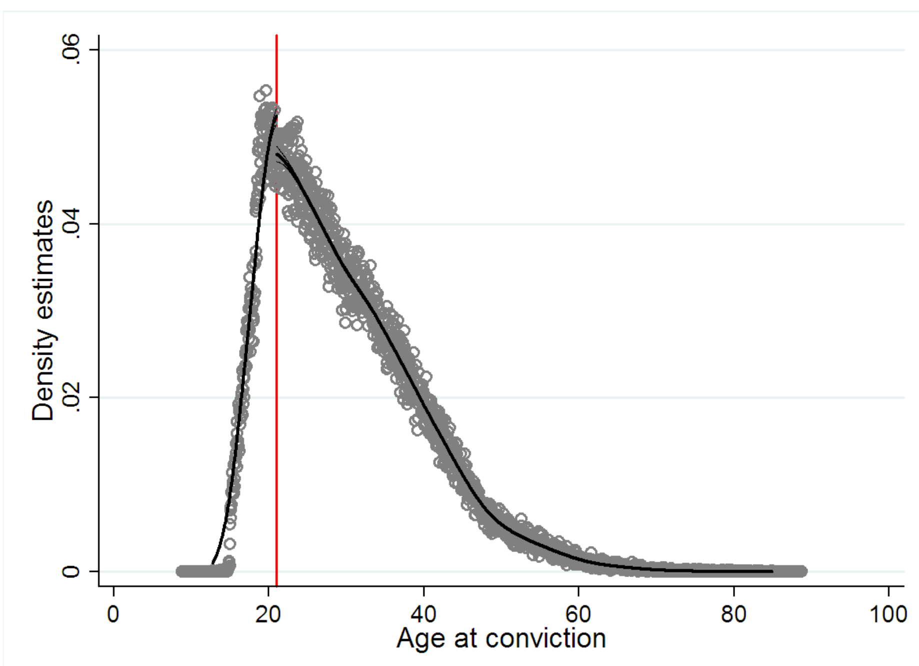
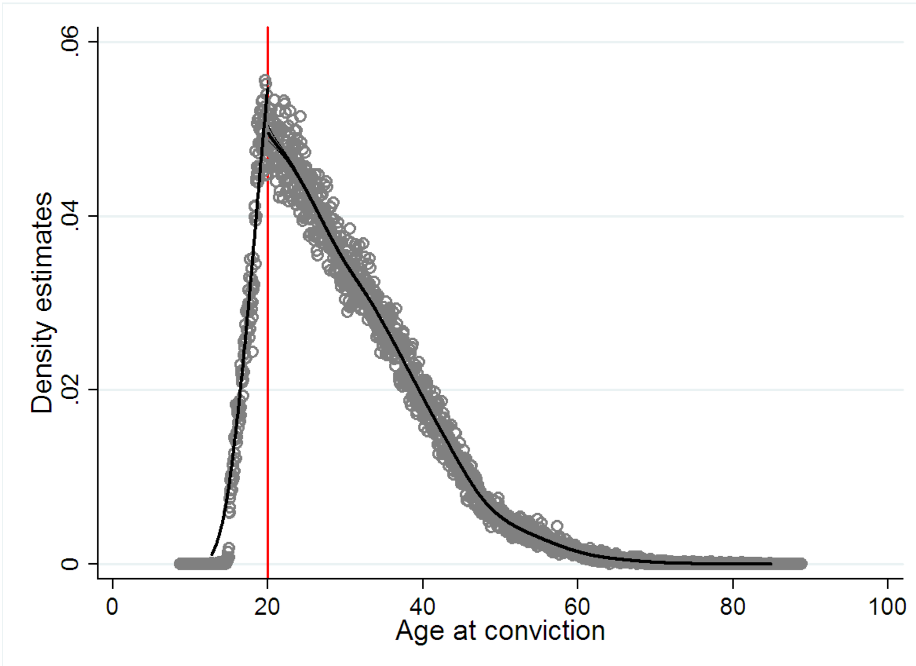


Figure 4.11: McCrary density test for a cut-off at age = 21



Notes: The discontinuity estimate (log difference in height) is -0.087, with standard error 0.013.

Figure 4.12: McCrary density test for a cut-off at age = 20



Notes: The discontinuity estimate (log difference in height) is -0.102, with standard error 0.013.

Figure 4.13: Ratio of 12-14 months custody vs. 9-11 months custody

Table 4.1: Descriptive statistics of the sample of male adult offenders sentenced to less than 12 months in custody and released in 2002 - 2008

	Variable	Mean	Std. Dev.
Demographics	Age at conviction	30.61	9.43
	UK	0.91	0.29
	White	0.84	0.36
	Black	0.09	0.29
	Asian	0.05	0.21
	Other ethnicity	0.01	0.11
	Unknown ethnicity	0.01	0.08
Index offence type	Violence	0.19	0.39
	Robbery	0.00	0.05
	Public order or riot	0.04	0.20
	Sexual offence	0.01	0.09
	Sexual offence (child)	0.01	0.09
	Soliciting	0.00	0.03
	Domestic burglary	0.02	0.13
	Other burglary	0.04	0.20
	Theft	0.23	0.42
	Handling	0.02	0.15
	Fraud and forgery	0.03	0.17
	Bail and offences	0.03	0.18
	Taking and driving away	0.03	0.16
	Theft from vehicles	0.02	0.13
	Other motoring offences	0.18	0.38
	Drink driving	0.05	0.23
	Criminal manage	0.02	0.15
	Drugs (trade/production)	0.01	0.07
	Drugs (possession)	0.02	0.14
	Other offences	0.04	0.19
Custody variables	Sentence length (days)	130.22	74.35
	Post release licence	0.15	0.36
Criminal histories	Previous prison spells	5.07	7.16
	Previous convictions	15.14	15.39
Match rates	NBD	0.78	0.41
	P45	0.65	0.48
	NBD & P45	0.61	0.49
N		263,146	

Table 4.2: RD estimates of treatment effect on reoffending outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1y reoff	0.006	0.009	0.015	0.017	0.016	0.016	0.006	0.015
	(0.023)	(0.021)	(0.016)	(0.015)	(0.017)	(0.016)	(0.023)	(0.021)
2y reoff	-0.014	-0.006	0.003	0.007	0.004	0.005	-0.010	0.001
	(0.020)	(0.019)	(0.014)	(0.013)	(0.015)	(0.014)	(0.020)	(0.019)
3y reoff	-0.013	-0.006	-0.004	0.000	-0.002	-0.001	-0.016	-0.005
	(0.018)	(0.017)	(0.013)	(0.012)	(0.014)	(0.013)	(0.019)	(0.017)
Bandwidth	3m	3m	6m	6m	12m	12m	12m	12m
Functional form	Lin.	Lin.	Lin.	Lin.	Quad.	Quad.	Cubic	Cubic
Control	No	Yes	No	Yes	No	Yes	No	Yes
N	6,758	6,758	13,366	13,366	26,966	26,966	26,966	26,966

- Notes: Standard errors are in parenthesis. The outcome variables are whether offenders reoffended within 1, 2 and 3 years after release from custody. The sample is restricted to male with custody length <1 year. The set of control variables include UK nationality, ethnicity, index offence type, previous number of custody spell, previous number of conviction and custodial sentence length. Statistical significance: ** 5% level, * 10% level.

Table 4.3: RD estimates of treatment effect on benefit outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1y ben	-0.031	-0.031	-0.015	-0.015	-0.018	-0.018	-0.032	-0.032
	(0.021)	(0.021)	(0.015)	(0.015)	(0.016)	(0.016)	(0.021)	(0.021)
2y ben	-0.029	-0.029	-0.009	-0.009	-0.014	-0.014	-0.027	-0.027
	(0.018)	(0.018)	(0.012)	(0.013)	(0.013)	(0.013)	(0.018)	(0.018)
3y ben	-0.026	-0.026	-0.013	-0.013	-0.015	-0.015	-0.028*	-0.028*
	(0.016)	(0.016)	(0.011)	(0.011)	(0.012)	(0.012)	(0.016)	(0.016)
Bandwidth	3m	3m	6m	6m	12m	12m	12m	12m
Functional form	Lin.	Lin.	Lin.	Lin.	Quad.	Quad.	Cubic	Cubic
Weight	N	Y	N	Y	N	Y	N	Y
N	5,307	5,307	10,429	10,429	20,966	20,966	20,966	20,966

- Notes: Standard errors are in parenthesis. The outcome variables are whether offenders ever claimed benefit within 1, 2 and 3 years after release from custody. The sample is restricted to male with custody length <1 year and matched PNC records to the NBD. The set of control variables (UK nationality, ethnicity, index offence type, previous number of custody spell, previous number of conviction and custodial sentence length and a binary benefit variable during the year before custody) is included in all specifications. Statistical significance: ** 5% level, * 10% level.

Table 4.4: RD estimates of treatment effect on employment outcomes using non-random P45 records

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1y emp	-0.008	-0.008	-0.023	-0.023	-0.021	-0.021	-0.015	-0.015
	(0.029)	(0.029)	(0.020)	(0.020)	(0.021)	(0.022)	(0.029)	(0.029)
2y emp	-0.020	-0.020	-0.019	-0.019	-0.020	-0.020	-0.011	-0.011
	(0.031)	(0.031)	(0.022)	(0.022)	(0.023)	(0.023)	(0.031)	(0.031)
3y emp	-0.012	-0.012	-0.004	-0.004	0.001	0.001	-0.011	-0.011
	(0.031)	(0.031)	(0.022)	(0.022)	(0.023)	(0.023)	(0.031)	(0.031)
Bandwidth	3m	3m	6m	6m	12m	12m	12m	12m
Functional form	Lin.	Lin.	Lin.	Lin.	Quad.	Quad.	Cubic	Cubic
Weight	N	Y	N	Y	N	Y	N	Y
N	4,084	4,084	8,061	8,061	16,280	16,280	16,280	16,280

- Notes: Standard errors are in parenthesis. The outcome variables are whether offenders ever worked in P45 employment within 1, 2 and 3 years after release from custody. The sample is restricted to male with custody length <1 year and only matched PNC records to the P45 database with non-random subseuquent P45 start dates. The set of control variables (UK nationality, ethnicity, index offence type, previous number of custody spell, previous number of conviction and custodial sentence length and a binary employment variale during the year before custody) is included in all specifications. Statistical significance: ** 5% level, * 10% level.

Table 4.5: RD estimates of treatment effect on employment outcomes using all P45 records

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1y emp	-0.007	-0.007	-0.025	-0.025	-0.022	-0.022	-0.014	-0.014
	(0.028)	(0.027)	(0.019)	(0.019)	(0.020)	(0.020)	(0.027)	(0.027)
2y emp	-0.018	-0.018	-0.014	-0.014	-0.014	-0.014	-0.007	-0.007
	(0.029)	(0.029)	(0.020)	(0.020)	(0.021)	(0.021)	(0.029)	(0.029)
3y emp	-0.018	-0.018	-0.008	-0.008	-0.002	-0.002	-0.017	-0.017
	(0.029)	(0.029)	(0.020)	(0.020)	(0.021)	(0.021)	(0.028)	(0.029)
Bandwidth	3m	3m	6m	6m	12m	12m	12m	12m
Functional form	Lin.	Lin.	Lin.	Lin.	Quad.	Quad.	Cubic	Cubic
Weight	N	Y	N	Y	N	Y	N	Y
N	4,648	4,648	9,134	9,134	18,507	18,507	18,507	18,507

- Notes: Standard errors are in parenthesis. The outcome variables are whether offenders ever worked in P45 employment within 1, 2 and 3 years after release from custody. The sample is restricted to male, age$\geq$18, previous conviction events $\leq$1 and all matched PNC records to the P45 database. The set of control variables (age on release, UK nationals, ethnicity, index offence type, previous number of custody spell, previous number of conviction, a binary benefit variable during the year before custody and a binary employment variale during the year before custody) is included in all specifications. Statistical significance: ** 5% level, * 10% level.

Table 4.6: Duration analysis estimates of the treatment effect on reoffending hazard

		(1)		(2)		(3)		(4)	
Treatment effect ($\hat{\beta_m}$)									
$m=$	1	-0.007	(0.074)	0.067	(0.052)	0.045	(0.048)	-0.002	(0.06)
	2	0.149	(0.087)	0.013	(0.061)	-0.001	(0.052)	-0.047	(0.063)
	3	0.142	(0.097)	0.075	(0.068)	0.023	(0.057)	-0.024	(0.067)
	4	0.018	(0.105)	0.062	(0.075)	0.084	(0.061)	0.037	(0.071)
	5	-0.192	(0.117)	-0.128	(0.084)	-0.058	(0.066)	-0.104	(0.075)
	6	-0.117	(0.126)	0.079	(0.09)	0.097	(0.07)	0.050	(0.079)
	7	-0.097	(0.139)	0.023	(0.103)	0.098	(0.079)	0.051	(0.086)
	8	-0.097	(0.148)	0.087	(0.11)	0.040	(0.084)	-0.007	(0.091)
	9	0.123	(0.167)	0.030	(0.116)	0.091	(0.087)	0.044	(0.094)
	10	0.128	(0.179)	0.038	(0.124)	0.018	(0.091)	-0.029	(0.098)
	11	0.211	(0.2)	0.112	(0.134)	0.077	(0.097)	0.031	(0.103)
	12	-0.036	(0.184)	-0.106	(0.133)	-0.016	(0.099)	-0.062	(0.106)
	13	-0.330	(0.218)	-0.234	(0.15)	-0.218**	(0.11)	-0.264**	(0.116)
	14	0.000	(0.24)	0.055	(0.163)	0.110	(0.116)	0.064	(0.122)
	15	-0.077	(0.225)	-0.098	(0.162)	-0.101	(0.12)	-0.148	(0.126)
	16	-0.251	(0.237)	-0.303*	(0.169)	-0.201*	(0.12)	-0.248**	(0.125)
	17	-0.142	(0.233)	0.030	(0.169)	0.084	(0.128)	0.038	(0.133)
	18	-0.464	(0.278)	0.118	(0.184)	0.027	(0.132)	-0.020	(0.137)
	19	-0.072	(0.265)	-0.121	(0.207)	0.037	(0.148)	-0.009	(0.152)
	20	-0.462	(0.299)	0.031	(0.222)	0.162	(0.152)	0.115	(0.156)
	21	-0.200	(0.277)	0.100	(0.196)	0.330**	(0.148)	0.283*	(0.153)
	22	0.067	(0.29)	-0.118	(0.225)	-0.013	(0.161)	-0.059	(0.165)
	23	0.358	(0.326)	0.198	(0.209)	0.125	(0.154)	0.079	(0.158)
	24	0.500	(0.309)	0.401*	(0.232)	0.126	(0.17)	0.080	(0.174)
	25	0.188	(0.33)	-0.184	(0.244)	0.185	(0.177)	0.138	(0.181)
	26	-0.433	(0.411)	-0.262	(0.285)	0.088	(0.184)	0.042	(0.187)
	27	0.039	(0.374)	-0.027	(0.282)	-0.052	(0.2)	-0.099	(0.203)
	28	-0.334	(0.355)	0.085	(0.265)	0.140	(0.198)	0.094	(0.202)
	29	0.170	(0.376)	0.180	(0.259)	0.117	(0.194)	0.070	(0.197)
	30	-0.487	(0.486)	0.145	(0.356)	0.271	(0.223)	0.224	(0.226)
	31	-1.098***	(0.442)	-0.617**	(0.302)	-0.300	(0.231)	-0.346	(0.234)
	32	-0.062	(0.45)	0.092	(0.291)	0.150	(0.223)	0.103	(0.226)
	33	0.298	(0.495)	-0.162	(0.341)	0.205	(0.243)	0.159	(0.246)
	34	-0.059	(0.474)	-0.169	(0.298)	-0.309	(0.234)	-0.355	(0.237)
	35	0.040	(0.439)	0.279	(0.334)	-0.030	(0.246)	-0.076	(0.248)
	36	-1.449**	(0.647)	-0.860**	(0.423)	-0.231	(0.259)	-0.278	(0.261)
Window		1		2		3		3	
Functional form		Linear		Linear		Quadratic		Cubic	
N		6,758		13,366		26,966		26,966	

- Notes: Standard errors are in parenthesis. Same sample as reoffending RD analysis. No control variables are included. Statistical significance: *** 1% level, ** 5% level, * 10% level.

Table 4.7: Duration analysis estimates of the treatment effect on benefit hazard

		(1)		(2)		(3)		(4)	
Treatment effect ($\hat{\beta_m}$)									
$m=$	1	-0.063	(0.061)	-0.032	(0.043)	-0.036	(0.044)	-0.062	(0.058)
	2	-0.153	(0.129)	-0.012	(0.091)	-0.017	(0.072)	-0.043	(0.081)
	3	0.026	(0.161)	-0.151	(0.112)	-0.104	(0.085)	-0.129	(0.093)
	4	0.091	(0.183)	0.088	(0.131)	0.102	(0.097)	0.076	(0.104)
	5	0.172	(0.193)	0.179	(0.133)	0.111	(0.1)	0.086	(0.107)
	6	-0.038	(0.2)	0.173	(0.145)	0.089	(0.105)	0.064	(0.111)
	7	-0.194	(0.23)	-0.091	(0.164)	-0.179	(0.122)	-0.205	(0.127)
	8	-0.412*	(0.243)	-0.312*	(0.169)	-0.432***	(0.12)	-0.457***	(0.126)
	9	-0.217	(0.258)	-0.211	(0.179)	-0.100	(0.131)	-0.126	(0.136)
	10	-0.102	(0.249)	0.056	(0.175)	-0.040	(0.131)	-0.066	(0.137)
	11	0.071	(0.263)	0.207	(0.196)	-0.070	(0.139)	-0.096	(0.144)
	12	-0.684**	(0.297)	-0.604***	(0.217)	-0.404***	(0.155)	-0.430***	(0.16)
	13	-0.047	(0.296)	-0.074	(0.212)	-0.108	(0.16)	-0.134	(0.164)
	14	0.011	(0.32)	-0.028	(0.21)	-0.203	(0.157)	-0.228	(0.162)
	15	0.659*	(0.355)	0.489**	(0.237)	0.083	(0.172)	0.057	(0.176)
	16	-1.177***	(0.411)	-0.316	(0.257)	-0.176	(0.179)	-0.201	(0.183)
	17	0.017	(0.338)	0.190	(0.24)	0.067	(0.178)	0.042	(0.182)
	18	-0.096	(0.32)	-0.090	(0.245)	-0.002	(0.183)	-0.027	(0.186)
	19	-0.646*	(0.382)	-0.437	(0.266)	-0.094	(0.185)	-0.120	(0.189)
	20	0.174	(0.385)	-0.084	(0.288)	-0.208	(0.194)	-0.234	(0.197)
	21	0.245	(0.38)	0.137	(0.246)	0.191	(0.183)	0.165	(0.186)
	22	-0.357	(0.424)	-0.633**	(0.274)	-0.299	(0.207)	-0.325	(0.211)
	23	0.258	(0.495)	0.528	(0.336)	0.195	(0.21)	0.169	(0.213)
	24	0.247	(0.417)	-0.084	(0.294)	0.047	(0.225)	0.021	(0.228)
	25	-0.407	(0.467)	-0.473	(0.334)	-0.254	(0.231)	-0.279	(0.234)
	26	0.091	(0.608)	0.508	(0.444)	0.404	(0.282)	0.378	(0.284)
	27	-0.179	(0.42)	-0.144	(0.318)	-0.262	(0.224)	-0.288	(0.227)
	28	-0.089	(0.502)	0.034	(0.387)	-0.178	(0.262)	-0.204	(0.264)
	29	-1.366***	(0.569)	-0.579*	(0.351)	-0.322	(0.238)	-0.348	(0.241)
	30	0.214	(0.588)	0.536	(0.396)	0.049	(0.259)	0.023	(0.262)
	31	0.176	(0.542)	-0.270	(0.389)	-0.408	(0.259)	-0.434*	(0.262)
	32	0.742	(0.692)	0.426	(0.405)	0.127	(0.274)	0.101	(0.276)
	33	-0.438	(0.588)	0.050	(0.419)	-0.052	(0.257)	-0.078	(0.26)
	34	-0.656	(0.478)	-0.630	(0.396)	0.056	(0.268)	0.030	(0.271)
	35	0.208	(0.588)	0.322	(0.435)	-0.282	(0.31)	-0.308	(0.312)
	36	-0.021	(0.134)	-0.007	(0.097)	-0.007	(0.075)	-0.033	(0.084)
Window		1		2		3		3	
Functional form		Linear		Linear		Quadratic		Cubic	
N		5,307		10,429		20,966		20,966	

- Notes: Standard errors are in parenthesis. Same sample as benefit RD analysis, without propensity score weighting. No control variables are included. Statistical significance: *** 1% level, ** 5% level, * 10% level.

Table 4.8: Duration analysis estimates of the treatment effect on employment hazard

	(1)		(2)		(3)		(4)	
Treatment effect ($\hat{\beta_m}$)								
$m=$ 1	0.015	(0.146)	-0.057	(0.102)	-0.128	(0.083)	-0.177*	(0.096)
2	-0.113	(0.151)	-0.095	(0.11)	-0.154*	(0.09)	-0.203**	(0.102)
3	-0.018	(0.17)	0.099	(0.124)	-0.08	(0.097)	-0.129	(0.109)
4	-0.256	(0.188)	-0.12	(0.133)	-0.045	(0.102)	-0.094	(0.113)
5	0.1	(0.201)	-0.095	(0.14)	-0.066	(0.112)	-0.115	(0.122)
6	-0.088	(0.222)	-0.112	(0.154)	-0.007	(0.116)	-0.055	(0.126)
7	-0.552**	(0.24)	-0.352**	(0.174)	-0.228*	(0.123)	-0.277**	(0.133)
8	-0.383	(0.238)	-0.174	(0.175)	-0.045	(0.126)	-0.094	(0.135)
9	-0.154	(0.234)	-0.053	(0.165)	0.009	(0.126)	-0.04	(0.135)
10	-0.007	(0.255)	-0.131	(0.186)	0.011	(0.135)	-0.038	(0.144)
11	-0.342	(0.277)	-0.032	(0.186)	0.015	(0.132)	-0.034	(0.141)
12	-0.713***	(0.269)	-0.528***	(0.193)	-0.31**	(0.14)	-0.359***	(0.149)
13	-0.172	(0.246)	-0.224	(0.178)	-0.196	(0.132)	-0.244*	(0.141)
14	-0.141	(0.266)	0.026	(0.197)	-0.021	(0.145)	-0.07	(0.153)
15	0.555	(0.359)	0.251	(0.222)	0.352**	(0.16)	0.304*	(0.168)
16	-0.47	(0.3)	-0.352*	(0.198)	-0.305**	(0.155)	-0.353**	(0.163)
17	-0.275	(0.306)	-0.01	(0.21)	-0.106	(0.152)	-0.154	(0.159)
18	0.164	(0.326)	0.033	(0.223)	0.027	(0.164)	-0.021	(0.172)
19	-0.085	(0.327)	-0.014	(0.223)	-0.154	(0.168)	-0.202	(0.175)
20	0.088	(0.342)	0.049	(0.232)	-0.034	(0.169)	-0.083	(0.176)
21	-0.725**	(0.329)	-0.646***	(0.242)	-0.421***	(0.174)	-0.47***	(0.18)
22	-0.008	(0.306)	-0.249	(0.239)	-0.29*	(0.174)	-0.339*	(0.18)
23	0.204	(0.324)	0.34	(0.246)	0.308*	(0.178)	0.26	(0.184)
24	0.276	(0.351)	-0.105	(0.228)	0.041	(0.178)	-0.007	(0.184)
25	-0.679*	(0.384)	-0.528*	(0.272)	-0.461***	(0.191)	-0.51***	(0.197)
26	0.521	(0.348)	0.261	(0.246)	0.029	(0.172)	-0.02	(0.179)
27	0.689**	(0.34)	0.247	(0.247)	0.199	(0.185)	0.15	(0.191)
28	0.083	(0.317)	0.227	(0.23)	-0.121	(0.174)	-0.17	(0.181)
29	0.054	(0.399)	0.061	(0.277)	0.009	(0.186)	-0.039	(0.193)
30	0.187	(0.387)	-0.012	(0.281)	-0.08	(0.196)	-0.129	(0.202)
31	0.389	(0.454)	0.067	(0.282)	0.078	(0.187)	0.029	(0.193)
32	0.511	(0.381)	0.525*	(0.297)	0.211	(0.197)	0.162	(0.203)
33	-0.777*	(0.438)	-0.339	(0.301)	-0.553***	(0.212)	-0.602***	(0.217)
34	0.096	(0.433)	0.235	(0.284)	0.187	(0.2)	0.138	(0.206)
35	0.02	(0.464)	0.141	(0.329)	-0.057	(0.215)	-0.106	(0.221)
36	-0.23**	(0.113)	-0.122	(0.079)	-0.044	(0.07)	-0.093	(0.085)
Window	1		2		3		3	
Functional form	Linear		Linear		Quadratic		Cubic	
N	4,084		8,061		16,280		16,280	

- Notes: Standard errors are in parenthesis. Same sample as employment RD analysis, excluding records with randomised start dates and without propensity score weighting. No control variables are included. *** 1% level, ** 5% level, * 10% level.

Table 4.9: Treatment coefficients for RD regressions with control variables as outcomes

		Estimates	S.E.
Demographics	UK	0.008	(0.008)
	White	0.016	(0.012)
	Black	-0.018*	(0.009)
	Asian	-0.002	(0.007)
	Other ethnicity	0.002	(0.004)
	Unknown ethnicity	0.001	(0.002)
Index offence	Violence	0.031**	(0.014)
	Robbery	-0.003	(0.002)
	Public order or riot	-0.009	(0.008)
	Sexual offence	0.001	(0.002)
	Sexual offence (child)	0.001	(0.002)
	Soliciting	-0.001	(0.001)
	Domestic burglary	-0.009*	(0.006)
	Other burglary	-0.006	(0.007)
	Theft	0.015	(0.013)
	Handling	-0.003	(0.005)
	Fraud and forgery	0.002	(0.005)
	Bail and offences	0.005	(0.007)
	Taking and driving away	0.000	(0.007)
	Theft from vehicles	0.001	(0.005)
	Other motoring offences	-0.013	(0.014)
	Drink driving	-0.016***	(0.005)
	Criminal manage	0.007	(0.006)
	Drugs (trade/production)	0.001	(0.002)
	Drugs (possession)	-0.004	(0.005)
	Other offences	-0.000	(0.007)
Custody	Sentence length	-1.414	(2.666)
Criminal histories	Previous prison spells	-0.265	(0.233)
	Previous convictions	-0.080	(0.096)
Other histories	Benefit claim	-0.007	(0.017)
	P45 Employment	-0.003	(0.021)
N =		13,366	

- Notes: All coefficients are estimated using specification (2) under table 2. The treatment and control groups are statistically different at the 5% level in the variables in red. The benefit and employment histories variables are defined as whether the offender ever claimed benefit / worked during the 1 year prior to custody. The treatment coefficients for these two histories variables are estimated using the NBD and P45 matched samples respectively. Statistical significance: *** 1% level, ** 5% level, * 10% level.

Table 4.10: Sensitivity of estimates to the inclusion of 9-11 months group

	(1)	(2)		(3)	(4)		(5)	(6)
1y reoff	0.017	0.019	1y ben	-0.016	-0.015	1y emp	-0.025	-0.033
	(0.015)	(0.016)		(0.015)	(0.016)		(0.019)	(0.020)
2y reoff	0.007	0.003	2y ben	-0.009	-0.008	2y emp	-0.014	-0.028
	(0.013)	(0.014)		(0.012)	(0.013)		(0.020)	(0.021)
3y reoff	0.000	-0.001	3y ben	-0.013	-0.011	3y emp	-0.008	-0.020
	(0.012)	(0.013)		(0.011)	(0.012)		(0.020)	(0.021)
Custodial length	0-11	0-8	Custodial length	0-12	0-9	Custodial length	0-12	0-9
N	13,366	12,038	N	10,429	9,390	N	9,134	8,185

- Notes: Standard errors are in parenthesis. The specification of linear functional form in age with a bandwidth of 6 months is used. The full set of control covariates is included. Statistical significance: ** 5% level, * 10% level.

Table 4.11: Sensitivity of estimates to criminal histories

	(1)	(2)	(3)
1y reoff	-0.015	0.015	0.027*
	(0.055)	(0.070)	(0.016)
2y reoff	0.007	0.002	0.016
	(0.062)	(0.076)	(0.013)
3y reoff	-0.013	0.007	0.009
	(0.066)	(0.076)	(0.012)
Previous conviction	0	1	>1
N	892	731	11,743

	(4)	(5)	(6)
1y ben	-0.028	-0.077	-0.010
	(0.075)	(0.080)	(0.015)
2y ben	-0.051	-0.027	-0.003
	(0.069)	(0.074)	(0.013)
3y ben	-0.057	-0.042	-0.007
	(0.065)	(0.068)	(0.011)
Previous conviction	0	1	>1
N	553	519	9,357

	(7)	(8)	(9)
1y emp	-0.018	-0.053	-0.023
	(0.079)	(0.089)	(0.020)
2y emp	0.019	-0.013	-0.018
	(0.075)	(0.087)	(0.022)
3y emp	-0.015	0.061	-0.013
	(0.070)	(0.083)	(0.022)
Previous conviction	0	1	>1
N	628	532	7,974

- Notes: Standard errors are in parenthesis. The specification of linear functional form in age with a bandwidth of 6 months is used. Statistical significance: ** 5% level, * 10% level.

Chapter 5

The labour market cost of a criminal record

5.1 Introduction

What is the cost of crime? The term is most easily associated with damages to victims, but criminals also face costs when they undertake crimes. Costs mainly come in two forms: direct punishment cost such as a fine or an incarceration spell, and potential cost in terms of limited future options in employment (and travel, accommodation, insurance, etc., to a lesser extent) due to social stigma, which I refer to as scarring. The former is often observable and easy to measure, but the latter much less so. Of course, this does not stop common wisdom to emerge about the extent of scarring. A quick search of "the effect of a criminal record" on the internet would return many mainstream media commentaries and real-life stories about how ex-criminals struggle to resettle and find a job even after a long time. Take the specific case of the UK. The Government's view on this issue is perfectly captured by the following quote of the then-Deupty Prime Minister Nick Clegg in 2014 at the introduction of a new law to shorten the amount of time before offenders can legally hold back from revealing their past convictions: "*Making a mistake and committing a minor crime when you are fifteen shouldn't mean you are barred from employment for the rest of your life.*" Clearly, the popular belief is that scarring has so far been disproportionate. Is this claim supported by empirical research?

Understanding and estimating the scarring cost is useful and important for several reasons. Apart from scarring being a topic of immense public interest, it should be a component of the behavioural process in any economic model of crime. Most dynamic models of crime (Imai and Krishna 2004; Lochner 2004; Sickles and Williams 2008; McCrary 2010) attempt in some ways to incorporate the feature of agents taking into account of future labour market implications in their current criminal decisions. It is important to know what modelling assumptions about scarring would be supported by data. Empirical research on the effect of crime on labour market outcomes so far shows crime has negative effects, but probably not so much as life-ruining as it is popularly believed. Early empirical work by Freeman (1991) and Waldfogel (1994) using self-reported longitudinal data suggest that a spell in prison when young or a youth criminal record can have long-term damage on adult employment and wages. More specifically, Freeman (1991) finds that a young male in prison in 1979 worked 25% less than he otherwise would have in the next 8 years. More recently, Apel and Sweeten (2010) and Dominguez Alvarez and Loureiro (2012) have come to similar conclusion about the relationship between imprisonment and labour market outcomes using American and German self-report datasets respectively. However, these findings are reliant on accurate self-reporting, and the samples sizes are sometimes small. They also focus specifically on the effect of imprisonment, not that of criminal conviction, restricting the generalizability of their results. One alternative and perhaps more direct way to estimate the scarring of a criminal record is by studying employers behaviour through survey data or experiment. Holzer et al. (2002) find using US employers survey data that employers are much more reluctant to hire persons with a criminal record, even when compared to other disadvantaged minority groups such as benefits recipients. More recently, Baert and Verhofstadt (2015) conduct a field experiment in Belgium by sending out two fictitious applications of school-leavers, identical bar the mention of a juvenile record in one of them, to nearly 500 vacancies for labour market entry jobs. They find that the callback rate for non-juvenile applications is 29% higher that for juvenile. Another line of empiricial research exists and uses large-scale administrative datasets. Grogger (1995) finds using an US administrative panel data of arrest and earnings that historical arrests cause no damage to the employment and earnings of young men after 6 quarters, and this is also true when the person is sentenced to prison. Kling (2006) finds that conditional on being a prisoner, incarceration length has no effect on job market outcomes. The literature largely agrees that scarring effects are heterogeneous by punishment type, but there is no sense of consensus on the magnitude or persistence.

In this paper, I empirically estimate the scarring effect of criminal convictions on labour market outcomes by using the linked dataset described in chapter 3 that covers the criminal record and earnings of individuals who were convicted or cautioned in England and Wales between 2003 and 2013. By carrying out fixed effects analysis using a distributed lag model, I find mild negative effects of convictions on employment likelihood that persists for at least 10 quarters, and some evidence that scarring on earnings dies out after 10 quarters. This appears in line with a hypothesis of statistical discrimination combined with employer learning. I estimate the scarring effect by punishment type, and find consistent with the existing literature that a prison spell has the largest and most persistent effect, while less severe punishments like fines and police cautions have smaller effects. There is little evidence that crime types matter. I test another hypothesis whereby employment experience to date becomes a more useful signal of true productivity in the presence of a criminal record and find mixed support. I carry out the analysis separately for two periods, before and after the recent great economic crisis which started in September 2008. I find that the results are somewhat different and inconsistent. The difference can be explained by a compositional change in the productivity of offenders using standard economic of crime, and anecdotal evidence suggests this could be the case.

The rest of the paper is organized as follows. Section 2 gives an overview of the theory behind scarring. Section 3 describes the data. Section 4 outlines the econometric specification. Results are discussed in section 5. In section 6 I present evidence on the robustness of the analysis. Finally, section 7 concludes.

5.2 Theory of Employment Scarring

The adverse effect of a criminal record can come in many forms, affecting different areas of life. I focus on the scarring on employment outcomes, specifically the extent to which the employment likelihood and/or earnings of an individual are reduced due to the existence of a past conviction independent of his true productivity. I review the possible theories behind scarring, but do not discuss whether scarring is, or what magnitude of it would be, fair.

Many countries have in the name of public interest placed legal restrictions on limiting the jobs that offenders can possibly take. For example, in the UK jobseekers with a criminal record cannot take up posts to become say medical doctors or members of the armed force. This legal restriction itself does not necessarily inflict a great deal of scarring if the offenders were not in those occupation in the first place, and it only applies to the minority of jobs and individuals are always free to seek jobs in many other sectors. However, as Rasmusen (1996) points out, convicted criminals suffer from public penalties as well as stigmatization, which could lead to reluctance of others to interact with them socially and economically. In the UK, employers, though not legally bound to, often ask job applicants to reveal their criminal history either at the start of the process or during it. Individuals are legally obliged to reveal their history if the convictions have not become 'spent'. The time it takes for a conviction to become spent varies by punishment type. For example, up till the recent UK policy change in 2015, a conviction dealt with by a fine takes 5 years to become spent and a conviction dealt with by immediate custody of between 6 months to 30 months takes 10 years to become spent. After this time, applicants can legally lie about their criminal past, as long as the job they are applying to is not from the few occupations that are exempted from this disclosure law, such as carers or school teachers. In any case, an applicant can always request a copy of the criminal background check himself to see what unspent convictions he needs to disclose to prospective employers, and firms are also able to arrange such check on applicants as employers. While a criminal record does not legally prevent someone from working in the large majority of occupations and industries, the compulsory disclosure of recent convictions when asked may easily induce stigmatization and therefore scarring.

Employers discriminating against a person with criminal record is a likely and often discussed source of scarring. Employers may discriminate against a criminal record purely based on taste and preference (Becker 1957) and not on productivity concerns. Under this theory, if there are enough discriminating firms in the economy so that discrimination cannot be competed away, a wage/hiring differential will emerge and sustain between offenders and non-offenders of identical productivity. An alternative discrimination theory to explain scarring is that of statistical discrimination (Phelps 1972; Arrow 1973; Aigner and Cain 1977). Under this theory, employers form expectation about the group means and/or variances of the productivity of offenders and non-offenders. General economic theory of crime (Becker 1968) gives the insight that individuals with lower legitimate earnings are more likely to engage in criminal activities. Based on this, employers may form the belief that offenders are a group within the working population with lower average productivity and this may then be imbued in lower wage offers and fewer interview opportunities, resulting in a wage and/or hiring differential. On the other hand, given how technology has changed productivity and also how the nature of crime has changed over the recent years, it can also be the case that criminality in certain areas (e.g. white-collar or internet crimes) are complementary to the skills that firms are looking for and hence firms may not expect offenders to have lower productivity on average. They may instead expect a higher variance of skill levels within the offenders group compared to the population. If firms also are risk-averse (Aigner and Cain 1977), a gap in wage offer and hiring would then exist even if firms do not believe offenders are less capable on average, and the gap is bigger the more risk-averse the firms are.

One dimension of scarring is persistence, and links here can be made to the employer learning literature (Altonji and Pierret 2001; Schoenberg 2007). Suppose discrimination against offenders is taste-based, then any wage and hiring gaps are likely to persist conditional on there is not enough non-discriminating firms to compete and that the competition environment does not change over the analysis period. Alternatively, if discrimination is statistical, under employer learning the wage gap that exists at first between working offenders and working non-offenders of same productivity will close over time. The hiring gap would still persist over time because there is no scope to learn before employment begins. An empirical analysis of the persistence in hiring and earnings gaps could reveal the mode of discrimination through a theory of employer learning.

A final area of the literature that I refer to is that of signalling costs and their consequences. Golbe (1985) shows that in an environment where productivity is imperfectly observed through a noisy signal and the proportion of high- and low-productivity workers are the same across groups but minority workers face a higher cost in obtaining a good signal, this can lead to an equilibrium where minority workers are rewarded more for the good signal. Applying the concept here, let's take workers with criminal record as the minority, and the signal of productivity being work experience. Suppose the productivity composition is the same for the groups of criminals and non-criminals, and this is likely true for many types of less severe offences, and offenders face a higher cost to obtaining a long and stable employment history than non-offenders, say due to some correlating factors that led them into committing crime in the first place. Then, the same long employment history would reveal more positive information for an offender than it would for a non-offender. In other words, the good signals are of better quality for offenders and should, at the margin, be worth more. I can test this hypothesis directly from the data.

5.3 Data

5.3.1 Component datasets

The empirical analysis is carried out using the linked PNC and P14 Earnings data resulting from the data linkage initiative between the Ministry of Justice and Her Majesty's Revenue and Customs. Refer to chapter 3 for general descriptions of the datasets as well as the matching.

There are three data issues associated with the datasets that are relevant here. Firstly, as discussed in chapter 3, the PNC records only offences that are punishable by imprisonment plus many of the serious summary offences (ie recordable offences). It generally does not cover less serious offences that most likely attract fines as punishment, such as TV license evasion, careless driving, driving without insurance, reproducing British currency notes, etc. Despite missing out on information about non-recordable offences which make up a significant part of overall crime, I argue this does not affect my analysis. This is because these offences would not show up in most levels of criminal record checks that employers or job applicants requested during the sample periods of the analysis, meaning there was no stigma attached. The only possibility that they would matter for the analysis is through inducing negative human capital effects, but I deem this as unlikely. Secondly, I choose to use P14 Earnings data in informing employment spell information in this analysis over P45 Employment data, unlike in chapters 4 and 6. I have explained in chapter 3 that there is not enough information to judge if P14 or P45 is more accurate with regards to employment spell, but since I am interested in the earnings aspect in this analysis which is only available in P14, I choose to derive all the necessary information consistently from it only. Finally, since the data linkage carried out by MoJ/DWP uses the MoJ extract of PNC as the basis, I observe no employment information at all for individuals who do not have a criminal record. This creates a unique problem in terms of the creation of a suitable control group - ideally one would like to have information about both offenders and non-offenders. I follow the approach in Grogger (1995), explained in more detail below.

5.3.2 Analytical samples

Using PNC and P14 I create quarterly panel of criminal records and labour market outcomes. I choose to focus on two periods of interest, both of which are 16 quarters long: the pre-economic-crisis "good" period of April 2004 – March 2008 and the post-crisis "bad" period of October 2008 – September 2012. The quarters are structured this way to coincide with the UK financial quarters that the P14 is aligned to. The issue I have in constructing the analytical samples is the same as Grogger (1995) - that the panel contains only individuals who have a criminal record and I need to create a suitable control group from within. Following Grogger's (1995) method, I exploit the differences in the timing of individuals' first appearances on the PNC. Within each of the 16-quarter periods, I treat those who first appeared (ie convicted for the first time in their lives) in the first 12 quarters as the treatment group and others who first appeared between the 13th and 16th quarter as the control group. I then analyse employment outcomes only in the first 12 quarters. The basis of doing this is that the evolution of employment outcomes for the control group during the first 12 quarters should be free of any conviction effects. One person within each sample period consequently contributes 12 rows of quarterly observations towards the panel. I randomly choose about 110,000 individuals in each period to form the analysis panels and avoid using the full PNC which proves very large when turned into a panel. The sampling rates are 15% and 21% respectively for the two periods. The key statistics of the samples are well matched to the population and I believe there are no representativeness concerns.

Tables 1 and 2 summarize the descriptive statistics of the two samples. They show the averages of variables pooled over all 12 quarters. The ratios of treatment/control groups size are about 3-4:1, consistent with the design that selection into the control group is based on the individual's PNC record first appearing in the last year of the 4-year analytical periods. The average age is about 34 in both periods, though it has increased slightly in the later years. The gender and ethnicity mix is fairly constant between groups and across time with about three-quarters being male, and also three-quarters being white. On the surface, there is little support for claims that offenders do a lot worse in the labour market. The control groups in both samples, having no conviction before and during the analysis periods, have better quarterly earnings, but surprisingly not by much. The differences in employment rates are smaller, and subject to sampling errors the control group in the good years even had a slightly but not statistically lower quarterly employment rates. Notice that average quarterly employment rates of around 45-50% and quarterly earnings of about £1800 are poorer labour market outcomes than the general population by some way, even though the control group individuals have had no convictions. This suggests that potential criminals are not a random subset of the working population even when they are not under the effect of a proven conviction and this must be taken into account in any analysis of scarring. Otherwise, cross-sectional research designs may simply uncover spurious scarring.

5.4 Econometric specification

I employ mainly a distributed lag model with fixed effects in my estimation, ie:

$$Y_{it} = \sum_j X'_{it-j}\beta + Z'_{it}\gamma + u_i + e_{it}, \tag{5.1}$$

where Y_{it} is either the quarterly employment indicator or earnings[1] of person i in period t, X_{it-j} the full vector of conviction variables lagged by j quarters including the disposal and crime type interactions, Z_{it} the vector of non-crime variables such as age and age squared, u_i the unobserved "fixed effect" that may correlate with all X_{it-j} and e_{it} a random unobserved error. When calculating standard errors, the errors are clustered at the individual level. The number of lag I choose is 10. I use the within

[1] Interpreting the scarring coefficients on earnings is not straight forward and requires caution. Theoretically, they involve both the external and internal margins, and are not clear-cut parameters.

estimator to calculate the coefficient vector of interest $\hat{\beta}$. My specification is similar to Grogger's (1995) but there are two main advantages over it. Firstly, my main explanatory variable is conviction instead of arrest. Conviction is a much more accurate measure of criminality since it does not include innocent individuals who were wrongly arrested and later acquitted. For this reason it is also a more likely and direct cause of employer stigmatization than simply arrest which employers may not observe at all, allowing me to interpret the results much more confidently using a scarring hypothesis. Secondly, I allow a larger number of lags than 6. This is important because, as will be seen, scarring can persist beyond the immediate short term and including more lags can reveal dynamic patterms that are of interest.

The fixed-effect set-up takes advantage of the longitudinal nature of the datasets in eliminating the time-invariant u_i, which in our case represent a wide range of variables that I do not have data on such as parental criminal history, area of upbringing, education, substance misuse history, etc., and other genuinely unobservable characteristics such as attitude and cognitive functioning. As these variables explain both labour market performance and criminality, failure to deal with them would lead to inconsistent estimation of the conviction coefficients β. The distributed lag feature allows us to estimate the effect of current conviction on both current and future employment outcomes, to see how scarring shape out dynamically. Other the other hand, there are, of course, limitations to this method. I discuss further under the Robustness section and show the results are in general valid under a different specification.

5.5 Results

I first carry out fixed-effect analysis for the two samples. I start by discussing the empirical results for the good years (2004-2008) sample. Columns 1 and 2 of table 3 show OLS results for employment and earnings, and columns 3 and 4 show FE results. We know that OLS is biased in the existence of fixed effect and this appears to be the case here. The more negative OLS results are consistent with a theory of confounding unobservables causing both poor labour market outcomes and more active criminal profile. I focus on FE results from here onwards. On the employment indicator regression, I see that the baseline effect of a current period conviction (j=0) on employment likelihood within the same quarter is -1.4% point. A conviction from the previous quarter reduces current employment likelihood by 2.4% point. The effect on current employment in general rises the more lagged the conviction is, and by the tenth lag (j=10), the effect is still statistically significant at -3.5% point. Relative to the quarterly employment rate of this sample at 44%, the reduction in employment due to a past conviction ranges from 3% to 10% of the mean. While this is a clear departure from Grogger's (1995) finding that no effects are significant for lags over 6 quarters old, it is still a long way from being detrimental. Turning my attention to the scarring on earnings, I see a persistent reduction of about £100, or equivalently 5-6% of this sample's quarterly average. This is again different from Grogger (1995)'s finding of diminishing scarring on earnings.

The baseline estimates so far are true for convictions of property crime with a sentence of discharge. Table 4 and 5 show the effects on employment and earnings for being sentenced to different disposals and for convicted of violent crime. The results are somewhat consistent with the popular belief in the sense that a more severe punishment causes more damage in the labour market. Certainly, a record of having been to prison reduces employment and earnings more at first (up to over 30% of the mean) than a record of having been to probation (up to over 10%), which in turn is more damaging at first than a record of discharge, police caution or fine, although partly and especially at first it can be due to the incapacitating nature of the custodial sentences. However, all disposals appear to have similar impact after 10 quarters have expired. This suggests that ultimately it is the criminal record that matters, not necessarily the type of punishment. This is also true for the type of crime convicted - violent and property crime convictions are more of less equally damaging. Although not reported in the tables, analyses by further crime type breakdown are available but they do not reveal any interesting patterns. For example, fraud convictions are not found to be statistically more damaging to employment than serious motoring offences, as one might have conjectured.

I now move on to discuss the analysis of the 2008-2012 sample, with results shown in tables 6-8. It provides an interesting comparison to the analysis of the previous good years sample. The fixed effect cofficients of convictions on employment are of similar magnitude to the previous sample albeit slightly higher, ranging between -2% points to -5% points with a general trend of increase the higher the number of lags. The baseline effects of conviction on earnings, on the other hand, look different to before and resembles Grogger's (1995) finding of a diminishing effect, starting from a contemporaneous reduction of 6.8% of the mean quarterly earnings and then down to statistically insignificant levels after 10 quarters. For the heterogenous effects on employment, the general pattern again shows that the initial scarring is proportionate to the severity of punishment but eventually converges to similar levels, and the effects of property and violent crime convictions are largely similar. As for the heterogenous effects on earnings, the correlation of damages to severity of punishing remains and the diminishing pattern in the baseline case is apparent for all disposals, but I see, different from before, that violent crime convictions are now more damaging.

Overall, the empirical results are indicative of the kind of discrimination and learning behaviour that employers adopt. First of all, a hypothesis of pure taste-based discrimination against criminal record is not supported by either the diminishing effect on earnings detected in the 2008-2012 sample in this study or Grogger's (1995) previous finding. This is because, if discrimination is based on taste, then any earnings gap would reflect the time-constant distaste of employers to take on someone with a criminal record and this cannot be alleviated in time through learning. A hypothesis of statistical discrimination, on the other hand, is plausible and with additional normal assumptions the results would suggest that statistical discrimination by group variance is more important than discrimination by group mean. Why? Let me take the simple model of criminal behaviour of the Becker (1968) type where the marginal offender is the person with a one-dimensional productivity measure being just below the criminality threshold that makes it just worthwhile for him to commit crime. As the economy turns from good to bad, unemployment rises and the criminality threshold rises too because it becomes harder and more costly to find or stay in a job. Consequently, the marginal offender now is going to have higher productivity than before, leading to a higher mean and variance of productivity for the group of offenders. Now consider the group of non-offenders. By virtue of the discussion above, the average productivity of the group of non-offenders is also going to be higher, but its variance smaller. Overall, I know for sure that the gap in the group variances has increased, but the gap in the means has probably decreased. The latter is due to the fact that if earnings in the population are approximately normally distributed and the marginal offender's legitimate earning is less than the median, the increase in mean productivity of the non-offender group shall be less than that of the offender group.[2] According to statistical discrimination theories, an increase in the variance gap would raise the effect of scarring, while a decrease in the mean gap would reduce it. Since I observe that scarring is in general more severe both in magnitude and in relation to the average employment likelihood and earnings during the later and economically worse years, it is suggestive that statistical discrimination by variance is the more common type of strategy employed by firms.

In terms of employer learning, I see some evidence of it in the 2008-2012 analysis. In that sample, the employment scarring stays pretty constant in relation to the number of lags, while the earnings scarring reduces with it. This can be attributed to the catching up in earnings through time of the offenders who managed to find a job. Both observations can be explained by employer learning. If statistication discrimination against a criminal record regardless of timing occurs at the point of appointment, the scarring on employment would not diminish as there is no learning opportunities available to alleviate it. However, for those offenders who managed to get employed, employers can begin the learning process and start rewarding offenders more in accordance to their true productivities over time. This plausibly explains the diminishing scarring effect on earnings in the 2008-2012 sample and Grogger's (1995) results. But why is it that there appears no learning in the 2004-2008 sample? One possible explanation could be the selection argument as outlined - people who choose to commit crime in economic good years are less productive on average than those who commit crime in economic bad years, which leaves firms a smaller margin to learn about and reward productivities during the good years. It is potentially not that firms do not learn, but that there is not much to learn. This is supported indicatively in the descriptive analysis in tables 1 and 2, where I see that the PNC sample from the later, post-economic crisis years indeed demonstrates higher employment rate and inflation-adjusted earnings. Analysis of the change in the age profile of first-time PNC entrants throughout 2004-2012 in figure 1 also shows that the distribution has significantly shifted to the right, ie there were proportionately more

[2] This is actually not observed in the descriptive analysis of my two samples - the gap in quarterly earnings between the groups increased slightly as the economic downturn took place. However, this is subject to sampling errors.

juveniles who would not have a working history and fewer adults making their first entries on PNC during the pre-crisis years than in the post-crisis years.

The conjecture that a criminal record makes more of a difference during recruitment than during employment is supported by the transition probabilities in my data. Tables 9 and 10 show the proportion of individuals moving from their current employment status into the future, with and without a historical criminal record for the two periods. I see that during both periods, currently unemployed individuals without a criminal record are more likely to get employment next quarter than those also unemployed but with a record, while currently employed individuals without a criminal record actually face slightly higher chances of losing employment in the next quarter than those currently employed with a criminal record. Logit regressions of future employment status on current employment status, possession of a criminal history and their interactions (with age, ethnicity and gender control variables) confirm that the coefficient on the interactive term, as shown in table 11, is positive and statistically significant, adding weight to the argument here that scarring is more severe at the recruitment stage and a criminal history matters less once the individual manages to find a job after the conviction.

I now dig deeper into a component of the fixed effect and investigate another dimension of firms' behaviour by testing the hypothesis of Golbe (1985) whereby a long employment history becomes a more useful signal of productivity in the presence of a criminal record than otherwise. The idea is that if building a stable employment history is more costly and harder to the offender for whatever reason, such as being under the constant influence of a criminal parent, then such a history could become a more valuable signal of his quality to employers compared against that of a non-offender. I study this by running regressions that also include a variable h on the proportion of time worked since April 2002 (the furthest back allowed by the dataset) in quadratic form and its interaction with all the conviction lags. I normalize the scale of h to between 0 to 100. If the hypothesis is true, the combination of the coefficients on h and h^2 would be such that at least for some large values of h that they benefit the convicted more than the unconvicted. Table 12 shows the results. I suppress the coefficients on the conviction variables and interactions but they are available on request. my results show that there is mixed support for the hypothesis and the general economic environment may again have a role to play. Columns 1 and 3 refer to the employment and earning regressions using the good years sample. They show first of all that without a criminal record, time previously worked improves employment outcomes at a decreasing rate. When convictions are considered, the estimated first derivatives are always lower (negative coefficients on the interaction terms) but still positive, and the second derivatives always higher. Actually, for most of the conviction lags considered in columns 1 and 3, the second derivatives are high enough relative to the first derivative that there exists a high h where it benefits the convicted more. An easy way to see this is to pick the extreme value of $h = 100$ (so $h^2 = 10000$), ie the person has worked all the time from April 2002 up till the observation period, and plug in. This supports the hypothesis that a long employment history is a more useful signal of productivity for offenders. However, when I turn my attention to the bad years sample, ie columns 2 and 4, the picture looks different. For the large majority of the conviction lags now, the coefficients on the second derivative are not large enough, even for $h = 100$, to overcome the negative coefficients on the first derivative. This means that while on an absolute term a long employment history still helps improving employment outcomes, it does not improve them by more in the presence of a conviction. This is apparent evidence against the hypothesis, and a different result from the good year sample. However, it is important to bear in mind that the hypothesis assumes equal productivity between offenders and non-offenders. If it is true that the average productivity of offenders is indeed lower than non-offenders as suggested by a simple economic model of crime, then the good year results should be understood as strong evidence for the hypothesis while the bad year results are ambiguous. The change in the results between the two samples coincides with a downturn in the economy. Could the economy be an explanatory factor? One plausible explanation is again that criminals in good economic times are more negatively selected in terms of productivitiy. Realizing this, firms may regard a long employment history as a more useful indicator of productivity than otherwise when the criminals are not as negatively selected. I leave the investigation into the exact behaviour to future research, and only document for now that the state of the economy appears also an important factor in influencing firms' behaviour in response to the interaction between criminal records and employment history.

5.6 Robustness

I present two sets of robustness test in this section. First, I try adding firms' fixed effects to the regressions. The results from this may not be informative as evidence for the statistical discrimination and learning hypothesis outlined above, but they are useful as proof against alternative hypotheses of why employment and earnings may respond negatively to a criminal record. One example would be the loss of human capital after conviction, leading to worse outcomes regardless of firms behaviour. If this alternative hypothesis is true, firms fixed effects will not make a difference to the estimated coefficients on the effect of conviction. Note that I cannot simply add firms' fixed effects to the regressions above due to the presence of unemployment (since I cannot treat it as an employer and assign a fixed effect). Instead, I restrict the sample to those who always had employment in every quarter and run only the earnings regressions. When I rerun the original regression without firms fixed effects for the subsample, I can see from table 13 that scarring effects are stronger than before in absolute magnitude. This is somewhat counter-intuitive, since this subsample being always employed is supposedly positively selected. The reason behind this peculiar observation is likely because I have excluded those who were always unemployed and whose earnings were not responsive to having a criminal record, which in turn makes the earnings margin now more sensitive. When I add the firms fixed effects, I see that most of the scarring effects are gone, including some of the effects of imprisonment. Based on this result, I can reject hypotheses of the human capital depreciation type in explaining the scarring effect.

The second robustness test I present is one of econometric specification. In the original employment regression, I specify a linear probability model with distributed lags and individual fixed effect. The issues with using linear probability model for binary employment outcome are well-known but in the presence of fixed effect standard nonlinear methods (e.g. logit or probit) has the problem of incidental parameters. Chamberlain (1980) suggests an nonlinear estimator that solves the problem at the requirement of discarding from the estimation individuals who always had the same outcome. Table 14 presents the results of applying Chamberlian's estimator to the specific required subsamples. I also provide linear regression results of the subsamples as comparison. After transforming the nonlinear estimates into odds and converting into comparable linear estimates using group means, I can see that the picture remains similar though the Chamberlain estimates are slightly larger in magnitude. I hence determine that the linear probability model does not pose a misspecification problem.

5.7 Conclusion

It has long been the popular belief that a criminal record causes large and potentially irreversible scarring to one's future employment prospects. While in cross-sectional analysis we are likely to observe large differences in employment outcomes of offenders and non-offenders, empirical research into the causal relationship between criminal record and employment prospects in general does not find huge impact, but there is no consensus on the magnitude. Existing evidence using administrative data largely points towards a temporary scarring effect, but this is somewhat conflicted by longitudinal survey or field experiment that find larger and/or more persistent impact. Using a new UK micro-dataset encompassing individuals' records in criminal justice and employment, my results represent a common ground between the previous results. I detect a persistent scarring effect of a criminal record on employment likelihood. This is consistent with the longitudinal self-report studies and is undocumented in a similar research by Grogger (1995), who finds that all scarring vanishes for criminal records over 6 quarters old. The scarring gap I identify while persistent is not considered to be overly large. The effect differs by punishment type at first, and at the most severe I find an over 30% decrease in employment likelihood from the mean employment rate for an imprisonment record, but it fades over time and eventually after 10 quarters, regardless of punishment type the scarring on employment is down to about 5-10% of the mean. On earnings, I detect a diminishing scarring effect in the post-crisis years, much like Grogger's (1995) finding but lasts up to the ninth quarter after conviction. The scarring on earnings appear to more persistent, however, during the pre-crisis years. I attribute the employment scarring to a statistical discrimination hypothesis and the diminishing scarring on earnings is consistent with an employer learning hypothesis. I note a difference in the results between the good and bad economic years, specfically that apparently there is no learning in the post-crisis years. Descriptive analysis suggests that this could be due to a change in the composition of the offenders population, in particular that criminals in a bad economic environment are of higher productivity on average and at the margin. I also test a hypothesis where a long employment history becomes a more valuable signal of productivity in the presence of a criminal record and find mixed support. The state of the economy could well again play a part in influencing how firms view the usefulness of employment history as a signal in the presence of a criminal record.

What lessons or implications does this study have for policy? The bigger question on the topic of scarring is whether it is well balanced (ie "optimal") between acting as a crime deterrent to stop one from becoming an offender, while not ruling out the rehabilitation opportunities for ex-offenders to reintegrate into society properly and not fall back into a career of crime due to restricted life choices. My results suggest that on the latter part of rehabilitation, a criminal record is certainly not detrimental to future employment prospects. I cannot judge by the results here alone whether the current level of scarring is optimal or if it is currently effective as a crime deterrent. These questions, as well as those on what would an optimal level of scarring be and how it can be achieved, would be very useful avenues for future economic and social research.

Table 5.1: Quarterly averages of the analysis sample during Apr 2004 - Mar 2007

	Treatment group	Control group	Overall
Data rows	1,009,728	299,772	1,309,500
Number of individuals	84,144	24,981	109,125
Employment	44.00%	43.74%	43.95%
Earnings	£1773	£1864	£1794
Conviction rate	9.68%	-	7.47%
Caution	4.06%	-	3.13%
Fine	2.79%	-	2.15%
Probation	1.43%	-	1.10%
Prison	0.35%	-	0.28%
Others	1.05%	-	0.81%
Violence	0.87%	-	0.67%
Male	75.75%	74.64%	75.49%
White	78.50%	77.60%	78.29%
Age (quarters)	135.91	136.57	136.06

Table 5.2: Quarterly averages of the analysis sample during Oct 2008 - Sep 2011

	Treatment group	Control group	Overall
Data rows	1,042,032	251,988	1,294,020
Number of individuals	86,836	20,999	107,835
Employment	49.46%	50.11%	49.58%
Earnings	£1818	£1934	£1840
Conviction rate	9.66%	-	7.78%
Caution	4.29%	-	3.46%
Fine	2.38%	-	1.91%
Probation	1.62%	-	1.30%
Prison	0.36%	-	0.29%
Others	1.01%	-	0.81%
Violence	0.75%	-	0.61%
Male	73.66%	73.00%	73.53%
White	77.11%	76.33%	76.96%
Age (quarters)	138.27	140.54	138.71

Table 5.3: Baseline effect of convictions on employment outcomes, 2004-2007

Conviction lag:	(1) Employment OLS	(2) Earnings OLS	(3) Employment FE	% of mean	(4) Earnings FE	% of mean
0	-0.062**	-591**	-0.014*	-3.2%	-116**	-6.5%
	(0.005)	(25)	(0.004)		(17)	
1	-0.067**	-563**	-0.024**	-5.2%	-106**	-5.9%
	(0.005)	(41)	(0.004)		(35)	
2	-0.060**	-522**	-0.023**	-5.0%	-90**	-5.0%
	(0.005)	(44)	(0.004)		(38)	
3	-0.056**	-475**	-0.024**	-5.2%	-71*	-3.9%
	(0.005)	(49)	(0.005)		(42)	
4	-0.047**	-449**	-0.021**	-4.6%	-73	-4.0%
	(0.006)	(55)	(0.005)		(45)	
5	-0.053**	-518**	-0.027**	-5.9%	-110**	-6.1%
	(0.006)	(34)	(0.006)		(31)	
6	-0.047**	-503**	-0.027**	-5.9%	-115**	-6.4%
	(0.007)	(37)	(0.006)		(32)	
7	-0.051**	-465**	-0.037**	-8.4%	-100**	-5.6%
	(0.008)	(41)	(0.007)		(35)	
8	-0.042**	-407**	-0.04**	-10.0%	-114**	-6.4%
	(0.009)	(49)	(0.007)		(39)	
9	-0.047**	-449**	-0.045**	-10.0%	-117**	-6.5%
	(0.011)	(57)	(0.008)		(44)	
10	-0.035**	-388**	-0.035**	-7.7%	-92*	-5.1%
	(0.013)	(71)	(0.009)		(51)	
N	1,309,500	1,309,500	1,309,500		1,309,500	

Standard errors in parenthesis. ** $p<0.05$, * $p<0.1$. The set of control covariates includes interactions of the binary conviction variable with punishment type and crime type indicator for lags of up to 10 quarters, age, age squared, gender and ethnicity. Gender and ethnicity drop out of the fixed effect estimation.

Table 5.4: Heterogenous effects on employment, 2004-2007

			The effects of being sentenced to:									
Conviction lag:	Baseline	% of mean	Police caution	% of mean	Fine	% of mean	Probation	% of mean	Prison	% of mean	Violent Crime	% of mean
0	-0.014*	-3.2%	-0.003	-0.7%	0.001	0.2%	-0.025**	-5.7%	-0.054**	-12.3%	-0.014**	-3.2%
	(0.004)		(0.002)		(0.002)		(0.003)		(0.006)		(0.005)	
1	-0.024**	-5.2%	-0.014**	-3.2%	-0.011**	-2.5%	-0.035**	-8.0%	-0.127**	-28.9%	-0.025**	-5.7%
	(0.004)		(0.002)		(0.003)		(0.004)		(0.007)		(0.006)	
2	-0.023**	-5.0%	-0.016**	-3.6%	-0.017**	-3.9%	-0.037**	-8.4%	-0.102**	-23.2%	-0.026**	-5.9%
	(0.004)		(0.003)		(0.003)		(0.004)		(0.008)		(0.007)	
3	-0.024**	-5.2%	-0.017**	-3.9%	-0.021**	-4.8%	-0.039**	-8.9%	-0.078**	-17.7%	-0.03**	-6.8%
	(0.005)		(0.003)		(0.003)		(0.004)		(0.008)		(0.007)	
4	-0.021**	-4.6%	-0.019**	-4.3%	-0.024**	-5.5%	-0.04**	-9.1%	-0.061**	-13.9%	-0.029**	-6.6%
	(0.005)		(0.003)		(0.003)		(0.005)		(0.009)		(0.008)	
5	-0.027**	-5.9%	-0.021**	-4.8%	-0.026**	-5.9%	-0.041**	-9.3%	-0.062**	-14.1%	-0.027**	-6.1%
	(0.006)		(0.003)		(0.004)		(0.005)		(0.01)		(0.008)	
6	-0.027**	-5.9%	-0.025**	-5.7%	-0.031**	-7.1%	-0.043**	-9.8%	-0.047**	-10.7%	-0.029**	-6.6%
	(0.006)		(0.004)		(0.004)		(0.006)		(0.01)		(0.009)	
7	-0.037**	-8.4%	-0.03**	-6.8%	-0.037**	-8.4%	-0.044**	-10.0%	-0.04**	-9.1%	-0.042**	-9.6%
	(0.007)		(0.004)		(0.004)		(0.006)		(0.011)		(0.01)	
8	-0.04**	-10.0%	-0.034**	-7.7%	-0.041**	-9.3%	-0.047**	-10.7%	-0.052**	-11.8%	-0.046**	-10.5%
	(0.007)		(0.004)		(0.005)		(0.007)		(0.012)		(0.011)	
9	-0.045**	-10.0%	-0.036**	-8.2%	-0.04**	-9.1%	-0.053**	-12.1%	-0.047**	-10.7%	-0.044**	-10.0%
	(0.008)		(0.005)		(0.005)		(0.008)		(0.014)		(0.012)	
10	-0.035**	-7.7%	-0.036**	-8.2%	-0.042**	-9.6%	-0.052**	-11.8%	-0.033**	-7.5%	-0.036**	-8.2%
	(0.009)		(0.005)		(0.006)		(0.009)		(0.017)		(0.015)	
N						1,309,500						

Standard errors in parenthesis. ** $p<0.05$, * $p<0.1$. The set of control covariates includes interactions of the binary conviction variable with punishment type and crime type indicator for lags of up to 10 quarters, age, age squared, gender and ethnicity. Gender and ethnicity drop out of the fixed effect estimation.

Table 5.5: Heterogenous effects on earnings, 2004-2007

			The additional effects of being sentenced to:									
Conviction lag:	Baseline	% of mean	Police caution	% of mean	Fine	% of mean	Probation	% of mean	Prison	% of mean	Violent Crime	% of mean
0	-116**	-6.5%	-94**	-5.2%	-24	-1.3%	-181**	-10.1%	-437**	-24.4%	-100**	-5.6%
	(17)		(21)		(21)		(17)		(32)		(32)	
1	-106**	-5.9%	-125**	-7.0%	-100**	-5.6%	-214**	-11.9%	-657**	-36.6%	-97**	-5.4%
	(35)		(27)		(23)		(19)		(37)		(46)	
2	-90**	-5.0%	-108**	-6.0%	-97**	-5.4%	-217**	-12.1%	-585**	-32.6%	-104**	-5.8%
	(38)		(29)		(24)		(21)		(39)		(50)	
3	-71*	-3.9%	-81**	-4.5%	-112**	-6.2%	-189**	-10.5%	-476**	-26.5%	-107**	-6.0%
	(42)		(26)		(26)		(23)		(42)		(52)	
4	-73	-4.0%	-81**	-4.5%	-109**	-6.1%	-208**	-11.6%	-427**	-23.8%	-115**	-6.4%
	(45)		(28)		(29)		(24)		(45)		(54)	
5	-110**	-6.1%	-70**	-3.9%	-120**	-6.7%	-209**	-11.6%	-370**	-20.6%	-115**	-6.4%
	(31)		(24)		(32)		(27)		(47)		(45)	
6	-115**	-6.4%	-52*	-2.9%	-117**	-6.5%	-213**	-11.9%	-299**	-16.7%	-162**	-9.0%
	(32)		(27)		(37)		(30)		(51)		(48)	
7	-100**	-5.6%	-69**	-3.8%	87	4.8%	-183**	-10.2%	-229**	-12.8%	-168**	-9.4%
	(35)		(31)		(133)		(33)		(56)		(55)	
8	-114**	-6.4%	-101**	-5.6%	153	8.5%	-235**	-13.1%	-224**	-12.5%	-150**	-8.4%
	(39)		(34)		(168)		(37)		(62)		(61)	
9	-117**	-6.5%	-101**	-5.6%	240	13.4%	-240**	-13.4%	-189**	-10.5%	-171**	-9.5%
	(44)		(38)		(221)		(43)		(71)		(70)	
10	-92*	-5.1%	-98**	-5.5%	329	18.3%	-232**	-12.9%	-82	-4.6%	-91	-5.1%
	(51)		(42)		(308)		(51)		(83)		(81)	
N					1,309,500							

Standard errors in parenthesis. ** $p<0.05$, * $p<0.1$. The set of control covariates includes interactions of the binary conviction variable with punishment type and crime type indicator for lags of up to 10 quarters, age, age squared, gender and ethnicity. Gender and ethnicity drop out of the fixed effect estimation.

Table 5.6: Baseline effect of convictions on employment outcomes, 2008-2011

Conviction lag:	(1) Employment OLS	(2) Earnings OLS	(3) Employment FE	% of mean	(4) Earnings FE	% of mean
0	-0.072**	-558**	-0.020**	-4.0%	-125**	-6.8%
	(0.005)	(26)	(0.004)		(15)	
1	-0.077**	-545**	-0.031**	-6.3%	-142**	-7.7%
	(0.005)	(28)	(0.004)		(18)	
2	-0.072**	-520**	-0.033**	-6.6%	-130**	-7.1%
	(0.005)	(28)	(0.004)		(19)	
3	-0.066**	-498**	-0.034**	-6.8%	-120**	-6.5%
	(0.005)	(30)	(0.005)		(20)	
4	-0.058**	-488**	-0.030**	-6.1%	-107**	-5.8%
	(0.006)	(32)	(0.005)		(22)	
5	-0.052**	-459**	-0.029**	-5.8%	-86**	-4.7%
	(0.006)	(35)	(0.006)		(23)	
6	-0.048**	-460**	-0.030**	-6.1%	-83**	-4.5%
	(0.007)	(39)	(0.006)		(25)	
7	-0.040**	-414**	-0.031**	-6.3%	-55**	-3.0%
	(0.008)	(45)	(0.007)		(28)	
8	-0.033**	-403**	-0.033**	-6.7%	-51*	-2.8%
	(0.009)	(55)	(0.008)		(31)	
9	-0.040**	-367**	-0.043**	-8.6%	-60*	-3.2%
	(0.011)	(71)	(0.008)		(34.94)	
10	-0.047**	-335**	-0.052**	-10.6%	-66	-3.6%
	(0.014)	(77)	(0.010)		(43.05)	
N	1,294,020	1,294,020	1,294,020		1,294,020	

Standard errors in parenthesis. ** $p<0.05$, * $p<0.1$. The set of control covariates includes interactions of the binary conviction variable with punishment type and crime type indicator for lags of up to 10 quarters, age, age squared, gender and ethnicity. Gender and ethnicity drop out of the fixed effect estimation.

Table 5.7: Heterogenous effects on employment, 2008-2011

Conviction lag:	Baseline	% of mean	Police caution	% of mean	Fine	% of mean	Probation	% of mean	Prison	% of mean	Violent Crime	% of mean
			The additional effects of being sentenced to:									
0	-0.020**	-4.0%	0.001	0.2%	0.008**	1.6%	-0.024**	-4.8%	-0.063**	-12.7%	-0.022**	-4.4%
	(0.004)		(0.002)		(0.002)		(0.003)		(0.007)		(0.005)	
1	-0.031**	-6.3%	-0.014**	-2.8%	-0.011**	-2.2%	-0.039**	-7.9%	-0.16**	-32.3%	-0.033**	-6.7%
	(0.004)		(0.002)		(0.003)		(0.003)		(0.008)		(0.006)	
2	-0.033**	-6.6%	-0.013**	-2.6%	-0.02**	-4.0%	-0.041**	-8.3%	-0.152**	-30.7%	-0.035**	-7.1%
	(0.004)		(0.002)		(0.003)		(0.004)		(0.009)		(0.007)	
3	-0.034**	-6.8%	-0.012**	-2.4%	-0.026**	-5.2%	-0.038**	-7.7%	-0.13**	-26.2%	-0.034**	-6.9%
	(0.005)		(0.003)		(0.003)		(0.004)		(0.009)		(0.007)	
4	-0.030**	-6.1%	-0.015**	-3.0%	-0.029**	-5.9%	-0.042**	-8.5%	-0.125**	-25.2%	-0.027**	-5.4%
	(0.005)		(0.003)		(0.003)		(0.004)		(0.01)		(0.008)	
5	-0.029**	-5.8%	-0.015**	-3.0%	-0.03**	-6.1%	-0.039**	-7.9%	-0.113**	-22.8%	-0.035**	-7.1%
	(0.006)		(0.003)		(0.004)		(0.005)		(0.01)		(0.008)	
6	-0.030**	-6.1%	-0.016**	-3.2%	-0.035**	-7.1%	-0.038**	-7.7%	-0.107**	-21.6%	-0.036**	-7.3%
	(0.006)		(0.003)		(0.004)		(0.005)		(0.011)		(0.009)	
7	-0.031**	-6.3%	-0.018**	-3.6%	-0.034**	-6.9%	-0.04**	-8.1%	-0.095**	-19.2%	-0.04**	-8.1%
	(0.007)		(0.003)		(0.004)		(0.005)		(0.012)		(0.01)	
8	-0.033**	-6.7%	-0.019**	-3.8%	-0.035**	-7.1%	-0.035**	-7.1%	-0.064**	-12.9%	-0.042**	-8.5%
	(0.008)		(0.004)		(0.005)		(0.006)		(0.013)		(0.011)	
9	-0.043**	-8.6%	-0.023**	-4.6%	-0.034**	-6.9%	-0.038**	-7.7%	-0.046**	-9.3%	-0.046**	-9.3%
	(0.008)		(0.004)		(0.005)		(0.007)		(0.016)		(0.012)	
10	-0.052**	-10.6%	-0.023**	-4.6%	-0.042**	-8.5%	-0.042**	-8.5%	-0.068**	-13.7%	-0.045**	-9.1%
	(0.010)		(0.005)		(0.006)		(0.008)		(0.018)		(0.014)	
N					1,294,020							

Standard errors in parenthesis. ** p<0.05, * p<0.1. The set of control covariates includes interactions of the binary conviction variable with punishment type and crime type indicator for lags of up to 10 quarters, age, age squared, gender and ethnicity. Gender and ethnicity drop out of the fixed effect estimation.

Table 5.8: Heterogenous effects on earnings, 2008-2011

			The additional effects of being sentenced to:											
Conviction lag:	Baseline	% of mean	Police caution	% of mean	Fine	% of mean	Probation	% of mean	Prison	% of mean	Violent Crime	% of mean
0	-125**	-6.8%	-66**	-3.6%	-18	-1.0%	-203**	-11.0%	-463**	-25.2%	-102**	-5.5%
	(15)		(11)		(15)		(15)		(30)		(38)	
1	-142**	-7.7%	-63	-3.4%	-102**	-5.5%	-251**	-13.6%	-750**	-40.8%	-189**	-10.3%
	(18)		(42)		(24)		(17)		(37)		(51)	
2	-130**	-7.1%	-76**	-4.1%	-122**	-6.6%	-230**	-12.5%	-678**	-36.8%	-162**	-8.8%
	(19)		(15)		(20)		(17)		(39)		(50)	
3	-120**	-6.5%	-55**	-3.0%	-118**	-6.4%	-189**	-10.3%	-578**	-31.4%	-164**	-8.9%
	(20)		(17)		(24)		(24)		(40)		(50)	
4	-107**	-5.8%	-56**	-3.0%	-111**	-6.0%	-180**	-9.8%	-526**	-28.6%	-161**	-8.8%
	(22)		(18)		(26)		(19)		(41)		(54)	
5	-86**	-4.7%	-44**	-2.4%	-113**	-6.1%	-166**	-9.0%	-468**	-25.4%	-145**	-7.9%
	(23)		(19)		(26)		(21)		(42)		(59)	
6	-83**	-4.5%	-40*	-2.2%	-73**	-4.0%	-133**	-7.2%	-437**	-23.8%	-122**	-6.6%
	(25)		(21)		(26)		(22)		(43)		(55)	
7	-55**	-3.0%	2	0.1%	-37	-2.0%	-111**	-6.0%	-354**	-19.2%	-127**	-6.9%
	(28)		(32)		(36)		(25)		(46)		(62)	
8	-51*	-2.8%	10	0.5%	-52*	-2.8%	-65**	-3.5%	-250**	-13.6%	-137**	-7.4%
	(31)		(38)		(31)		(31)		(54)		(62)	
9	-60*	-3.2%	-45	-2.4%	-40	-2.2%	-68*	-3.7%	-159**	-8.6%	-138**	-7.5%
	(34.94)		(28)		(33)		(37)		(66)		(67)	
10	-66	-3.6%	-10	-0.5%	-21	-1.1%	-99**	-5.4%	-208**	-11.3%	-154*	-8.4%
	(43.05)		(50)		(41)		(37)		(87)		(88)	
N					1,294,020							

Standard errors in parenthesis. ** p<0.05, * p<0.1. The set of control covariates includes interactions of the binary conviction variable with punishment type and crime type indicator for lags of up to 10 quarters, age, age squared, gender and ethnicity. Gender and ethnicity drop out of the fixed effect estimation.

Table 5.9: Employment transition probabilities, 2004-2007

Current period:		Next period: Unemployed	Employed	Row total
Unemployed	With criminal history	199,771	13,436	213,207
		(93.70%)	(6.30%)	(100%)
	Without criminal hitory	367,197	28,319	395,516
		(92.84%)	(7.16%)	(100%)
Employed	With criminal history	13,919	155,545	169,464
		(8.21%)	(91.79%)	(100%)
	Without criminal hitory	26,645	286,409	313,063
		(8.51%)	(91.49%)	(100%)

Table 5.10: Employment transition probabilities, 2008-2011

Current period:		Next period: Unemployed	Employed	Row total
Unemployed	With criminal history	193,335	18,662	211,997
		(91.20%)	(8.80%)	(100%)
	Without criminal hitory	299,379	32,932	332,311
		(90.09%)	(9.91%)	(100%)
Employed	With criminal history	14,446	195,935	210,381
		(6.87%)	(93.13%)	(100%)
	Without criminal hitory	23,545	300,116	323,661
		(7.27%)	(92.73%)	(100%)

Table 5.11: Logit regressions of future employment on current employment, criminal history and their interaction

	(1)	(2)
Current Employment	5.002**	4.840**
	(0.009)	(0.009)
Criminal history	-0.111**	-0.089**
	(0.011)	(0.010)
Employment x Criminal History	0.169**	0.168**
	(0.015)	(0.015)
Sample	04-07	08-11
N	1,091,250	1,078,350

Standard errors in parenthesis. ** $p<0.05$, * $p<0.1$. Both logit regressions. The set of control covariates includes age, age squared, ethnicity, gender and quarter indicators.

Table 5.12: Effect of employment history on employment and earnings

		(1)	(2)	(3)	(4)
Outcome:		Employment		Earnings	
Baseline estimates	h	1.88**	1.62**	76.90**	41.89**
	h^2	-0.0113**	-0.008**	-0.402**	0.0906**
Interacted with conviction lag:					
0	h	-0.150**	-0.0527**	-15.66**	-7.060**
	h^2	0.00155**	0.000275*	0.143**	0.0178
1	h	-0.148**	-0.140**	-13.77**	-10.34**
	h^2	0.00174**	0.000989**	0.138**	0.0471**
2	h	-0.174**	-0.149**	-13.45**	-9.370**
	h^2	0.00277**	0.00103**	0.150**	0.0276
3	h	-0.199**	-0.135**	-14.32**	-10.06**
	h^2	0.0027**	0.000974**	0.178**	0.0400**
4	h	-0.222**	-0.129**	-14.61**	-11.18**
	h^2	0.00301**	0.000993**	0.192**	0.0530**
5	h	-0.209**	-0.111**	-16.37**	-9.841**
	h^2	0.00305**	0.000874**	0.218**	0.0448**
6	h	-0.222**	-0.133**	-16.51**	-8.452**
	h^2	0.00337**	0.00109**	0.225**	0.0331
7	h	-0.229**	-0.112**	-16.23**	-6.405**
	h^2	0.00352**	0.00104**	0.258**	0.0216
8	h	-0.256**	-0.0866**	-17.01**	-6.316**
	h^2	0.00375**	0.000957**	0.288**	0.0242
9	h	-0.256**	-0.105**	-18.64**	-6.118**
	h^2	0.00394**	0.00123**	0.326**	0.0201
10	h	-0.296**	-0.0720**	-24.22**	-1.862
	h^2	0.00444**	0.000847**	0.414**	-0.0108
Sample		04-07	08-11	04-07	08-11
N		1,309,500	1,294,020	1,309,500	1,294,020

Standard errors not reported. ** $p<0.05$, * $p<0.1$. All OLS estimations. The set of control covariates includes interactions of the binary conviction variable with punishment type and crime type indicator for lags of up to 10 quarters, age, age squared, ethnicity and gender.

Table 5.13: Effects on earnings for those who always worked, with and without firms fixed effect

	(1)		(2)		(3)		(4)	
Conviction lag:	Baseline	Prison	Baseline	Prison	Baseline	Prison	Baseline	Prison
0	-166**	-981**	-55	-863**	-110**	-752.6**	-44	-526**
	(58)	(136)	(65)	(135)	(43)	(137.7)	(39)	(110)
1	-17	-946**	167	-1084**	-119**	-965.4**	-2	-561**
	(191)	(140)	(229)	(149)	(54)	(154.2)	(47)	(113)
2	30	-725**	211	-705**	-139**	-566.0**	-21	-214**
	(206)	(143)	(253)	(151)	(50)	(154.3)	(40)	(104)
3	117	-583**	258	-558**	-141**	-416.7**	-7	-153*
	(222)	(149)	(274)	(141)	(53)	(146.5)	(47)	(86)
4	149	-524**	298	-329**	-134**	-194.9	-10	-20
	(236)	(158)	(289)	(143)	(62)	(150.6)	(54)	(83)
5	39	-325*	184	-298**	-128**	-170.6	2	-11
	(123)	(168)	(166)	(133)	(62)	(138.5)	(49)	(86)
6	-5	-436**	153	-275**	-121*	-153.7	-16	-17
	(129)	(170)	(168)	(132)	(65)	(136.1)	(59)	(89)
7	-1	-211	133	-217	-47	-170.3	2	144
	(134)	(180)	(171)	(178)	(71)	(182.4)	(64)	(119)
8	-3	-60	132	-224	-26	-197.4	-5	156
	(138)	(180)	(173)	(198)	(81)	(203.8)	(70)	(138)
9	-77	-136	86	87	-22	109.8	-37	435
	(152)	(212)	(177)	(300)	(90)	(304.3)	(76)	(296)
10	-84	77	85	484	35	448.9	50	584
	(165)	(235)	(182)	(489)	(103)	(495.1)	(83)	(445)
Firms FE	No		Yes		No		Yes	
Sample	04-07		04-07		08-11		08-11	
N	280,860		280,860		320,592		320,592	

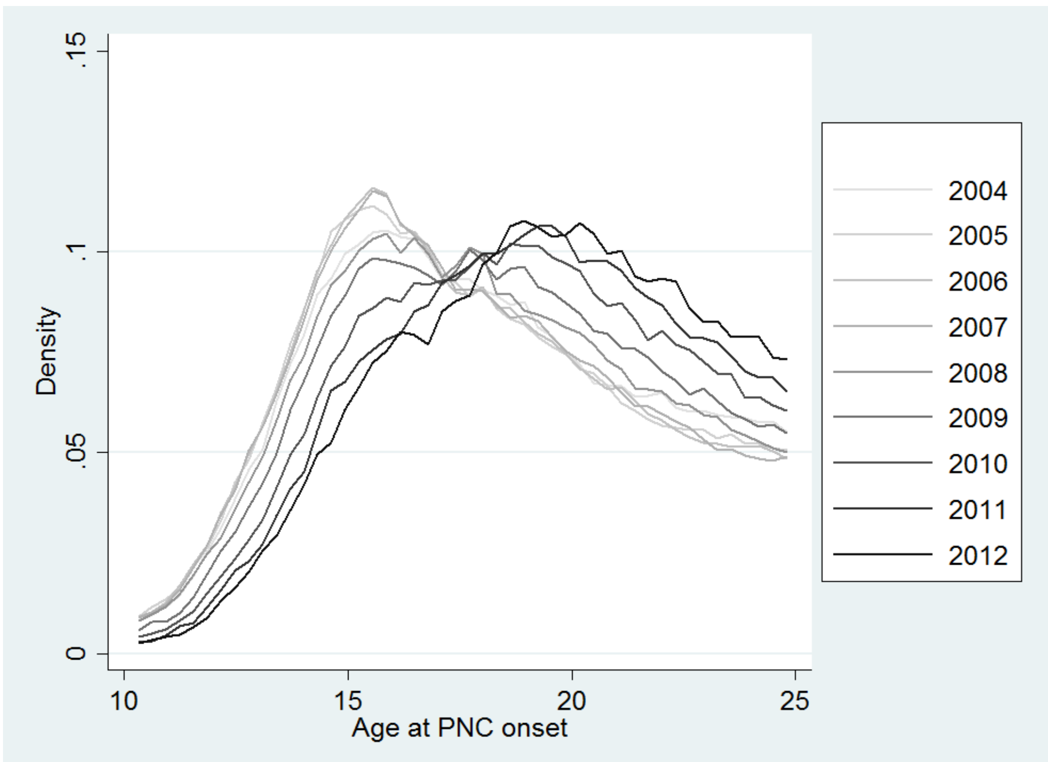
Standard errors in parenthesis. ** $p<0.05$, * $p<0.1$. All fixed effect estimation. The set of control covariates includes interactions of the binary conviction variable with punishment type and crime type indicator for lags of up to 10 quarters, age, age squared.

Table 5.14: Chamberlain's (1980) nonlinear estimates

	(1)		(2)		(3)		(4)	
Conviction lag:	Baseline	Prison	Baseline	Prison	Baseline	Prison	Baseline	Prison
0	-0.038**	-0.141**	-0.031**	-0.119**	-0.054**	-0.161**	-0.041**	-0.124**
1	-0.065**	-0.289**	-0.053**	-0.277**	-0.083**	-0.345**	-0.064**	-0.306**
2	-0.06**	-0.244**	-0.050**	-0.224**	-0.088**	-0.334**	-0.066**	-0.293**
3	-0.063**	-0.192**	-0.052**	-0.169**	-0.092**	-0.302**	-0.070**	-0.257**
4	-0.053**	-0.155**	-0.044**	-0.134**	-0.083**	-0.292**	-0.062**	-0.248**
5	-0.069**	-0.157**	-0.057**	-0.136**	-0.079**	-0.271**	-0.059**	-0.225**
6	-0.068**	-0.121**	-0.059**	-0.103**	-0.078**	-0.26**	-0.059**	-0.215**
7	-0.096**	-0.101**	-0.082**	-0.085**	-0.083**	-0.237**	-0.062**	-0.196**
8	-0.113**	-0.129**	-0.097**	-0.110**	-0.09**	-0.174**	-0.068**	-0.141**
9	-0.116**	-0.114**	-0.097**	-0.098**	-0.11**	-0.134**	-0.084**	-0.101**
10	-0.089**	-0.071**	-0.075**	-0.060**	-0.132**	-0.189**	-0.100**	-0.158**
Method	Nonlinear		Linear		Nonlinear		Linear	
Sample	04-07		04-07		08-11		08-11	
N	280,860		280,860		320,592		320,592	

Standard errors not reported. ** $p<0.05$, * $p<0.1$. Nonlinear estimations are done according to Chamberlain's (1980) conditional logit estimator. Linear estimations are FE regressions. The set of control covariates includes interactions of the binary conviction variable with punishment type and crime type indicator for lags of up to 10 quarters, age, age squared.

Figure 5.1: Trends in the age profile of under-25 first-time PNC entrants 2004-2012



Shown above are trends in the relative, not absolute, distribution of the age of under-25 first-time PNC entrants during 2004-2012. The area under curve for each year is normalized to 1.

Chapter 6

After Prison - the natural experiment of England riots 2011

6.1 Introduction

Incarceration is a prominent feature of any criminal justice system. According to the latest edition of World Prison Population List, in 2016 there were more than 10.35 million people held in penal institutions. The global prison population rate is estimated to be 144 per 100,000. This is a sizable number. Furthermore, the growth of prison population since 2000 is estimated to have outpaced that of global population by 2% point. Take England and Wales as an example. The prison population in mid 1993 stood at about 44,000. At the beginning of 2017, the number has increased almost twofold to about 85,000 (MoJ 2016b, 2017b). At the same time, the use of less punitive punishment such as fines have come down, alongside a general decrease in the number of individuals entering the criminal justice system (MoJ 2014b). What has driven this movement towards a more active and punitive approach? It is unlikely to be a story of demand. Over the same period there was a mild decrease in police recorded crime and a much sharper fall in Crime Survey estimates of total number of crime (excluding fraud and misuse of computer). While there is evidence of an increase in the severity of crime towards more violence, but the scale was different - the prevalence rate according to the Crime Survey has gone up from 18% to just under 21%. Also, within violent crime the composition has shifted towards more non-injury crime, from 37% to 50% (ONS 2017). However, these mild trends in reported violence are not fully transferred to entries into the criminal justice system. For example, between 1999 and 2010 the proportion of violence out of all offences brought to justice has gone up from 20% to 29% (MoJ 2012a). Clearly, the increase of 100% in prison population over a 24-year period is out of scale with the underlying trends in crime. It is likely that the dramatic movement is at least partly driven by a shift in policy and attitude towards believing that the benefits of incarceration as a punishment outweight the costs. Indeed it might be tempting just looking at the figures to view the decrease in crime as a winner for the incarceration policy - crime has drastically decreased after all. However, the crime trend is in fact observed in many of the advanced economies, despite a variety of approaches to incarceration, and is likely part of a global development rather than a direct consequence of the UK incarceration policy , as can be see in table 1.

This brings the questions of what the policy rationales of such drastic incarceration are, and what effects it can achieve. Around the world there is no consensus on the right approach with regards to prison as seen by the variance in the levels of prison population per capita and the trends. The US is notorious for its prison policy, having the globally second highest per capita incarceration rate of about 700 per 100,000. In contrast, Europe, especially Scandinavia, has a much lower rate and takes a much less punitive approach. The commonly discussed benefits of incarceration in the literature, as discussed in chapter 2, are general deterrence (Becker 1968), specific deterrence (Smith and Gartin 1989), incapacitation (Ehrlich 1981) and rehabilitation (Ehrlich 1981). On the other hand, compared to non-custodial sentence the drawbacks are stigma (Rasmusen 1996), loss of human capital (Ehrlich 1981, Lochner 2004), loss of social capital (Sickles and Williams 2008), build-up of criminal capital and network (Bayer et al 2009) and disruption to life courses in general. Of course, prisons are also much more expensive to run than its criminal justice alternatives. In 2016, the estimated average annual cost of a prison place in England and Wales stood at £35,182 (MoJ, 2016a).

A large part of the cost-benefit analysis, as can be seen in above discussion, is focussed on the effect of incarceration on later outcomes of inmates. Imprisonment can reduce reoffending via specific deterrence, ie prisoners are deterred from future crime due to the negative experience in prison, and also through a positive rehabilitative effect on labour market outcomes if the right training and help is given while in prison. But the more time they spend in prison they may be more likely to be discriminated against by propsective employers either directly or statistically. Official figures from England and Wales in 2013 show that the one-year recidivism rates of offenders receiving the disposals of discharge, community order, fine and imprisonment are 33.4%, 34.3%, 28.9% and 45.8%. These numbers suggest prisons caused the worst outcome but of course they mean little without controlling for other factors and selection. An increasing body of empirical evidence is emerging from the literature on the effect of prisons on later outcomes. Largely there are two approaches in overcoming the issue of selection. Matching methods are employed by social scientists and they tend to find significant negative effects of imprisonment. Nieuwbeerta et al. (2009) using data from a longitudinal study in the Netherlands find that first time imprisonment increases recidivism by 1.9 times over the 3 years after release. Apel and Sweeten (2010) using the American National Longitudinal Survey of Youth 1997 data find that first time imprisonment significantly reduces the probability of employment compared to non-custodial punishment, even if the spell of incarceration is only a few months long. They show evidence that the gap is due to ex-inmates not looking for jobs rather than being unable to get jobs, suggesting a human capital story rather than a stigma one. On the other hand, economists typically take an alternative approach in tackling selection. A very common strategy is to utilise randomisation of court cases to judges as quasi-experiment, since different judges demonstrate different levels of intrinsic harshness. Recent examples include Kling (2006), Green and Winik (2010), Di Tella and Schargrodsky (2013), Nagin and Snodgrass (2013), Loeffler (2013), Aizer and Doyle (2015), Mueller-Smith (2015) and Bhuller et. al (2016). In general this literature finds that custody or sentence length has little effect on later reoffending and employment outcomes. This is perhaps not overly surprisingly given the well-known problem of severe self-selection in criminal justice. There are several exceptions, however. Di Tella and Schargrodsky (2013) focus on juveniles in particular and find that incarceration causes significantly worse outcomes than the softer alternative of electronic monitoring. Aligning this with the majority of the empirical evidence suggests that individual criminal fixed effects are perhaps not fixed until adulthood. Mueller-Smith (2015) argue that the popular approach of using judge randomisation as instrument suffers from the assumption of monotonicity and exclusion, and shows that bias can result if they are violated. He proposes an improved estimation procedure that takes into account of this and finds that prisons are criminogenic instead of having no effects. Another result that deviates from the norm is that of Bhuller et.al (2016), where it is found that incarceration can greatly improve reoffending and employment outcomes. This may, however, be a unique result specific to the setting in Norway which is where their study is based and where the approach to incarceration is one of the most pro-rehabilitation in the world. It is reported in their study that "imprisonment causes a 34 percentage point increase in participation in job training programs for the previously nonemployed, and within 5 years, their employment rate increases by 40 percentage points", which is no doubt credible but nonetheless strikes as an anomaly given the policy setting.

I contribute to the literature by taking a different approach in this research to tackle the same problem. Instead of utilising judge-level randomisation of court cases as the quasi-experiment, which can have issues with judges not necessarily being highly self-consistent through time, I use the random event of the England riots of August 2011 as an instrument for the endogenous prison outcome. My hypothesis is that during the immediate period after the riot, which caught global attention and attracted very swift and harsh criminal justice reaction (MoJ 2012b, Bell et al 2014), judges in the riot areas became harsher towards offenders who had nothing to do with the riots but merely committed offences similar to the riot offences. The first stage difference-in-differences regression confirms this - the riots appear to have exogenously shifted judges' sentiment towards some offences in the riot areas, creating a temporary random shock in the probability of handing out imprisonment as the disposal. This makes the riots a valid instrument in identifying the effect of incarceration on outcomes. Using Instrumental Variable, I find that incarceration induces very short-lived specific deterrence effect as theorised but it fades away after 6 months and gives way to criminogenic factors. There is no significant effect on employment at least within one year. This may be due to the lack of variation within the relatively small sample but if anything the sign of the estimated coefficient suggests the effect is more positive than negative. The analysis also shows that prior employment record explains quite a lot of the variation in post-custody outcomes, suggesting fixed effects at the point of prison entry is important.

The rest of the chapter is structured as followed. Section 2 provides the background to the England Riots and the subsequent criminal justice response that makes it a valid instrument. Section 3 describes the empirical strategy. Section 4 discusses the results and finally section 5 concludes.

6.2 England Riots 2011

The England Riots of 2011 were not anticipated and the scale at which they escalated and the contagion were a surprise. It is reasonable to expect that they were a shock to the judicial and criminal justice system as recent history of England suggests that rioting at that level and scale is uncommon. The last time England saw widespread disturbances on a similar scale was in the 1980's. In 1981, riots happened in Brixton, London in April and there were further riots in Liverpool, Leeds and Birmingham in July, leaving hundreds injured. Later in late 1985, another wave of riots, each had a different local trigger, took place across Brixton, Tottenham (both London) and Birmingham. Most of these riots were initially sparked by conflicts between the local black community and the police, against a backdrop of poverty, deprivation, high unemployment, racial tension and inequality. Since then, nationwide riots were unseen but local ones did take place occasionally, for example in Brixton again in 1995 and Leeds in 2001. It is therefore fair to say that dealing with riot offences is not commonplace in recent English courts and the riots in 2011 came as an exogenous event.

Similar to most other riots in modern English history, the starting point of the one in 2011 was a conflict between the local black community and the police. On 4 August 2011, 29-year-old Mark Duggan's vehicle was stopped by a police officer near Tottenham Hale Station in North London as part of an intelligence-led stop-and-search procedure to investigate gun crime within the local black community. During the incident, Duggan who was in possession of a gun that he did not fire was shot and later died. Later after a lengthy inquiry, the killing of Duggan was found to be lawful but during the immediate aftermath, the local community was dissatisfied with the police response and a protest march organised by relatives and friends of Duggan demanding justice took place 2 days later, starting from Broadwater Farm, where a riot took place in 1985, and fnishing at the Tottenham Police Station. The protest was initially peaceful but rumour began to spread on social media that a 16-year-old girl sustained injury while confronted by the police. The rumour remain unconfirmed to this date but at the time it was enough to trigger an escalation of events.

Looting and rioting in the Tottenham area soon began, and over the next few days copy-cat riots spread widely and rapidly to many other regions of London including but not limited to Enfield, Brixton, Wood Green, Woolwich, Croydon, Islington, Hackney, Battersea, Ealing etc. Even centre areas such as Oxford Circus and Sloane Square were affected. The most vivd image of the London riots was perhaps the burning down of the House of Reeves, a large local furniture shop that had been trading in Croydon since 1867, as a result of arson set off by the rioters. The police failed to subdue the disturbances in London and soon rioting was spreading to many other parts of the country, such as Manchester, Salford, Liverpool, Birmingham, West Bromwich, Wolverhampton, Leeds, Nottingham, etc. Eventually, after 5 days of heavy chaos on a nationwide scale the situation was back under control again by 11 August 2011 when only a handful of new events took place. By then, the country has seen the worst riots in its modern history with 4 civilian deaths and nearly 200 police injuries. It is estimated that 13,000 to 15,000 people were actively involved (Singh et al 2012). Many shops and property was damaged - according to Singh et al (2012) the estimated total cost of the riots is more than half a billion pounds. Much of the damage was concentrated in areas that were affected by the economic downturn several years back where deprivation and youth unemployment were high. In fact, local economic conditions and inequality are often attributed as contributing factors to the England riots, amongst other facts such as racial tension, class tension, gang culture, policing and the rise of social media in facilitating the spread of rumours and the organisation of riots (see for example LSE 2011, Singh et al 2012). Evidently the riots were not endogenous to the penal system and local judges, which is crucial for establishing the exclusion restriction of a valid instrument.

The criminal justice response to the riot was swift. By 11 August 2011, 5 days after the beginning of the riots, over 1,200 arrests had been made across the country. Over 900 of them were in London and 400 of those were already charged. At the peak of the events, several courts, such as Westminster Magistrates' Court and Highbury Magistrates' Court, were even running 24 hours to hear the trials. One year later, over 3,000 offenders related to the riots appeared before court. According to a report published in the Daily Telegraph on 15 August 2011 (the Daily Telegraph 2011), courts and magistrates were advised to ignore sentencing guidelines and hand out tougher sentences to rioters and looters. An example of this was a teenager from West Midlands being sentenced to custody for 10 months after turning herself in for stealing two left-footed trainers and then leaving them outside the shop in Wolverhampton. Another example was two men being jailed for four years for using social media to incite a riot gathering that never took place. This had led to some public figures, including the then-President of Howard League for Penal Reform Lord Carlile, to voice concern over the disproportionate toughness of some of the responses. However, then-Prime Minister David Cameron also openly supported the decisions of the courts. Anecdotal evidence published by the Ministry of Justice (2012) and Bell et. al (2014) clearly show that judges in dealing with offenders related to the riot were a lot harsher than they were historically, even conditional on the same offences and offender characteristics. According to Bell et. al (2014), the probability of being sentenced to immediate custody is more than doubling from 0.247 for non-rioters to 0.550 for rioters, and the average custodial for rioters was also 1.6 months (or 13%) longer.

Given the established deviation from the sentencing guidelines of the judges for rioters, the stress that the criminal justice system was under and the public attention that the events attracted, I feel it is natural to inquire if the England riots acted as a temporary shock to the general sentencing behaviour of judges, not just towards rioters. Judges who were involved with the riot cases may carry over their sense of righteousness and the need to "send the right message" to other similar cases that were unrelated to the riots that they were dealing with during the same time without consciously realising, and hence be handing out prison sentences with higher probability. In contrast, judges in areas not affected by riot would not have seen an increase in work stress and would not have experienced the abnormal deviation from following the sentencing guidelines. I hypothesise that the exogenous event of the England riots in August 2011 can act as a shock to the sentencing system whereby a temporary wedge in judges harshness, as measured by the probability of handing down custody as disposal, is created as a result and courts in riot and non-riot areas diverge in their handling of cases that resemble but have nothing to do with the riot cases. In other words, the riots can act as a source of independent and exogenous variation in the likelihood of an offender receiving custody in the identification of the effects of incarceration on later outcomes such as reoffending and employment.

6.3 Sample and First Stage Analysis

The analysis sample is taken from the Police National Computer, with employment outcomes and history generated using employment spell information from P45 Employment data after linking to the PNC, as described in chapter 3. Analytical issues here with the datasets are shared with previous chapters, namely incomplete recording of offences on PNC and random start and end dates of employment spell in P45. See chapter 4 for the previous discussion. The first issue, I argue, is not a problem here because I am only interested in offences that can lead to custody. The second issue about employment spell is more problematic but various sensitivity analyses in chapter 4 already show there that it does not affect the conclusions about employment outcomes. In this chapter I am going to build on that result and simply take the imputed dates on face value.

When coming up with the sample, I restrict the time dimension to a relatively small window - 1 year before and after the riots. The reason for doing this is because the additional sentiment of righteousness from the judges should die out over that period. Moreover, observations outside the proposed sample period may bear little relevance to the analysis designed around the riots. I restrict the analysis to offences that were classed as either burglary, theft or violence, which were the classes most associated with the riot cases according to MoJ statistics in 2012 (MoJ 2012b). I define the riot areas to be the Police Force Areas of London Metropolitan, Greater Manchester, West Midlands and Merseyside. According to same MoJ statistics, these were the areas with the highest number of court cases in relation to the riot, accounting for over 94% of the 3,103 riot cases heard within one year of August 2011. Using the definition of riot areas, I restrict the analysis to exclude rioters, crudely defined as PNC observations that took place between 6-10 August 2011 in the riot areas. I do this because the focus of this analysis is on the riot effect on offenders who had nothing to do with the riots and the effect of custody on their outcomes. I further restrict the maximum custodial length to be one year. There are intentions behind this. Firstly the maximum custodial length that can be imposed by magistrates' courts, which deal with the largest number of hearings out of all court types and a tier down from the Crown courts which deal the more serious crimes, is 12 months (though this limit is reserved for offenders with multiple triable either-way offences for which they are guilty). Since the magistrates took up most of the heavy workload at the peak of the riots I expect the majority of the riot effect on general sentencing to be borne out in the magistrates' sentencing decisions rather than the Crown judges. This makes it sensible to focus on the sentences that could be given out by the magistrates rather than ones that are handed out by Crown judges. The other reason to ignore custodial punishment over 1 year, which must only given out in a Crown Court, is that while Crown judges may also be affected by the riot in their sentencing decision, the typical nature of a case that arrives at a magistrates' court compared against one at a Crown court means that the latter is often more complex, requires trial jury and more careful consideration. There is less scope for a Crown Court case to be affected by an exogenous surge of rigteousness of the judges. Putting together all the restrictions results in a final analysis sample of 330,340 unique offender-conviction date records (or 230,672 unique offenders). Table 2 displays the summary statistics of the sample for breakdowns by pre-/post-riot and within riot/non-riot areas. The characteristics of the riot and non-riot areas subsamples differ majorly in the proportion of white. This is due to the riot areas as defined being some of the most metropolitan and ethnically mixed cities in England. Another obvious difference between the areas is the offence mix and the likelihood of receiving custodial sentence: offenders in the big cities tend to commit crime of more serious nature and as a result attract more severe punishment.

I use a difference-in-differences approach in establishing the relevance condition of the riot as a valid instrument in identifying the effect of custody on subsequent outcomes. This is also, of course, known as the first stage regression in the instrumental variable estimation procedure, which I specify as follows:

$$x_i = \beta_0 + riot_i\beta_1 + time_i'\beta_2 + area_i'\beta_3 + offence_i'\beta_4 + z_i'\beta_5 + u_i, \tag{6.1}$$

where x_i is a binary outcome variable that indicates if the punishment of PNC record

i is custodial sentence, $riot_i$ a binary indicator of if offence i happened in a riot area, and z_i a vector of offender characteristics related to i including age of offender, age squared, gender, ethnicity, and employment history. The other variables $time_i$, $area_i$ and $offence_i$ are vectors of self-explanatory binary indicators describing the month, police force area in which the offence i took place and what offence type out of burglary, violence and theft the offence is. Note that x_i, as shall be described in the next section, is the endogenous independent variable of interest in the reoffending and post-custody employment equations. Before looking at the regression results it is perhaps more intuitive to visualise the impact of riot through figure 1.

There are 7 disposal outcomes available - police caution, absolute discharge, conditional discharge, fine, community penalty, suspended sentence and custodial sentence - and each graph plots the evolution of the usage of the referred disposal as a % of total disposals within the same month by riot and non-riot police force areas over a symmetric 2-year period around the England riots in August 2011 (referenced as the base period 0). It is obvious to see that the non-riot area series are all very smooth and show no reaction to the riot events in period 0. There is a general shift from police caution to more onerous types of punishment such as fines, suspended sentence and custody. The trend in the use of community punishment in non-riot areas is largely flat. As mentioned above, the usage of disposal has a different profile in the riot areas mainly due to the different composition of crimes in the metropolitan areas. The more severe punishment such as communty punishment, suspended sentence and custody are more commonplace in riot areas, causing disposal-specific wedges to be observed, but nonetheless the national trends as observed in the non-riot areas are all present in the riot areas. The major difference between the riot and non-riot area series however is that a statistically significant structural break (without controlling for the range of fixed effects and offender characteristics) in the riot areas can be easily observed in the graph for custody at period 0, increasing the wedge and indicating a very clear riot effect in sentencing as hypothesised. While more people were sentenced to prison, there is not an obvious discontinuity in the average custodial length as shown in the final panel of figure 1, suggesting there was no up-tariffing conditional on a custodial sentence. Interestingly, the riot effect on probability of imprisonment as represented by the increase in the wedge looks fairly constant at 1.5% over the one year after riot rather than dissipating, providing justification to the first stage specification of a time-constant riot effect within the analysis window rather than a time-dependent effect. This could be due to the judges facing a constant stream of riot cases to deal with over that time so the surge of sense of righteousness remained throughout. In face of the structure break in the likelihood of custodial sentences in the riot areas at period 0, there is a corresponding statistically significant decrease in the use of conditional discharge in riot areas, and it appears the usage of other disposals is not affected. While it is tempting to attribute the rise in one wedge to a direct substituting decrease in another, in reality if sentencing is linear then it could be a general upward shift that get transmitted to the top (custody) without showing obvious signs in the intermediate punishment, or the more likely scenario is that we have non-linear sentencing so the substitution is spread over a range of disposals. A statistical analysis controlling for the offence-level fixed effect and offender characteristics can increase estimation precision of the riot effect and may provide insight into the nature of the shift, which is directly linked to the counterfactual scenario that the second stage analysis is concerned with.

Table 3 shows the results of the statistical analysis as specified above, using all disposal outcomes in turn. The results bring out the pattern in the upscaling of sentencing more - we also now detect decrease in the use of fine and an increase in suspended sentence, which is a more severe version of conditional discharge as it also involves a criminal conviction record. Even then, it is not clear whether suspended sentence should be regarded as the direct counterfactual to conditional discharge, which would leave fine as the direct counterfactual to incarceration. I do not have enough empirical evidence to be certain of the counterfactual to incarceration. This caveat should be borne in mind as we discuss the main results. According to coumn 1 in table 3 the riot effect on the likelihood of receiving custodial sentence is 0.0129, or a 1.29% point increase, and is significant at the 5% level. The F-statistics for joint-significance stands at 82.3, well above the recommended 10 for testing weak instrument, confirming a strongly relevant first stage regression. As a note, the change may seem economically insignificant but considering the base likelihood of imprisonment in the year preceding the riot was about 13% according to table 2, the riot effect can be understood as a 10% increase in odds. For a punishment that is the most severe in the system and the consequences potentially costly to life courses, particuarly given there was no change in sentencing guideline in the usage of incarceration and the fact that I am looking at offenders who had nothing to do with the riots, this change in odds that persisted a year is actually not economically insignificant at all. Though, of course, the more pressing issue here for the second stage analysis is whether the exogenous variation in the likelihood of receiving custody due to the riot is sufficient to limit the bounds of the standard errors of the IV estimates in the second stage to a small enough band to allow statistical significance and a meaningful interpretation of the results.

Regardless, in this section I have demonstrated through a difference-in-differences set up that the riot has not only led to judges in the riot areas dish out more severe punishment to rioters as documented by MoJ (2012a) and Bell et al (2014), but has also created a spillover effect to all non-riot offenders in the affected area afterwards whereby judges have become harsher towards them in general as well, perhaps out of an additional sense of righteousness and the surge in willingness to punish induced by the riots. The statistical analysis clearly shows that the relevance condition for instrumental variable estimation is satisfied. And as discussed in section 2, the trigger of the riot was the random event of the death of Mark Duggan, so exclusion restriction of the instrument is theoretically sound. In the next section, I outline the second stage specification and discuss results from the main analysis.

6.4 Main analysis

The main equation of interest, or the second stage, is as follows:

$$y_{ik} = \gamma_{0k} + x_i\gamma_{1k} + time_i'\gamma_{2k} + area_i'\gamma_{3k} + offence_i'\gamma_{4k} + z_i'\gamma_{5k} + v_{ik}, \tag{6.2}$$

for k =1,...,12, where y_{ik} is the outcome, either the number of reoffences (I use the variable on date of offence to compute this, rather than date of conviction) or a binary employment indicator, for person i in the k-th period after conviction or release from prison. The idea is that a collection of the $\hat{\gamma}_{1k,IV}$ should display the profile of prison effects on outcome over time. Note that it is not a trivial point that the outcome is measured slightly differently for prisoners and non-prisoners. While both may have been convicted to on the same day, non-prisoners regain their freedom right after court and free to commit further crime or find a job, and prisoners have to wait until their sentences are over. This means that the counterfactuals of the experiment lie on displaced time planes, ie conditional on $time_i$, which is the period of conviction, the y_{ik} for prisoners and non-prisoners are most likely measured at different points in time and the difference in time exactly equals the prisoners' sentence length. While this issue does cause some problems, I argue that the effect is minimal because the maximum custodial sentence length is 1 year. Given the automatic release at half-way point, this means that the maximum counterfactual time displacement is 6 months, with the majority much lower than that (table 2 suggests that the average is 50% of 74 days, so about 5 weeks).

A more intuitive stacked specification of the second stage is as follows:

$$y_i = \sigma_0 + x_i\sigma_1 + x_i month\sigma_2 + month_i\sigma_3 + controls + Interactions with month_i + \epsilon_i, \quad (6.3)$$

where $month_i$ denotes the number of months since freedom is gained. I have imposed parametric restriction (linear) on the evolution of the prison effects but the advantage is that the specficiation now provides a neat imterpretation of the parameters. We can easily interpret $\hat{\sigma}_{1,IV}$ as the specific deterrence effect upon release from prison, and $\hat{\sigma}_{2,IV}$ as the linear decay of prison effects over time. Obviously, the model can be altered to accommodate higher polynomial evolution of the prison effects.

6.4.1 Reoffending equation

Before discussing the main analysis, I turn to the reduced form results first. The reduced form is a regression of the outcome variable on the instrument and controls, ie:

$$y_{ik} = \alpha_{0k} + riot_i\alpha_{1k} + time_i'\alpha_{2k} + area_i'\alpha_{3k} + offence_i'\alpha_{4k} + z_i'\alpha_{5k} + \zeta_{ik}. \quad (6.4)$$

Results from table 4 show that for $k \leq 4$, the estimated coefficients of $\hat{\alpha}_1$ are negative, some of which are statistically significant at the 5% level, and in general the estimates become more positive as k becomes larger. The parameters of interest in the equation (2), $\hat{\gamma}_{1k}$, are known to be ratios of the reduced form estimates of $\hat{\alpha}_{1k}$ and the first stage estimate of $\hat{\beta}_1$, so we can expect already the signs of the casual effect of prisons on reoffending to be at first negative and later deteriorates.

OLS and IV results of the reoffending analysis are presented numerically in table 5, and graphically in figure 2. Columns 1 and 2 of table 5 are results of flexible estimation of prison effects using the specification in equation (2), while columns 3 and 4 are results of the stacked linear specification as in equation (3). The OLS results in columns 1 and 3 are perhaps consistent with the popular belief of the effect of prison, that the associated stigma is strong such that criminals having left prison have limited options and are persistently more likely to be reoffending. The estimated effect on month 1 reoffending after prison release, reading from column 3, is an additional 0.23 offence. The results from column (1) suggest the evolution of effect over time is highly linear (at least during the first year) and column 3 shows a monthly decline of about 0.01 offence. This suggests that while the prison effect declines over time, at least over the first 2 years ex-prisoners are likely reoffending more than non-prisoners (though bear in mind this is a out-of-sample prediction for the analysis). This is not surprising - as noted above the effect of prison on reoffending is a priori ambiguous. On the 'positive' side there are specific deterrent and rehabilitative effects, but on the other hand there are also criminal network, stigma and scarring effects. On the whole it appears that higher reoffending is correlated with going to prison, and this (prisons appearing criminogenic) is actually a common observation from cross-sectional or OLS studies (e.g. Grogger 1995 or analysis in Chapter 5). However, an obvious problem with OLS is that it assumes the probability of going to prison is exogenous, which is very unlikely to hold. Previous research has shown that fixed effects are important in this area (e.g. Grogger 1995 or analysis in Chapter 5) in explaining outcomes. It is likely that criminals who are sentenced to prison are also tougher criminals who have committed more serious crime and are more likely to reoffend in the future. In this regard OLS estimates are biased and inconsistent.

Using the England riots as an instrument for the likelihood of getting a prison sentence has been discussed above as a valid strategy to identify the causal effect of incarceration on outcomes, getting round the issue of endogeneity. Columns 2 and 4 from table 5 show the results of IV analysis. From the flexible specification it can be seen that, as informed by the reduced form analysis, the estimates for $k \leq 4$ are indeed negative, with month 1 and month 3 significantly so. Eyeballing the IV estimates for all k = 1,...,12, it is clear that the evolution over time resembles a linear trend. When I impose the stacked linear specification, the estimated specific deterrence at the outset of prison release is -0.57 and is significant, ie prisons deter over half an offence per prisoner during their first month of release. However, this specific deterrence deteriorates over time at a high estimated rate of 0.1 offence per additional month since release, meaning that by 6 months after release prisons no longer have any rehabilitative or deterrent value and turns criminogenic afterwards.

There are several discussion points around the results. Firstly, the IV results are clearly different from the OLS results both in terms of the initial effect and the direction of evolution, and confirmed by a Hausman test as discussed later in section 4.3. This indicates that endogeneity exists in the original OLS analysis - the universally criminogenic nature of incarceration is to some extent spurious due to the influence of fixed effects. However, despite successful identification of specific deterrence, which is a uncommon result in the literature and a confirmation of the positive value that prisons can have, the effect also deteriorates very quickly, suggesting the negative effects such as stigma (looked at more closely in the next subsection under employment outcomes) and criminal capital/network effect eventually dominate. The difference in the profile of the effects can have an impact on the cost-benefit evaluation of prison-related policies and is important to note both for academic and policy purposes. Secondly, while the level of statistical significance is barely enough to identify the specific deterrence effect, especially in the flexible specification, I argue that the issue is not one of signifcant concern. The problem inherent in the current study design is that, while the first stage analysis confirms a significant riot effect on the likelihood of incarceration, the absolute magnitude of the first stage estimate is rather small (even though economically speaking a 10% increase in the odds is quite ignificant) so there is not as much exogenous variation to be utilised as one might have hoped. Better data in terms of exact court locations (currently riot effects in the courts are proxied at the police force area level for big areas like London Metropolitan and this reduces the precision of the data since there are courts within London

Metropolitan Police that did not deal with the riots at all) and a bigger theoretical churn through the courts size will help. Even without them, some sigificance is already detected and the trend is strong enough that I argue the result does not look like anomaly.

6.4.2 Employment equation

Apart from studying reoffending, this analysis also looks as employment after prison release as an outcome. Reduced form regression results presented in table 4 suggests rather little of interest - none of the estimates are statistically significant and they seem to scatter around zero. Unsurprisingly the same pattern is carried forward to the second stage analysis. OLS and IV results are summarized in table 6 and figure 3.

OLS estimates from columns 1 and 4 of table 6 tell the typical cross-section story - prisoners are less likely to be employed than a criminal otherwise punished, possible due to stigma and criminal network or criminal capital effects. As we hone in on the OLS results in columns 1 and 2, we can see that the effect of controlling for employment status at the point of conviction is important in explaining post-release employment. Without controlling for it, incarceration is estimated to reduce employment upon release by 10 percentage point at first, and declining to 7 percentage point after a year has lapsed. This is a less dramatic effect than the popular belief, suggesting that the incremental effect of prison over and above that of criminal record is not big, echoing the analysis presented in Chapter 5. Interestingly, when employment status at conviction is controlled for, the estimated incarceration effect reduces to only a 2 percentage point decrease, but the profile increases over time to 4 percentage point after a year. Due to the presence of endogeneity in the OLS model as previously discussed, I refrain from drawing too much insights from the results, but it is useful to note that individual fixed effects such as pre-prison employment status are important factors in explaining post-custody outcomes. This finding is borne out in other chapters of this thesis.

IV estimates that are free from the endogeneity problem paint a rather different picture from the OLS estimates. Evidently the standard errors of the IV estimates in columns 3 and 5 of table 6 are far too big to allow any statistical significance, which again arguably is a sample size issue. Nonetheless the IV estimates at least from the flexible model in column 2 suggest that the prison effect during the first 9 months after release is positive, despite eventually dipping below zero after 10 months. This has some interesting symmetry with the results from the reoffending equation - the identified short term specific deterrence effect coincide with a short period of positive employment and then both go in the undesired direction.

That the estimates are suggesting the causal effect of incarceration on employment can be more positive than negative at first (albeit not statistically significant) is a surprise in relation to other existing estimates in the literature. This result is also at odds with findings from Chapter 5. The most plausible reason is that, as suggested by the large standard errors, the estimates contain random noises and must be disregarded somewhat. That said, another reason why this temporary positive effect, potentially induced by specific deterrence, has not been picked up in the literature is because of the timeframe of the analysis. Most other studies do not study immediate outcomes and focus rather on longer term effects. This is mostly due to the way the counterfactual is set up differently - the "clock" for prisoners in other papers usually start at the point of conviction hence the results in the early periods from the clock start are disregarded to avoid the contamination of incapacitation effects. For example, Bhuller et. al (2016) reports prison effects from 2 years on. The IV results here show that by 1 year after release, the incarceration effects on reoffending and employment would appear criminogenic already (ie consistent with the rest of the literature) so it is possible that the other studies are simply not set up to detect the very short-lived positive effect of prisons on outcomes. In terms of a consistent theory for the apparently counter-intuitive finding, it's useful to bear in mind at the beginning of this chapter that I mention the channels through which prisons affect outcomes are multi-faceted, complex and ambiguous overall.

6.4.3 Robustness

I am unable to undertake the Sargan-Hansen test of overidentifying restrictions in this case due to exact identification in the research design. Instead I undertake the Hausman test to check for endogeneity. Because of the clustering I can no longer implement the classic Hausman test, which is based on the unlikely assumption that the OLS estimates are efficient under the null hypothesis. I implement a robust version of the Hausman test that is based on bootstrapping, on the linearised models. Results from column 1 of table 7 shows that endogeneity exists in the reoffending specification. A p-value of 0.049 means that the null hypothesis of the IV estimates of $\hat{\sigma}$ being equal to the OLS estimates is rejected at the 95% confidence level, confirming the public myth resulting from cross-sectional analyses is indeed confounded in a causal analysis. In the case of employment equation, the p-value is instead 0.865, suggest the null hypothesis of no endogeneity cannot be rejected. This is interesting but most probably due to the large standard errors of the IV estimates as noted above.

A second type of specification robustness test I do is on the degree of polynomial in the regression specification. Equation 3 specifies a linear evolution of the incarcernation effects which provide a neat interpretation that allows easy cost-benefit analysis if one wishes. I test whether the assumption is robust by adding higher degree polynomial terms in both the reoffending and employment IV regressions. In terms of reoffending, columns 2 and 3 from table 8 show that higher degree polynomial terms are not estimated to have statistically significant effect and the signs do not change, suggesting the original specification is robust. In terms of employment, however, column 5 shows that the quadratic specification fits better and produces some statistical significance especially to the negative quadratic term. This is not a surprise given the profile of effects uncovered in Figure 3. In any case the message borne out by the quadratic specification is not contradictory to the linear specification - there are benefits at first but they eventually gives way over time. The cubic specification returns no estimates of statistical significance. Given the imprecise nature of the original estimates, it is hard to conclude that the linear specification to employment equation is robust, but although the quadratic specification fits better, it sends a similar message.

6.5 Conclusion

The effect of incarceration on later outcomes is an important policy topic that have been debated much in the past, albeit unfortunately without much robust empirical evidence. Given the rise in priosn population observed in the recent history in developed countries and the improvement to availability of micro-dataset, there is not a better time to revisit the question. The cross-sectional observation of a correlation between more severe punishment and poorer post-custody recividivism and employment outcomes has led to the popular belief that incarceration leads to poor outcomes possibly via stigma, deterioration of human capital and criminal network effects. Theoretical positive effects such as specific deterrence and rehabilitation are not given much consideration, and are in fact largely undetected in the literature. A large part of this I believe is because research design on the topic has historically been quite bad at dealing with endogeneity, limited by the non-availability of micro-dataset with a sufficient time dimension that allows implementation of quasi-/natural experimental methods. However, this is fast-changing and an increasing body of empircal work is revisiting this policy area and returning robust estimates using different data sources.

This analysis is part of the aforementioned literature. I take a different approach in dealing with endogeneity from the popular method of utilising judge-level randomisation, ie achieving identification off judges' innate differences in their likelihood of giving harsh punishment in face of the same offence. My approach instead is to utilise natural experiment, in this case the England riots in 2011, which as I demonstrate empirically increased the likelihood of being given custody as punishment in the riot-affected areas. Since the event was exogenous, triggered by the unexpected death of a member of public, I argue this makes my approach, in theory at least, superior than judge-level randomisation since the driver of the variation in custody likelihood is demonstrably clearer and perhaps more robust than if it is attributed to innate differences between individuals. While it is not a requirement for the IV relevance condition to understand how innate differences lead to differential custodial probability as long as the effect is present (though presence of monotonicity is highly preferred as discussed in Mueller-Smith 2015), criminal cases are complex so even in the case of case randomisation when the differences are not understood there may still be a small degree of endogeneity.

Using the England riots as an instrument, I find short-lived specific deterrence effect of incarceration on recidivism, ie a reduction of reoffending, but it gives way to more criminogenic effect after 6 months. As far as I am aware, this is the first quasi-experimental study to empirically find a significant effect of specific deterrence. Bhuller et al (2016) also find a positive effect of incarceration on outcomes but since they are looking at longer term outcomes they are much more likely to be rehabilitative effects, particularly given the way prisons and rehabilitation programs are set up in Norway (with a much more positive, open and freedom-based approach compared to other countries). I also find that spending a short time in prison does not significantly reduce employment prospects, at least in the short term anyway. While the effects are not statistically significant, they are more positive than negative in magnitude, springing further surprises. This demonstrates clearly that the way prison impacts on individuals is complex and multi-faceted, with many effects competing with and countering each other over both short term and long term. I believe I am able to detect results that are previously unreported because I have focussed on a much shorter timescale than the rest of the literature.

In terms of policy implications, this study brings to light a couple. Firstly, even though the positive effect of prisons on reoffending is short-lived, it is previously undetected and can change the cost-benefit calculations in the appraisal of prison policy planning. Figure 2 shows that within the first 6 months an estimated total of 2.1 reoffences are prevented per ex-inmate. Back-of-envelop calculations using a combination of a Home Office (2005) study on the cost of crimes against persons and latest ONS Crime in England and Wales (2017) figures, and inflating appropriately, suggest the average cost of a typical offence against persons stands at about £2,600. Note that crimes against businesses are excluded due to unavailabilty of both reliable data and cost estimates. Assuming an annual prison release of 73,560 inmates using the latest MoJ (2017a) figure, together the numbers suggest prisons could bring in around £400 million annually on social cost savings just due to the short-lived specific deterrence that was previously unaccounted for. Considering the MoJ annual budget of around £9 billion, the benefit is not unsubstantial, standing at almost 5%. Of course, the analysis also shows that ex-inmates reoffend more after 6 months and this cancels out specific deterrence, but such negative effects are most probably included, if not exaggerated, in existing calculations already given the previous body of research and the popular belief. This point should not be taken as a nod for building more prisons, merely that the estimates here are part of the big puzzle that needs to be solved.

Secondly, echoing results from previous chapters, the observation that prisoners, or criminals, generally have poor outcomes is largely due to pre-determined factors before they enter the criminal justice system, rather than due to the criminal justice system, as I see that the causal estimates are much smaller than OLS estimates. Hypotheses about social stigma of convictions or negative human capital effects are not supported here. To migitate those pre-determined factors, criminal justice can have a role to play, as evident by Norway's very successful rehabilitation program (Bhuller et al 2016). However, interventions that take place before entry into criminal justice system or at the onset of criminal career may be more effective in general in improving future outcomes of criminals. Such interventions are likely outside the remit of the criminal justice system, such as education, vocational training or even fostering the neighbourhood and environment in which vulnerable children grow up in. Such interventions could provide individuals with marketable skill sets and values that deter them from starting a criminal career or ensure they can absorb the negative shock of a criminal conviction. For the UK, from the evidence here at least, despite prisons having some benefits putting more people to jail is unlikely to be the ailment, nor is it a heavy aggravating factor, for recidivism.

Figures and tables

Table 6.1: Comparison of assault and incarceration rate changes for selected developed economies

	Rate of incarceration			Rate of assault		
	2004	2014	Δ%	2004	2014	Δ%
England and Wales	141	149	6%	965.5	649.1	-33%
United States of America	725	693	-4%	288.7	232.1	-20%
Japan	60	48	-20%	47	21	-55%
Denmark	70	67	-4%	203.8	164.6	-19%
Norway	66	72	9%	62.8	46.2	-26%
Portugal	125	135	8%	399.5	239.1	-40%
Germany	96	76	-21%	582.9	155.9	-73%

Source: United Nations Office on Drugs and Crime.
Note: The rates are per 100,000 population.

Table 6.2: Descriptive statistics of the analysis sample

Before riot:	Riot areas		Non-riot areas	
Age (years)	31.2	(11.6)	29.8	(11.7)
White	0.677	(0.468)	0.908	(0.289)
Male	0.75	(0.433)	0.759	(0.428)
Pr(prison)	0.128	(0.334)	0.107	(0.309)
Pr(violence)	0.287	(0.452)	0.255	(0.436)
Pr(theft)	0.645	(0.479)	0.669	(0.471)
In a job currently?	0.123	(0.328)	0.132	(0.339)
Average prison length (days)	74.2	(48.3)	73.5	(48.9)
N	44,454		129,302	

After riot:	Riot areas		Non-riot areas	
Age (years)	32.4	(11.6)	31.3	(11.6)
White	0.692	(0.462)	0.91	(0.287)
Male	0.77	(0.421)	0.767	(0.423)
Pr(prison)	0.164	(0.371)	0.128	(0.334)
Pr(violence)	0.281	(0.45)	0.229	(0.42)
Pr(theft)	0.646	(0.478)	0.698	(0.459)
In a job currently?	0.103	(0.304)	0.109	(0.311)
Average prison length (days)	74.8	(48.1)	70.9	(47.8)
N	35,823		117,961	

Standard deviations in parenthesis

Table 6.4: Reduced form regression of outcomes on riot

Riot effect in month:	Recidivism (1)		Employment (2)	
1	-0.932**	(0.414)	0.015	(0.116)
2	-0.058	(0.370)	-0.108	(0.173)
3	-0.762**	(0.365)	0.015	(0.200)
4	-0.393	(0.344)	0.120	(0.212)
5	0.104	(0.344)	0.284	(0.224)
6	0.065	(0.326)	0.282	(0.227)
7	0.764**	(0.312)	0.229	(0.231)
8	0.301	(0.302)	0.080	(0.234)
9	0.264	(0.293)	-0.011	(0.234)
10	0.545*	(0.281)	-0.162	(0.235)
11	0.800**	(0.271)	-0.282	(0.235)
12	0.683**	(0.263)	-0.290	(0.236)
N	330,340			

Note: The reported employment estimates are multiplied by 100. Controls include age, age squared, police force area, crime type, ethnicity, gender, time and current employment. Clustered standard errors are in parenthesis. **/* denotes statistical significance at the 5%/10% level.

Table 6.3: Riot effects on different punishment outcomes

Outcome:	Prison, first stage	Caution	AD	CD	Fine	CP	SS
$\hat{\beta} \times 100$	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Riot	1.29**	0.251	-0.037	-0.920**	-1.00**	-0.475	0.572**
	(0.319)	(0.346)	(0.039)	(0.242)	(0.233)	(0.374)	(0.266)
F	82.3						
N	330,340						

Note: controls include age, age squared, police force area, crime type, ethnicity, gender, time and current employment. Clustered standard errors are in parenthesis. **/* denotes statistical significance at the 5%/10% level.

Table 6.5: OLS/IV regression of recidivism

	OLS (1)		IV (2)		OLS (3)		IV (4)	
$\hat{\gamma}_1$, Prison effect in month:								
1	0.218**	(0.004)	-0.721*	(0.373)				
2	0.192**	(0.004)	-0.045	(0.288)				
3	0.202**	(0.004)	-0.589*	(0.326)				
4	0.185**	(0.004)	-0.304	(0.283)				
5	0.182**	(0.003)	0.081	(0.265)				
6	0.173**	(0.003)	0.051	(0.251)				
7	0.157**	(0.003)	0.591**	(0.259)				
8	0.152**	(0.003)	0.233	(0.233)				
9	0.142**	(0.003)	0.205	(0.226)				
10	0.135**	(0.003)	0.422*	(0.226)				
11	0.128**	(0.003)	0.620**	(0.236)				
12	0.122**	(0.003)	0.529**	(0.221)				
$\hat{\sigma}_1$, Initial prison effect: (specific deterrence)					0.225**	(0.003)	-0.600**	(0.289)
$\hat{\sigma}_2$, Linear monthly evolution:					-0.008**	(0.000)	0.106**	(0.032)
Specification	Flexible		Flexible		Linear		Linear	
N	330,340							

Note: Controls include age, age squared, police force area, crime type, ethnicity, gender, time and current employment. Clustered standard errors are in parenthesis. **/* denotes statistical significance at the 5%/10% level.

Table 6.6: OLS/IV regression of employment

	OLS (1)		OLS (2)		IV (3)		OLS (4)		IV (5)	
$\hat{\gamma}_1$x100, Prison effect in month:										
1	-1.99**	(0.08)	-9.73**	(0.13)	1.15	(8.86)				
2	-2.62**	(0.10)	-9.07**	(0.13)	-8.28	(13.28)				
3	-3.17**	(0.11)	-8.90**	(0.13)	1.17	(15.1)				
4	-3.49**	(0.11)	-8.64**	(0.13)	9.19	(16.5)				
5	-3.86**	(0.12)	-8.55**	(0.13)	21.73	(17.9)				
6	-3.98**	(0.12)	-8.32**	(0.13)	21.59	(18.2)				
7	-3.99**	(0.12)	-8.04**	(0.13)	17.51	(18.2)				
8	-4.03**	(0.12)	-7.83**	(0.13)	6.11	(18.0)				
9	-4.08**	(0.12)	-7.63**	(0.13)	-0.86	(17.8)				
10	-4.14**	(0.12)	-7.51**	(0.13)	-12.91	(18.8)				
11	-4.01**	(0.12)	-7.17**	(0.13)	-24.51	(21.0)				
12	-4.00**	(0.13)	-6.99**	(0.13)	-34.36	(29.6)				
$\hat{\sigma}_1$, Initial prison effect:							-2.59**	(0.11)	14.90	(14.51)
$\hat{\sigma}_2$, Linear monthly evolution:							-0.16**	(0.01)	-2.19	(2.28)
Specification	Flexible		Flexible		Flexible		Linear		Linear	
Include current employment?	Yes		No		Yes		Yes		Yes	
N	330,340									

Note: controls include age, age squared, police force area, crime type, ethnicity, gender, time. Clustered standard errors are in parenthesis. **/* denotes statistical significance at the 5%/10% level.

Table 6.7: Hausman test of IV vs OLS estimates

	Recidivism eq.	Employment eq.
χ^2	9.53	1.28
p-value	0.049	0.865
Reject H_0 of no endogeneity?	Yes	No

Note: Results are from implementing the "rhausman" package on STATA, a cluster-robust version of the Hausman test based on the bootstrap and does not require the OLS estimators to be fully efficient under the null hypothesis.

Table 6.8: Specification tests of the IV regressions

Estimated prison effect:	Recidivism (1)	Recidivism (2)	Recidivism (3)	Employment (4)	Employment (5)	Employment (6)
Initial	-0.600**	-0.799**	-0.775	14.900	-21.461	-15.944
	(0.289)	(0.378)	(0.484)	(14.506)	(0.162)	(18.348)
Evolution - Linear	0.106**	0.191**	0.172	-2.190	13.741*	9.426
	(0.032)	(0.092)	(0.236)	(2.282)	(7.582)	(15.481)
Quadratic (x100)		-0.656	-0.306		-125.788**	-44.863
		(0.586)	(3.873)		(63.505)	(282.404)
Cubic (x10000)			-17.940			-4217
			(188.950)			(14907)
Degree of polynomial	One	Two	Three	One	Two	Three
N	330,340					

Note: Employment regression estimates are multiplied by 100, as in previous tables. Controls include age, age squared, police force area, crime type, ethnicity, gender, time. Clustered standard errors are in parenthesis. **/* denotes statistical significance at the 5%/10% level.

Figure 6.1: Probabilities of criminal justice punishment (time 0 = August 2011)

(a)

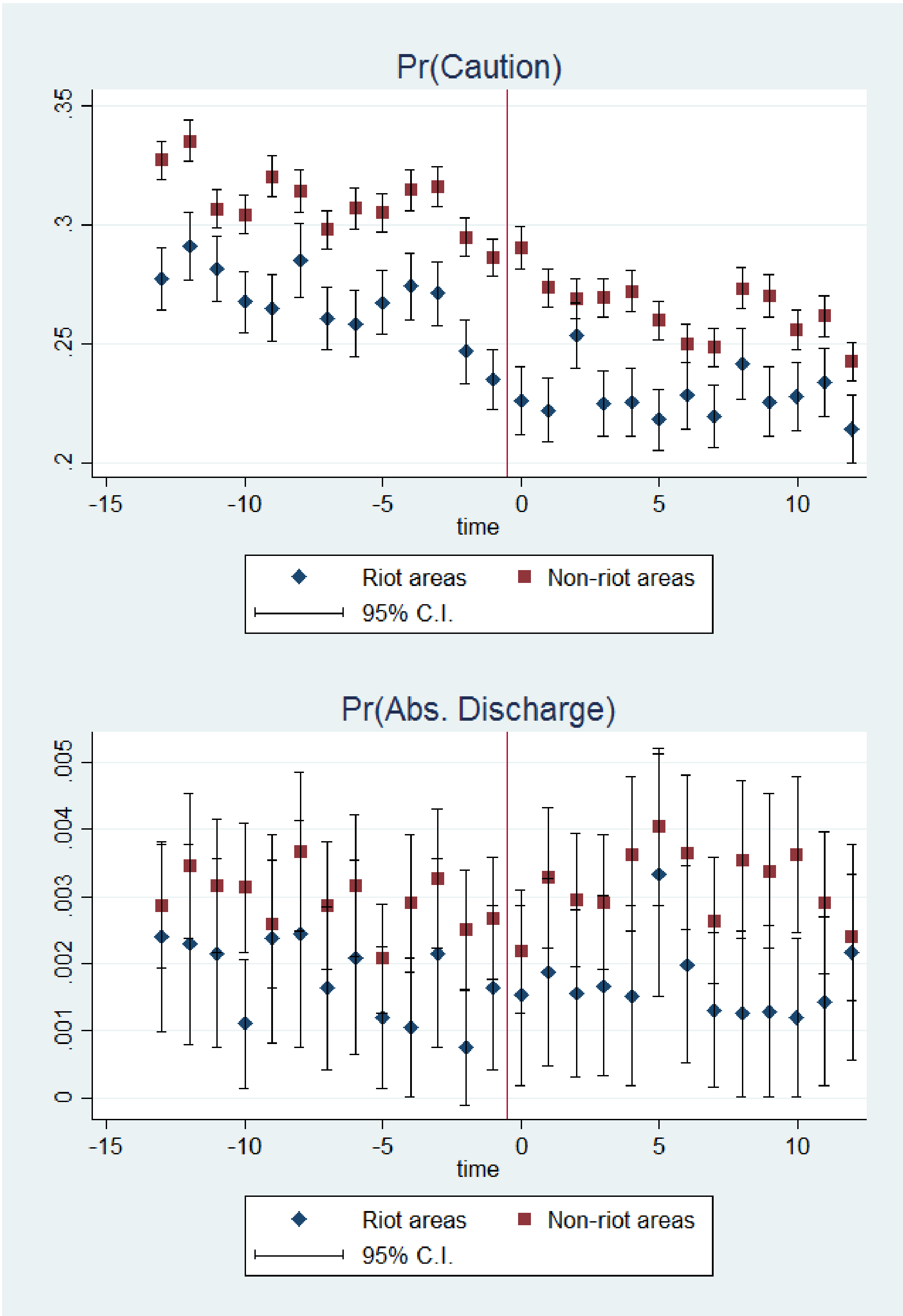


Figure 6.1: Probabilities of criminal justice punishment (time 0 = August 2011)

(b)

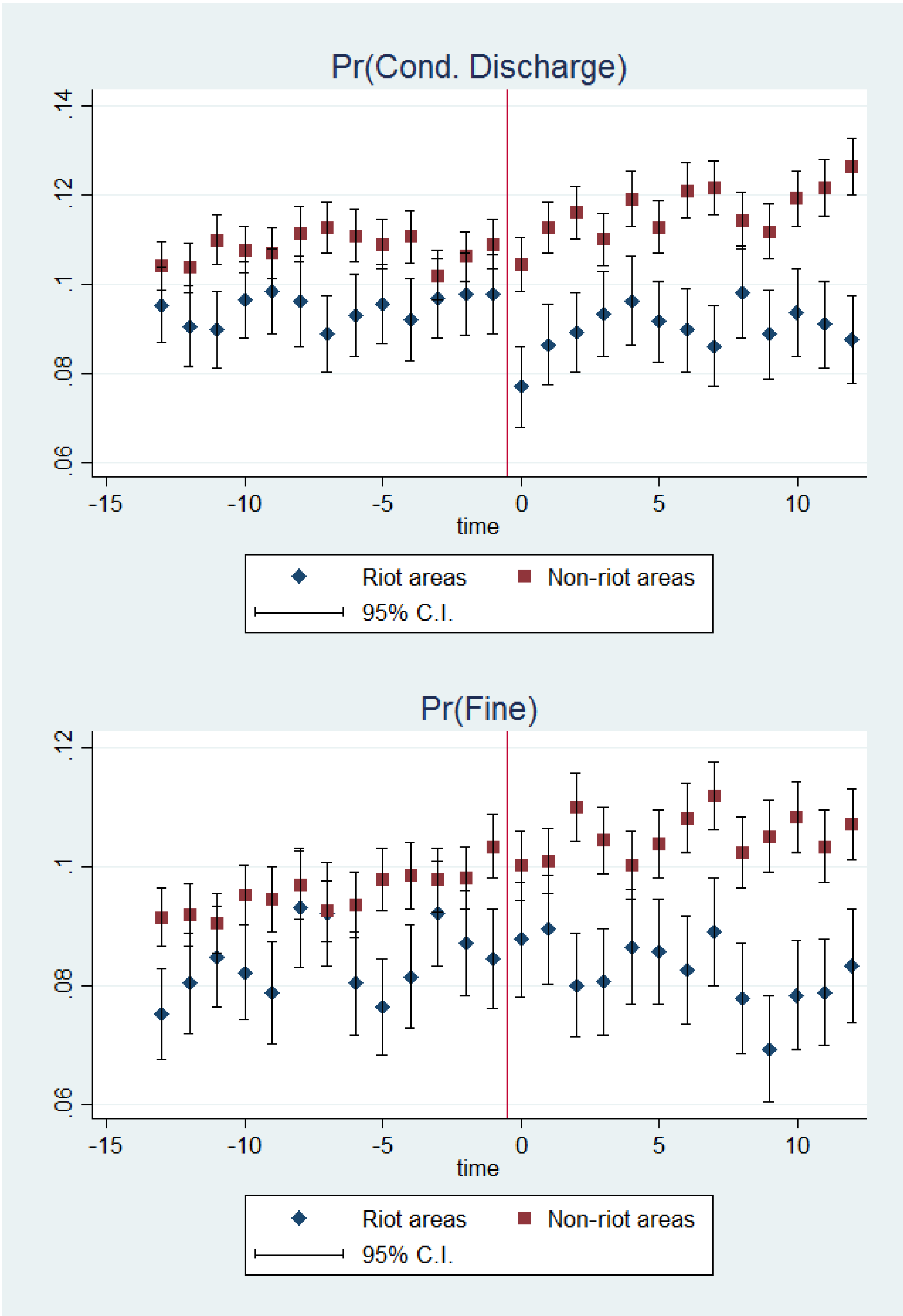


Figure 6.1: Probabilities of criminal justice punishment (time 0 = August 2011)

(c)

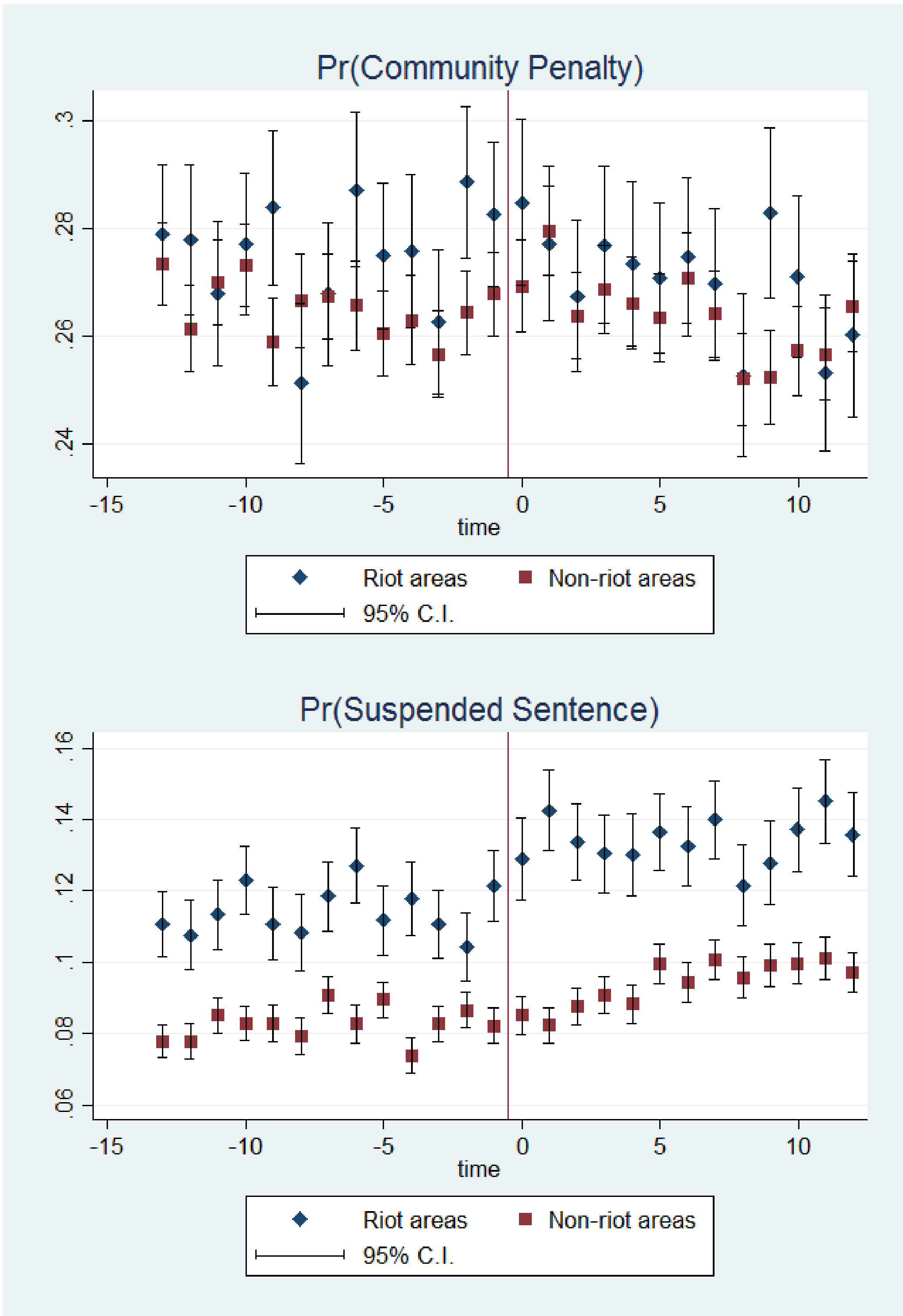


Figure 6.1: Probabilities of criminal justice punishment (time 0 = August 2011)

(d)

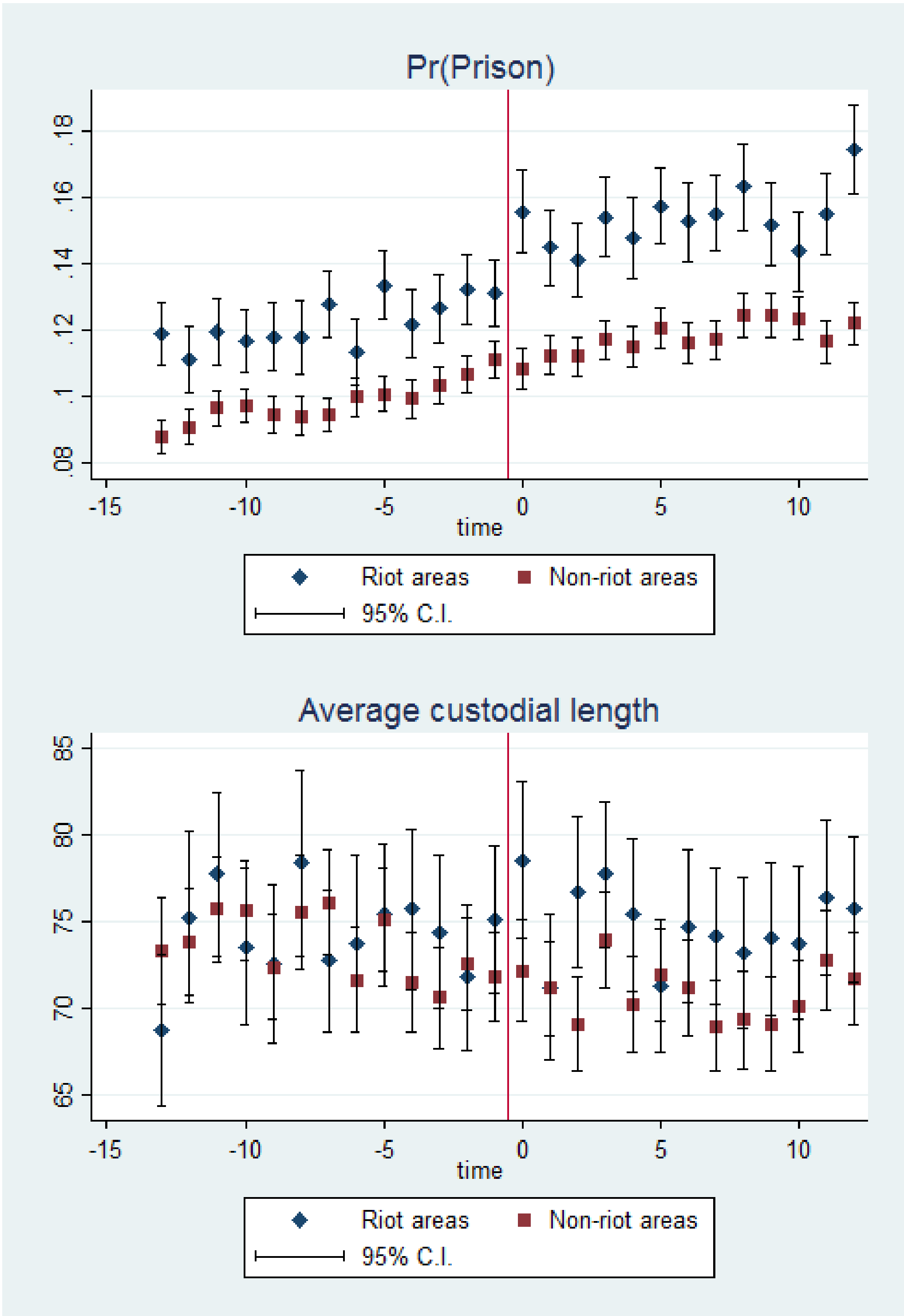


Figure 6.2: Estimated incarceration effect on the number of reoffences

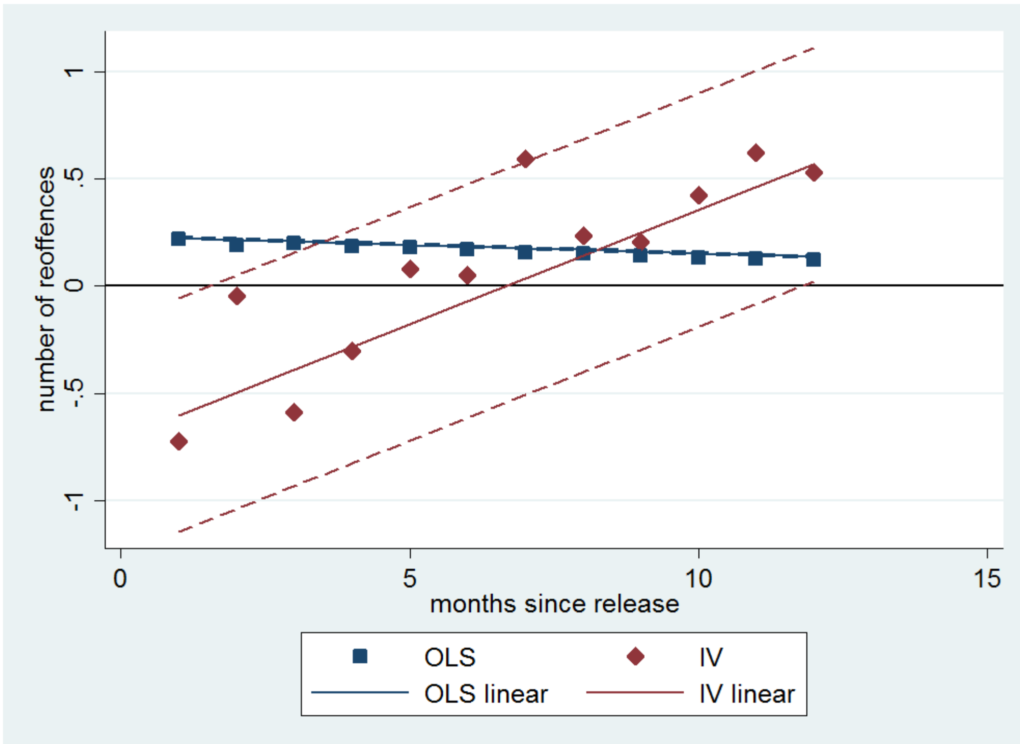
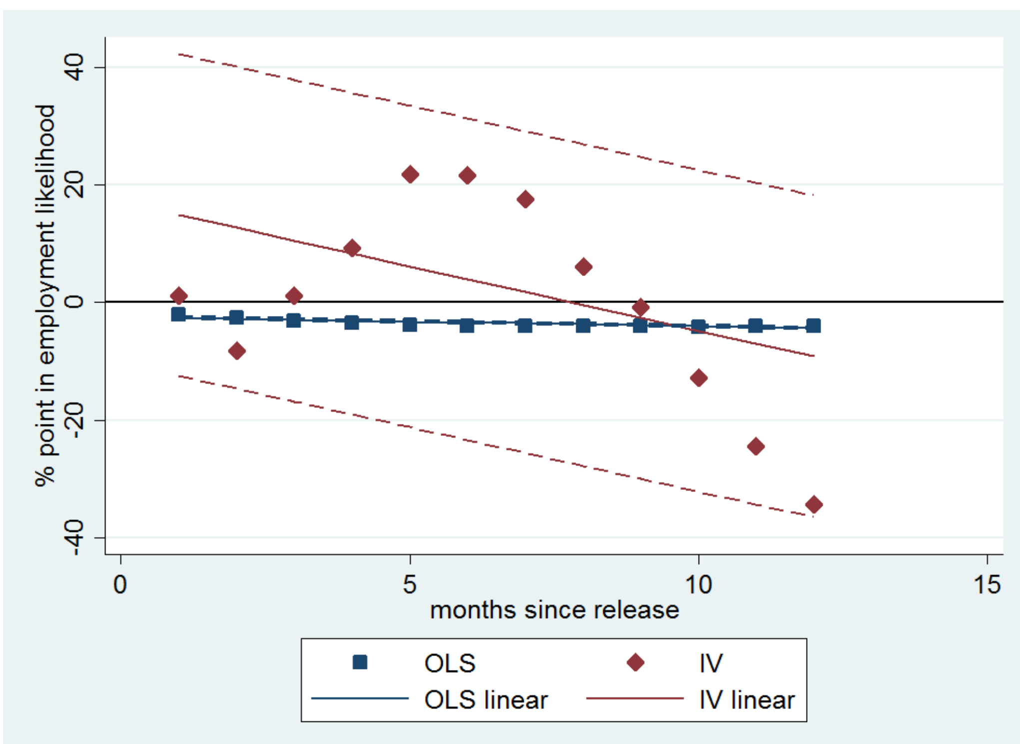


Figure 6.3: Estimated incarceration effect on the epmloyment likelihood



Chapter 7

Future research

The empirical literature on the economics of criminal justice is, while not totally lacking, not as lively as other fields related to social policy, such as labour market, health, education, etc. A lack of experiments that would be deemed ethical and the highly sensitive nature of personal level crime data mean that in the past there are limits to what data econometricians have access to and what research designs they can deploy to identify useful policy parameters. We see that early research by social researchers or economists were typically based on either aggregated data or reasonably small-sized longitudinal studies that were of voluntary nature, which somewhat restricted their usefulness. However, this is about to change. As the world moves towards one with more microdata and higher recognition for the value of linked data encompassing more inter-related outcomes, empiricists have a big role to play in taking advantage of the movement to inform the research agenda and policy debate. In this thesis I have used a new UK microdata covering crime, labour market and benefits outcomes to make three important empirical contributions. First, I find that post-custody supervision, contrary to the recent offender management policy movement in the UK, makes little difference to both crime and non-crime outcomes. It is not found to have any rehabilitative effect, at least not in the way the supervision programme was run. Second, I find that criminal convictions only cause rather short-lived damages to earnings and employment prospective, which is at odds with perhaps the traditional wisdom. Finally, I find that incarceration can induce short term specific deterrence effect on inmates upon release but it fades and gives way to more criminogenic factors after 6 months. They are part of the new wave of empirical evidence on criminal justice that is starting to emerge due to better availability of microdata around the world, but a lot more can and should be done in time. Below I list several future avenues for research.

The interplay between crime and labour market outcomes are complex and intricate and an immediate extension to my thesis will be to look at the role of occupation, which is overlooked here due to unavailability of information of employer information in my dataset. While, for example, I find that the effect of criminal conviction is not as adverse as traditional wisdom dictates, it will be interesting to see if this is because ex-inmates moving down the skills ladder and getting employment in industries requiring lower skills to compensate for the presence of criminal record which theoretically dampens employer's demand and wage offer. Occupation analysis will shed light on the some of 'black boxes' that I have identified here. A further step, therefore, is to develop better and more hollistic modelling of criminal behaviour, using the multi-dimensions of microdata to validate and identify structural paramters regarding preference for crime-labour market trade-off and responses to punishment. As discussed in the literature review, crime is a complex behaviour that has dependencies on many other factors and currently even the most sophisticated dynamic models fail to capture the intricacies and this limits their usefulness in policy predictions.

Another strand of work that will benefit hugely from availability of better microdata is criminal career analysis. The crime-age profile (Hirschi & Gottfredson 1983) is well-documented around the world but the determinants of the profile are less well-understood. For example, the causes of onset, continuation and termination are under-explored but clearly they have very important policy implications. A running theme through this thesis, echoed in some part of the literature, is that the criminal justice system, or a conviction record, or even a spell in the prison is limited in affecting outcomes, whether positively or negatively. Fixed effects formed at the point of entrance to the criminal justice system appear very important, suggesting that policies may be more effective if they follow a hollistic approach such as by looking also at health, education, training, childhood development, deprivation, community, family upbringing etc. Micro-datasets that track multiple of these outcomes and allow identification of clusters (such as family, community, or school) will go a very long way to demystify some of the fixed effects and allow policymakers and researchers to understand better the determinants of the criminal career profile and the counter-measures.

Recently, Kleinberg et al. (2015) points out that a policy problem can be divided into a prediction and a causal inference component, and both are equally important though economists tend to focus more on the latter. For instance, judges have to decide whether to detain or release arrestees as they await adjudication of their case. Knowing the causal relationship of pre-trial detention on outcome improves the policy in general, but so does an accurate prediction about the arrestee's probability of committing a crime in the pre-trial period. Kleinberg et al. (2017) show that machine learning techniques can dramatically improve upon judges' predictions and substantially reduce the amount of crime without adjusting the policy itself. While better microdata support causal analyses like I have presented in this thesis and those listed above as potential future research, they also lend themselves naturally to the fast-booming field of data science, which is more concerned with pattern recognition and prediction. Machine learning techniques are better suited than standard econometric analysis in predicting recidivism risk or employment likelihood, or sub-categorising offenders that may have heterogenous effects for optimal policy response, etc. Future criminal justice research should consider cross-cutting methods that incorporate machine learning such as those proposed by Athey and Imbens (2016), to take advantage of the richness in new microdata in order to enhance their usefulness for policy use.

All these are very exciting, of course. Criminal justice policies in the past have more often than not been based on belief and hypothesis, valid as they may be, rather than hard evidence. Better data will result in better research, which means the evidence base for policymaking is going to be broadened and become more robust than ever.

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