Labour Markets, Public Policies and Crime: An Empirical Analysis

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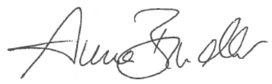
Declaration

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



London, November 2015

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Place and Date

Statement of conjoint work

Two of the chapters that form this thesis involve conjoint work, as specified below:

Chapter 3 "*Crime scars: Recessions and the making of career criminals*" is conjoint work with Brian Bell (University of Oxford and Centre for Economic Performance, London School of Economics) and Stephen Machin (University College London and Centre for Economic Performance, London School of Economics). Overall, my contribution amounts to one third of the total paper.

Chapter 5 "*Crime and racial profiling: New York's stop-and-frisk policy*" is conjoint work with Laura Jaitman (Inter-American Development Bank) and Stephen Machin (University College London and Centre for Economic Performance, London School of Economics). Overall, my contribution amounts to one half of the total paper.

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Abstract

The thesis consists of six chapters.

Chapter 1 is an introductory chapter.

Chapter 2 is a survey of the literature on crime and labour markets. In that chapter, I discuss the seminal work that has been undertaken in that field of research and discuss the advances in the theoretical and empirical literature. Reviewing the literature, I identify open research questions. Some of these questions are addressed in subsequent chapters of this dissertation.

Chapter 3 builds on the literature which provides evidence of long-term consequences for workers who first join the labour market during economic downturns. Using a range of data sources from the U.S. and UK, we demonstrate a substantial long-run effect of recessions on criminal behaviour: We find that youth who enter the labour market during recessions are significantly more likely to become criminal than those who graduate into a stronger labour market.

Chapter 4 investigates the contemporaneous relationship between unemployment and crime in the context of increasing unemployment durations in the U.S. I employ quasi-experimental estimation techniques to study the impact of temporary unemployment benefit extensions on crime, and to establish the causal link between unemployment, unemployment durations and crime. The results support the hypothesis that the relationship between unemployment and crime depends on the duration of unemployment.

Chapter 5 studies the impact of the stop-and-frisk policy in New York City as a policy that explicitly aims at crime deterrence. Using a range of data sources and quasi-experimental estimation techniques, we estimate the overall impact of the policy on crime in New York City. Further, we provide evidence that supports the hypothesis of racial bias and in particular claims that Afro-Americans face a disproportional probability of a stop-and-frisk encounter. Yet, our estimations suggest that there is no knock-on effect on crime.

Chapter 6 is a concluding chapter.

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Chapter 1. Introduction

Social scientists such as criminologists and sociologists have studied criminality and the determinants of criminal behaviour for more than two centuries. The economic framework for understanding crime dates back as far as the eighteenth and nineteenth century, when the social philosophers Cesare Beccaria (1738-1794) and Jeremy Bentham (1748-1832) developed the notions of rational choice and deterrence in illegal behaviour. They argued that individuals rationally choose to commit crime if the benefits exceed the cost of such activity. Further, they reasoned that imposing an additional cost to criminal action through punishment could help to deter individuals from crime. While different schools of thought developed intermittently, these ideas were taken up in the seminal writings on the economics of crime by Gary Becker (1968) and George Stigler (1970). Their work laid the foundation of a growing literature on theoretical and empirical aspects of the economics of crime.

The relationship between legal and illegal activity has received particular attention by labour economists over the last decades: How do individuals choose between participation in the labour market on the one hand and crime on the other hand? What is the margin at which individuals choose between legal and illegal activity: Is participation in the labour market and in crime mutually exclusive (choice at the extensive margin) or is the choice of criminal activity a time allocation decision (choice at the intensive margin)? Who are the individuals at risk of criminal behaviour, and how are they affected by the state of the labour market in general and their individual labour market performance in particular? In chapter 2 of this dissertation, I review the existing theoretical and empirical literature on labour markets and crime that addresses these questions. First, I discuss the seminal economic models of crime as well as subsequent models that take a more dynamic view of criminal choices. Second, I discuss examples of empirical work on different aspects of labour markets and crime.

Since the 1960s, the empirical literature on the causal relationship between labour markets and crime has grown considerably. One of the most intuitive features of that relationship is the causal link between unemployment and crime. Strikingly, the empirical evidence is not yet rigorous in terms of magnitudes and statistical precision, but indeed suggests that unemployment increases the probability for criminal behaviour. Two chapters of this thesis contribute to that literature by investigating aspects of the link between crime and unemployment which to the best of my knowledge have not yet been studied in the literature.

Chapter 3 addresses the question of how criminal careers are initiated, and in particular whether labour market conditions at the time when youth leave school play a role in forming criminal choices. The literature has demonstrated scarring effects of criminal behaviour on labour market opportunities later in life. Yet, we do not know whether there is a reverse scarring effect: Recessions typically lead to an increase in youth unemployment rates, leaving high school graduates to face more difficulties in finding jobs whilst not yet having financial insurance. Hence, low expectations on returns to legal activity and peer effects might trigger initial involvement in crime as well as a first encounter with the criminal justice system (Becker, 1968). Subsequently, knock-on effects might prompt criminal careers and hence lead to long-term scarring effects.

The empirical analysis of chapter 3 is based on a variety of U.S. and UK data sources at the individual and birth cohort level. The estimations yield robust evidence that young people who graduate from school during recessions are significantly more likely to become involved in crime than those who leave school while labour markets are more buoyant. Moreover, the results lead to the conclusion that recessions do play a role in the making of career criminals, as crime scars from higher entry level unemployment rates are both long lasting and substantial.

While chapter 3 demonstrates a scarring effect of labour market conditions at the time of labour market entry on long-term criminal behaviour and criminal careers, chapter 4 focuses on a more contemporaneous relationship between unemployment and crime. In particular, I study the impact of recent and unprecedented structural changes in the U.S. labour market on crime. During the Great Recession, similarly to previous recessions, unemployment rates were very high. Yet, unlike previous recessions, there has been a substantial increase in the average duration of unemployment spells. In particular, one has seen an unprecedented occurrence of long-term unemployment which stands in clear contrast to European labour markets with a history of long-term unemployment. These increases in unemployment durations have been associated with temporary unemployment benefit extensions which were implemented by policy makers in order to delay the time of benefit exhaustion in times of financial hardship.

In chapter 4, I use quasi-experimental methods to estimate the impact of these labour market and policy changes on crime. In line with previous findings in the literature, I find that higher unemployment is linked to higher crime rates. More surprisingly, the empirical results suggest that the positive relationship between unemployment and criminality is driven by the unemployment benefit extensions which are linked to longer unemployment durations and higher unemployment rates. In fact, there are models that suggest that the probability of criminal behaviour increases with the duration of unemployment for example due to human capital effects or behavioural responses. Indeed, I find empirical evidence that the relationship between unemployment and crime varies with the duration of unemployment. Hence, I conclude that not only is there a long-term, dynamic relationship between unemployment and crime as argued in chapter 3, but also the more contemporaneous link underlies dynamics that have not yet been captured in the empirical literature.

Economists have not only studied the interaction between labour markets and crime, but also have shown great interest in evaluating mechanisms of deterrence from crime. While understanding the relationship between unemployment and crime is important in order to apprehend the implications of labour market policy design on criminality and the broader prevention of initial criminal behaviour, understanding crime deterrence mechanisms and their effectiveness is important for policy decision with respect to the efficient allocation of resources. A particular interest has hence been shown in the impact of general policing and particular policing policies on crime.

Chapter 5 is an analysis of a prominent example of such a crime deterrence policy: The stop-and-frisk policy in New York City. The stop-and-frisk policy allows police officers to stop, question and frisk pedestrians in New York City based on a reasonable suspicion. The programme has been argued by some to be one of the contributing factors to the decline in criminality in the city over the last two decades. Yet, the stop-and-frisk policy has been controversial in particular with regard to claims of racial profiling and racial bias: Advocates of the policy argue that any racial profiling strategy is based on statistical discrimination only, whereas opponents argue that the policy is subject to racial discrimination. In this chapter, a variety of New York City data sources and empirical strategies are employed in order to investigate claims of statistical versus racial discrimination as well as to evaluate the effectiveness of any existing racial profiling on crime. The empirical results yield evidence which supports the hypothesis of racial bias with respect to the probability of being stopped and frisked, yet we do not find any evidence that there is an associated effect on crime. We conclude that the stop-and-frisk policy design in New York City is non-optimal as it stands but, the policy being very controversial, restrain from any broader conclusions.

Overall, in this dissertation I apply a variety of economic concepts and statistical methods to various aspects of criminal behaviour. In particular, I study different factors and policies that potentially trigger or deter criminal behaviour. The findings, as previous findings in the literature, are important for policy makers with regard to policy design and policy evaluation in the context of crime prevention and efficient resource allocation. In chapter 6, I summarise the findings in this dissertation and discuss the contributions and limitations of the presented research.

Chapter 2. Crime and Labour Markets: A Survey of the Literature

2.1 Introduction

Criminality constitutes a major burden for societies. Not only is crime associated with negative consequences for individuals involved in the criminal action - both offenders and victims - but also the economy as a whole. Creating social losses and inefficiencies, psychological consequences for victims and economic concerns for (ex-)prisoners, crime bears high costs for an economy. In monetary terms that includes for example costs for prisons, courts or crime prevention. In his seminal work on the economics of crime, Becker (1968) describes the *net* cost of crime caused to a society as the difference between its harms for the victims and the economy and its benefits for the offenders and potentially the economy. Whilst the harm of crime as well as the offenders' benefits are obvious, an economy's benefits of crime may be less so. Yet, it is possible to think of rare, positive externalities of crime such as the employment effect on police forces or courts. As the net cost of crime increases in the number of crimes committed, economies have an incentive to prevent crime - which explains the existence of punishment institutions in an economy.

In order to minimise the cost both for individuals and for the economy, it is thus important to understand which factors contribute to initiating criminal behaviour. It is often argued that labour market conditions and experiences - legal opportunities to generate income - are one important factor. In particular, various negative consequences arise in association with unemployment: Whilst high unemployment rates for example lead to higher costs for the social security systems, the individual experience of unemployment might trigger financial constraints, social stigma and a decline in general wellbeing. That in turn might trigger criminal behaviour. Thinking of crime as one pillar in an occupational choice system, reservation wages or search intensities for legal employment opportunities may then be affected by crime, either as a potential alternative source of income or by the time and effort already spent in the search for crime opportunities.

In the following I discuss the existing literature and findings in that field of research. This survey focuses on theoretical and empirical work on crime and labour markets in order to detect and outline gaps in the literature and open research questions. The remainder of this chapter is structured as follows: Section 2.2 reviews theoretical models in the literature. In particular, I outline the benchmark model of crime and further discuss dynamic economic models as well as related job search models. Section 2.3 refers to the empirical literature. The particularities of the empirical research on crime and labour markets and examples of such research are discussed. Section 2.4 concludes.

2.2 Theoretical Economics of Crime

The theoretical literature on the economics of crime provides valuable insights for understanding criminal behaviour. Studies have focused on understanding and modelling crime incentives and the interaction between criminal and non-criminal behaviour, as well as on crime-related policy and the economic cost of crime. In the following, I describe the benchmark economic model of crime before discussing extensions that focus on different aspects of criminal behaviour.

2.2.1 Benchmark Economic Model of Crime

The benchmark economic model of crime is based on the Becker (1968) seminal model: A rational decision maker chooses between legal and criminal activity, maximising his expected utility from either option. The choice is subject to uncertainty with respect to the gains and losses from legal and criminal activity. If the crime is successful, the individual gains in utility, while an unsuccessful crime leads to utility losses in terms of punishment.

The choice under uncertainty can be formalised in a model as follows (Becker, 1968, Freeman, 1999)[1]: Let Π_c denote the gain from successful criminal activity, w^l the earnings from legal activity (here: employment), p the probability of being caught when committing a crime and P the extend of punishment after being caught for crime. In this framework, the expected utility of crime and employment, respectively, can be written as:

$$\mathbb{E}(U_{crime}) = (1-p)\cdot U(\Pi_c) - p\cdot U(P) \tag{2.1}$$

$$\mathbb{E}(U_{empl}) = U(w^l) \tag{2.2}$$

According to expected utility theory, a rational decision maker in that model commits a crime if and only if (2.1) exceeds (2.2), i.e.:

$$(1-p)\cdot U(\Pi_c) - p\cdot U(P) > U(w^l) \tag{2.3}$$

The model has proven very useful in highlighting features of criminality: The decision on criminal activity depends interactively on the probability of being caught, on the extend of the potential punishment, on the gains from crime and work as well as on the individual's utility function. Yet, the model is based on a number of assumptions that may limit the conclusions that can be drawn from it.

[1] In order to provide consistency in notation throughout this dissertation, the notation has been adjusted compared to the original notation in the cited articles.

First, the model is explicitly static and does not allow for a more dynamic consideration of the crime decision process. That constraints the understanding of criminal decisions to contemporaneous incentives, but leaves out important dynamic factors such as those discussed in subsequent chapters of this dissertation. Second, the model is based on the rationality assumption that underlies expected utility theory. In the crime context, one may question that assumption: If moral concerns play a role over and above what is captured by the utility function, the rationality assumption may not be plausible. Third, the model is based on a binary choice between work (legal activity) and crime (illegal activity). Yet, other than time constraints, there is no obvious reason for the binary choice assumption to hold: A working individual is not inhibited from committing a crime other than by allocating time to legal and illegal activity, respectively. Models that incorporate an extension in that sense are discussed later in this chapter.

Despite these limitations, the model allows useful insights and provides a baseline microeconomic model for understanding criminal decision processes and the effect of criminal justice policies such as policing, sentencing, or imprisonment. In that sense the model has proven to be a starting point in many empirical analyses in the economics of crime. Also, the model has subsequently been extended to incorporate any of the reflections above.

Ehrlich (1973) relaxes the binary choice assumption and suggests a time allocation model with three possible activity states. In that model, an individual chooses between a non-risky, legal market activity (choice l), a risky, illegal market activity (choice c) and a non-market activity as for example consumption or child care (choice n). In addition, there are two states of the world: Either crime is followed by punishment at the end of the time period (state 0) or it is not (state 1) where only illegal market activity depends in its outcome on the state of the world. Here, the individual's decision regards the optimal time allocation with respect to the three non-exclusive activity states. Importantly, an individual can pursue legal, illegal and non-market activity simultaneously and is limited in his choice only by time constraints.

Let X_0 and X_1 denote composite goods in both states of the world, respectively. These goods are composed of the value of the individual's assets and the returns from legal as well as illegal market activity, where the latter depends on the state of the world. Let t denote the individual's time constraint, and t_l, t_c and t_n the time spent in each of the three respective activities. The rational expected utility maximiser solves the following optimisation problem (Ehrlich, 1973):

$$\max_{t_l,t_c,t_n} \{\mathbb{E}(U(X_0,X_1,t_n)) = \mathbb{P}(\text{state } 0)\cdot U(X_0,t_n) + \mathbb{P}(\text{state } 1)\cdot U(X_1,t_n)\}$$
$$\text{s.t. } t = t_l + t_c + t_n, t_l \geq 0, t_c \geq 0 \text{ and } t_n \geq 0 \tag{2.4}$$

That model captures a more realistic feature of criminality and the choice of criminal activity. Criminal activity is a very risky alternative to legal employment with risk arising from two different sources: First, there is a positive probability of being caught and punished. Second, there is uncertainty with respect to the number of arising crime opportunities. In contrast to legal occupational choices, specialisation on crime might hence be suboptimal. That is reflected in the model which allows for non-exclusive activities and incorporates features such as risk attitudes, captured in the utility function, and punishment probabilities, captured in the probabilities of the different states of the world.[2] Analysing comparative statistics, Ehrlich (1973) derives behavioural implications of the model. Yet, the validity of these considerations is limited as the model - although providing new insights - is still relatively basic and relies on strong assumptions and parameter choices.

As discussed above, these seminal microeconomic models rely on strong theoretical assumptions which limit the validity over and above the theoretical framework. Yet, as Freeman (1999) points out, these rather simplified models allow us to focus on the allegedly most important parameters and features of criminal behaviour. The models have provided the micro-foundation of more sophisticated theoretical models, and have also proven to be an important starting point for a large body of empirical research.

2.2.2 Peer Effects and Social Interactions

What is the role of social interactions with respect to crime decisions? The benchmark model as described above does not take social interactions into account. Yet, recent happenings such as for example the London Riots in 2011 lead us to believe that social networks become more and more important in the crime context. It is thus important to understand the nature of network and peer effects in order to understand crime decisions and the organisation of criminal action. Economists have modelled these social interactions and peer effects in a game-theoretical framework. In the following, I outline examples of such models.

[2] Block and Heineke (1975) extend these ideas to derive a simple model of labour supply where labour supply decisions depend on wealth, the (stochastic) returns to crime, enforcement, as well as on the degree and the probability of punishment.

Sah (1991) provides a model of social interactions and crime focusing on the perception of punishment probabilities. In that framework, beliefs about punishment are updated according to the social environment. That means that higher crime rates lower the subjective probability for an individual to be arrested conditional on a fixed number of arrests: The criminal perceives the risk of being arrested to be spread out more the more individuals participate in the crime market. Based on this notion, higher crime rates might contribute per se to an increase in the uptake of criminal activity if indeed they affect individuals' subjective arrest probabilities.

Glaeser *et al.* (1996) use a social interactions model in order to explain differences in criminal behaviour over time and space. The authors build their model on the idea that individuals' social networks correlate with their crime decisions. Unlike Sah (1991), they study local instead of global interactions: Here, individuals are influenced by their direct environment such as local neighbourhoods, friends or family, rather than by global influences such as the aggregated crime and arrest rates. Glaeser *et al.* (1996) find that variations in the crime patterns between cities can partly be explained by differences in the local networks and interactions.

Calvó-Armengol and Zenou (2004) focus on the type of the social network and its impact on criminal behaviour. Here, the model is set up as a two-stage game: First, individuals decide exclusively on crime or labour. Second, they decide on how much effort they put into the criminal activity. Studying sub-game perfect Nash equilibria, Calvó-Armengol and Zenou (2004) find multiple equilibria that stem from different social network patterns rather than from different networks per se: Social networks, and in particular co-offending networks, yield positive as well as negative externalities. For example, positive externalities include knowledge spillovers and negative externalities the sharing of the loot. These externalities depend on the structure of the network: An individual who is connected to a wider network is faced with higher externalities compared to an individuals with fewer links. Calvó-Armengol and Zenou (2004) argue that these externalities drive the multiplicity of equilibria that result from their model, more so than the pure existence of the networks.

The model illustrates the importance of social networks as a feature of criminal behaviour as observed in recent developments.[3] Yet, it relies on relatively strong assumptions. First, the model is based on a mutually exclusive choice between crime and labour. As discussed above, this assumption is a simplification and might

[3] For example, during the London Riots in 2011 social media seemed to play a significant role in spreading information across networks. Indeed it would be very interesting to not only include the existence and the type of network in the model, but also to consider the speed of information sharing and the spatial implications for crime.

easily be violated. Moreover, employment itself can yield positive and negative externalities on criminal success: While legal employment might restrain individuals from crime by lowering incentives and imposing a time constraint, the workplace can also be thought of as a platform to gather information on future victims or co-offenders, or simply as a platform for crime opportunities. Incorporating these ideas into the model could lead to some interesting findings. Second, the model assumes reciprocal, i.e. symmetrical social interactions. That implies that the social networks are not hierarchical. Yet, that is a strong assumption with respect to criminal networks which, to the contrary, typically exhibit rather strong hierarchical patterns.

Using a similar approach as Calvó-Armengol and Zenou (2004), Calvó-Armengol *et al.* (2007) model the impact of strong ties (family and best friends) and weak ties (occasionally occurring meetings) on labour market and crime decisions. The authors develop a "waiting room" model in which unemployment is the "waiting room" for either employment and crime. Here, a criminal is defined as an individual looking for criminal opportunities only and spending no time on legal job search. An unemployed person is defined as an individual looking full-time for a job. An individual can make a transition from unemployment to employment or crime, and from employment or crime to unemployment. Transitions from crime to employment and vice versa are not possible. Hence, in addition to the assumption of a mutually exclusive choice between employment and crime the model imposes restrictions on transitions between the three states employment, unemployment and crime. Moreover, the model does not allow for information spillovers between peer groups with respect to employment opportunities while explicitly allowing for those spillovers with respect to criminal opportunities. That asymmetry implies that the informative effect of peers is, if at all, negative. While the assumptions of the model lead to simplifications, the model yet allows us to develop an intuition about the interactions between labour market outcomes and criminal behaviour. In particular, it may provide a valuable theoretical framework for empirical research on unemployment and crime such as described in subsequent chapters of this dissertation.

The models outlined so far are based on the idea that the affiliation to a peer group affects crime decisions. Yet, associated beliefs about an individual's peer group might play an important role, too. Verdier and Zenou (2004) provide a game-theoretical approach in order to explain why discriminated groups commit more crimes. The notion is based on labour market discrimination and spatial location: If members of peer groups with higher average crime rates receive lower wages by their employers due to statistical discrimination, these individuals can only afford to live further away from their workplace. That would be the case if a higher criminal propensity, as perceived by the employer observing the peer group, lowers the worker's productivity. Now, the distance to the workplace implies commuting and transportation costs, further reducing the net wage. Moreover, individuals living further away from the workplace may be faced with an additional wage cut if employers assume a decreased productivity due to tiredness after commuting. This spatial location effect lowers effective wages and increases the propensity of crime, driving self-fulfilling beliefs in equilibrium.

2.2.3 Dynamic Economic Models of Crime

One of the main drawbacks of the benchmark economic model of crime is that it is an explicitly static model. Yet, one might be particularly interested in the dynamics of criminal behaviour. In the following, I outline general equilibrium models as well as search theoretical models that incorporate dynamic features. These models moreover allow for simultaneous choices with respect to participation in the labour as well as the crime market.

General Equilibrium Models

Imrohoroglu *et al.* (2004) derive a dynamic general equilibrium model of crime with heterogeneous agents. Here, heterogeneity arises from deviating skill levels and abilities implying differences with respect to labour income and earnings. By assumption, individuals receive a stochastic employment opportunity, either employment or unemployment, in each period. Once they know about their respective state, i.e. employment or unemployment, they decide about savings as well as crime participation. The models allows for simultaneous participation in the legal as well as the illegal market: Individuals can commit crime being employed or unemployed, and they can become a victim of crime in any state. The authors derive the budget constraints related to these assumptions, the dynamic programming problem as well as a stationary equilibrium definition.

While Imrohoroglu *et al.* (2004) introduce skill heterogeneity with respect to legal labour markets, Mocan *et al.* (2005) extend that idea by suggesting a dynamic model of differential human capital and criminal activity. Here, individuals are endowed with legal and criminal activity specific human capital which evolve over time. Hence, expected income from either employment or crime now depends on both types of human capital. The authors derive a model in which during unemployment the legal human capital stock and returns to legal human capital fall and involvement in criminal activity rises. That in turn leads to an accumulation of criminal know-how, and subsequently to increasing returns to crime. In this model, hysteresis occurs if the criminal human capital stock grows sufficiently large as for example during a deep recession or long-term unemployment. The authors' findings are very useful in understanding the asymmetric relationship between unemployment and crime where the increase in crime during a recession is typically larger than the decrease in crime after a recession.

Search-Theoretical Models

How do individuals initially decide about accepting or refusing job and crime opportunities? Burdett *et al.* (2003) use a search-theoretical framework in order to model these decisions. The authors extend the Burdett-Mortensen wage posting search model, developing a search equilibrium model incorporating crime choices. Here, the reservation wage is defined as an individual's threshold wage in order to accept a job offer. Similarly, the model features a crime reservation wage above which the opportunity cost of crime is sufficiently high such that individuals do not commit crime. The model relies on the standard assumptions of a search-theoretical model. In addition, the authors impose the assumption of a crime-employment-dichotomy such as discussed above.

The model leads to four different steady state equilibrium scenarios. If unemployment benefits in an economy exceed a certain threshold, then no individual commits crime in equilibrium: The opportunity cost of committing a crime is too high, even for the unemployed. Else, there are three scenarios: Only unemployed individuals commit crime if the wage for the employed is above the crime reservation wage; everyone commits crime if the wage is below the crime reservation wage; else all unemployed and some employed individuals commit crime. As Burdett *et al.* (2003) conclude, that multiple equilibrium result may explain spatial heterogeneity in crime.

In a follow-up paper, Burdett *et al.* (2004) introduce on-the-job-search into the model. The respective equilibrium outcome changes: While in the previous model the possibility of criminal activity generates a dispersion of wages around the reservation wage and the crime reservation wage, here there is additional wage dispersion above the reservation wage or the crime reservation wage.

As in the case of the static models, the assumption of a mutually exclusive choice between employment and crime limits the validity of these models. Based on a Pissarides search-theoretical framework, Engelhardt *et al.* (2008) develop a theoretical model in which outcomes on the labour as well as the crime market are jointly determined. The model is based on a standard search model extended by a crime component. Here, crime opportunities arrive randomly by assumption. Their arrival probability depends on the individual's state: Unemployment, employment or prison.[4] The authors derive the Bellman equations for the respective states, as well as the decision rules for committing crime. Moreover, they derive an optimal contract result, arguing that optimal contracts need to enforce individuals to internalise negative externalities of criminal activity. Here, negative externalities are caused by losses for the employer once the individual commits crime. For example, if the criminal worker is sentenced to prison, the employer-employee match is destroyed implying costs for the firms looking for a new match. In contrast to the models above, the authors do not find multiple equilibria.

Criminal behaviour in this model is an endogenous decision which depends on the labour market status. In particular, the decision between crime and labour market participation is non-binary: While unemployed individuals are assumed to have a higher propensity to commit crime than employed individuals, criminal opportunities do not *only* arrive to the unemployed. This is an important extension compared to previous models.

Building on Burdett *et al.* (2004) and Engelhardt *et al.* (2008), Engelhardt (2010) extends the on-the-job-search model, allowing for additional heterogeneity: Individuals differ in their valuation of leisure time and firms differ in their productivity. The author assumes that there are two types of workers with high or low leisure time valuation, and two types of firms with high or low productivity. As before, there are three distinctive states: Unemployment, employment and prison. Deriving the Bellman equations for each state, Engelhardt (2010) establishes two reservation wages which indicate the threshold wages for accepting a job or crime opportunity, respectively. Further, there is a leisure time valuation threshold below which individuals do and above which do not commit crime.

Engelhardt (2010) points out that only two states, employment and unemployment, lead to an optimal choice problem with respect to crime or employment decisions. Prisoners, however, are assumed to only wait for release from prison. This assumption simplifies the theoretical model, but may be violated in empirical research mainly for two reasons: First, prisoners optimise their behaviour. Good conduct in prison is subject to effort (cost), but increases the probability of early release (benefit). Second, prisoners optimise their participation and effort in in-prison

[4]Here, the crime opportunity arrival rate equals zero for individuals in prison. However, this assumption might be violated if indeed criminal human capital is accumulated in prisons ("universities of crime") and criminal networks are built. This would impact on the future, if not the present, crime opportunity arrival rate.

training programmes or day-release job opportunities, as far as available.[5]

Similarly to Burdett *et al.* (2003), Engelhardt (2010) finds three equilibria. First, high-productivity *and* low-productivity firms offer wages which are high enough such that employees commit less crime than unemployed individuals. Second, only high-productivity firms offer wages which are high enough such that employees commit less crime than unemployed individuals or the low-productivity firm employees. Third, wages and labour market status do not influence the crime propensity at all, leading to an equilibrium in which all individuals commit the same amount of crime. Here, the role of leisure valuation is not obvious. Considering only two types of individuals with fixed leisure time valuations, one type committing crime and the other type never committing crime, the equilibria are tractable. Yet, it is not obvious that the third case holds if valuations were continuously measured. In particular, if the crime propensity depends on the individual's leisure time valuation, the amount of time already spent in employment matters. Regarding the crime decision, the cost of lost leisure time during prison time is important. Hence, emphasising the interaction of crime and labour supply decisions could be an interesting extension of the model.

2.3 Empirical Economics of Crime

There is a large body of literature on empirical economics of crime. That literature faces particularities that are discussed in the following. First, sources and challenges concerning crime data are described. Moreover, I discuss different sources of endogeneity which may arise in addition to concerns about data availability and quality. Second, I present examples of empirical research on the economics of crime with a particular focus on labour markets.

2.3.1 Crime Data and Sources of Bias

Crime data come from a variety of sources each with respective advantages and disadvantages. Conventional sources include administrative crime records, data on prisoners, convictions as well as victimisations and crime surveys. Administrative data sources include reported crime only and thus suffer from measurement error with respect to the true number of crimes. While victimisation data are subject to a similar concern, they still offer an improvement in that respect. However, that type

[5]This type of optimising behaviour might be particularly important for young prisoners whose education and employment biographies are short. Indeed, it would be very interesting to empirically evaluate such programmes with respect to labour and crime market outcomes.

of data typically does not provide any information on the offender. Self-reported crime captures criminal action that does or does not show in administrative records, however suffers from survey response bias.[6] Further, researchers have employed less conventional approaches to collect data on crime. One such example are Krueger and Pischke (1997) who use newspaper reports on crime against foreigners in Germany in order to construct a data set. Yet, these approaches can be very costly and their feasibility in research therefore restricted.

Observational levels of crime vary among the data sources. For example, in some countries individual level register data including criminal records are available whereas in other countries that type of information is only accessible at an aggregated level. While individual level data have a clear informational advantage over aggregated data, one still needs to take into account the statistical implications of crime being a low probability event. Aggregation at the spatial, temporal or demographic level partly overcomes that issue and also typically allows to add variables from other data sources, one such example being labour market information. Yet, estimations based on aggregated data have to be interpreted according to the particular statistical and behavioural assumptions in order to avoid misinterpretations (Durlauf *et al.*, 2010).

Unobserved Heterogeneity

Standard cross-sectional or time-series estimations using crime data can suffer from unobserved heterogeneity at the spatial, temporal or demographic level. Individuals differ in their attitudes towards crime; particular regions and time periods may be subject to specifics which are not observed, but which correlate with crime. Panel data allow to control for this unobserved heterogeneity by means of fixed effects estimation.[7] Yet, even fixed effects estimations may still be biased (Levitt, 2001, Bjerk, 2009). In particular, fixed effects estimation relies on the assumption of exogeneity, i.e. that the assignment to a treatment of interest happens at random. If that assumption does not hold, the estimates of a fixed effects estimation model are biased.

Omitted Variables

Related to that is the problem of omitted variables. Which are the contributing factors to crime? If variables which correlate with crime as well as with the treatment of interest are omitted, the estimates of the respective model are biased. The

[6] See for example Tabarrok *et al.* (2010) for a more detailed discussion of that point.
[7] See for example Cornwell and Trumbull (1994) for an early discussion.

problem of omitted variables results from a lack of knowledge about contributing factors to crime on the one hand, and from a lack of data on the other hand. For example, Raphael and Winter-Ebmer (2001) and Entorf and Winker (2008) discuss the inclusion of procyclical effects; Mustard (2003) discusses the effect of omitted variable bias in the context of arrest rates, conviction rates and sentence lengths, the latter often being neglected in the literature.

Simultaneity and Reversed Causality

One of the main challenges in identifying causal effects on crime is to overcome simultaneity or reversed causality issues: What is the causal effect of police on crime if police presence is higher in high crime areas? What is the impact of low wages on criminal activity if employers in a region with higher crime rates pay lower wages, because they take potential cost for criminal employees into account? What are the implications of unemployment for crime participation if employers avoid regions with higher crime rates which increases local unemployment rates per se? If these concerns are not taken into account, estimation results are biased. A priori, there is no consistent opinion about the general direction of the bias which on the contrary depends on the specific research question (Mustard, 2010).

In so-called reduced form empirical research, quasi-experimental methods on the one hand and instrumental variable methods on the other hand offer a solution to overcome endogeneity bias. These econometric models are very useful in order to retrieve causal estimates, however are subject to two major drawbacks:[8] First, it can be hard to find a suitable quasi-experiment or instrumental variable. Second, the external validity of quasi-experiment or the instrumental variable approach can be limited to the particular institutional setting.

2.3.2 Empirical Evidence

In the following, I discuss examples of empirical research on the interaction between labour markets and crime. One can think of a number of mechanisms through which criminality relates to labour market outcomes and formulate respective hypotheses. First, there is a scarring effect of crime: Individuals with criminal records are faced with tougher conditions on the labour market than their non-criminal counterfactuals. Second, unemployment increases criminal behaviour: Employment is substituted by criminal activity in terms of time use and income generation. Third, low wages trigger criminal behaviour: Low wage earners show

[8] See for example Levitt (2001).

a higher propensity to generate additional income from criminal activity. In the following, I discuss examples of empirical research on these hypotheses. Moreover, I outline empirical research that has been undertaken in order to understand the interactions between education and crime as well as peer effects on crime.

Scarring Effects of Crime

There is a large body of evidence on scarring effects of criminal records on labour market outcomes such as employment and wages. An early and prominent example is found in Grogger (1995), studying the question whether arrests cause a decrease in earnings and employment or whether the statistical correlation is driven by unobserved characteristics of the arrestees.[9] If these underlying characteristics jointly correlate with individual crime propensity and labour market performance, the estimated scarring effect is biased. Grogger (1995) estimates a fixed effects model exploiting longitudinal data in order to identify the scarring effects of arrests on labour market outcomes (see the discussion above).

The study uses longitudinal earning records for the years 1980-1984, provided by the California Development Department, which are matched to longitudinal criminal justice records from the California Justice Department's Adult Criminal Justice System. In line with the literature the author restricts his analysis to young males, as this group is the most prone to criminal activity. He defines the treatment group as individuals who have been arrested in 1984 or earlier, and the control group as individuals who have been arrested in 1985 or later. Using a distributed lag model, he estimates the following equation for individual i at time t where y denotes the labour market outcome (earnings, employment), A the arrest records, X a set of control variables, μ the fixed effect and η the residual:

$$y_{it} = \sum_{j=0}^{m} A_{it-j} \cdot \beta_j + X_{it} \cdot \delta + \mu_i + \eta_{it} \tag{2.5}$$

Grogger (1995) finds significant evidence that arrest records decrease both employment probabilities and earnings. The effects are fairly moderate in their magnitude: Initially there is a 4 per cent decrease in earnings which falls to about 2 to 3 per cent, and eventually fades out. More recent studies support the notion of a scarring effect of a criminal record on labour market outcomes although, not surprisingly, magnitudes differ (e.g. Kling (2006) or Baert and Verhofstadt (2015)).

[9] These correlated, unobserved characteristics actually open ground for new research questions. Which factors precisely are linked to both labour market and crime outcomes? This would be highly interesting in the context of job search behaviour and crime prevention.

Unemployment and Crime

The relationship between unemployment and crime has been subject to a large number of studies. From the theoretical models, crime can be understood as an alternative to employment where an unemployed individual substitutes legal employment by illegal activity. Thus, one might expect compelling empirical evidence that unemployment increases crime. Yet, as Freeman (1999) writes:

> "Thus, unemployment is related to crime, but if your prior was that the relation was overwhelming, you were wrong. Joblessness is not the overwhelming determinant of crime that many analysts and the public a priori expected it to be. Why?"

Even almost ten years later, that puzzle remains and the findings differ in magnitudes, statistical precision and even in signs (Mustard, 2010):

> "This literature, which is decades old and contains hundreds of papers, is characterized by an intriguing puzzle – the large gap between the theory and empirical work."

In the following, I discuss examples of the existent literature on unemployment and crime. Two subsequent chapters of this dissertation contribute to that body of research.

The paper by Raphael and Winter-Ebmer (2001) is based on the Becker-Ehrlich paradigm with a time allocation model of crime and employment. They use a state-level panel for 50 U.S. states from 1971-1997 with data from the FBI Uniform Crime Reports, the U.S. Census and the Bureau of Labor Statistics. In order to identify the causal effect of unemployment on crime, Raphael and Winter-Ebmer (2001) employ two different empirical strategies: First, they estimate an OLS model and tackle the problem of unobserved heterogeneity by extensively including control variables and adding state and year fixed effects. Second, they estimate an instrumental variable model with military spending and state-specific measures of oil price shocks as instruments. Their baseline estimation equation for state i at time t reads as follows, where C denotes the crime rate, α and δ the fixed effects, t and t^2 a quadratic time trend, UR the unemployment rate, X a set of control variables and η the residual:

$$C_{it} = \alpha_t + \delta_i + \psi_i \cdot t + \omega_i \cdot t^2 + \gamma \cdot UR_{it} + \beta \cdot X_{it} + \eta_{it} \tag{2.6}$$

The authors find that the OLS estimation yields elasticities of the property crime rate with respect to the unemployment rate between 1.6 and 2.4 per cent, and more ambiguous results for violent crime. The estimates from the instrumental variable estimation exceed the OLS estimate in magnitude (2.8 to 5.0 per cent) for property crime and are more ambiguous for violent crime.[10]

More recently, Lin (2008) estimates the causal effect of unemployment on crime using data from the FBI Uniform Crime Reports, too, however over a longer and more recent time span. His identification strategy relies on an instrumental variable framework with changes in the real exchange rate, state manufacturing sector percentages, and state union membership rates as instrumental variables. He finds elasticities of the property crime rate with respect to unemployment of 4 to 6 per cent (compared to a 1.8 per cent OLS estimate), and no significant impact on violent crime.

Fougère *et al.* (2009) study the relationship between crime rates and (youth) unemployment in France. The authors construct a panel of reported crimes for the 95 French departments from 1990 to 2000 and match labour market data from the French Labor Force Survey and the French Public Employment Service. Similar to the previously discussed papers, Fougère *et al.* (2009) begin their analysis with a fixed effects estimation. Their baseline estimating equation for department i and time t reads as follows, where C denotes the crime rate, X a set of control variables, UR the unemployment rate, α and δ the fixed effects and η the residual:

$$C_{it} = X_{it} \cdot \beta + \gamma \cdot UR_{it} + \alpha_i + \delta_t + \eta_{it} \tag{2.7}$$

As discussed previously, the results from the fixed effects estimation may be biased. In order to retrieve causal estimates, Fougère *et al.* (2009) additionally estimate an instrumental variable model. Their findings suggest that youth unemployment, but not overall unemployment, has a positive causal effect on most property crime rates which is robust to a number of tests.

While the studies cited above rely on spatially aggregated data, Grönqvist (2013) uses individual level data from Swedish administrative registers in order to estimate the effect of youth unemployment on crime. He finds sizeable effects on all types of crime, but in particular for income generating crimes such as property crime and drug crime. The magnitudes of the effects exceed the magnitudes found in studies relying on aggregated data. As Grönqvist (2013) points out, this is likely due to the fact that the individual level estimation allows to disentangle the behavioural response to unemployment from any underlying general equilibrium effects.

[10] In the literature on unemployment and crime, there typically is a downward bias of the OLS estimates compared to IV estimates. This is what is expected following from the reversed causality problem.

Two chapters of this dissertation contribute to the literature on the relationship between unemployment and crime. While the following chapter studies scarring effects on crime of graduating in times of weak labour markets, the subsequent chapter studies the causal effect of unemployment and crime in the context of the Great Recession and explicitly looks at the role of unemployment durations.

Low Wages and Crime

One of the mechanisms behind the unemployment-crime relationship is time reallocation from legal to illegal activity. Another mechanism refers to income generation. The hypothesis is the following: Unemployment lowers an individual's income and hence triggers criminal behaviour. Now, if that is the case one would expect low wage earners to have a higher crime propensity than higher wage earners, too.

An early approach to this question is found in Grogger (1998). He estimates the impact of the market wage on crime rates, using U.S. individual level data from the National Longitudinal Survey of Youth. The underlying hypothesis suggests that crime which is triggered by low wages is income related, and hence only property crime is considered. Grogger (1998) derives three equations from a theoretical, time allocation model: A wage equation, a crime participation function and the consumer's marginal rate of substitution (labour supply). Let w_l denote the wage, w_c the returns to crime, w_n non-market income, t the allocated time, and η the residual:

$$\ln w_l = X_1 \cdot \beta_1 + \eta_1 \tag{2.8}$$

$$\ln w_c = X_2 \cdot \beta_2 - \alpha_2 \cdot t_c + \eta_2 \tag{2.9}$$

$$\ln MRS = X_3 \cdot \beta_{31} + \beta_{32} \cdot t_l + \beta_{33} \cdot (w_n + w_c) + \beta_{34} \cdot (t - t_c) + \eta_3 \tag{2.10}$$

Estimating the model, Grogger (1998) finds a robust, causal effect: Lower wages increase the crime propensity of young men. He draws two main conclusions from his findings. First, the differential in the share of low wage workers between black and white young men can partly explain the gap in the respective crime rates. Second, if low wages increase crime propensities and if wages increase with age, then the findings can contribute to understanding the strong crime-age patterns which are typically observed in the data. Yet, it is unlikely that low wages can fully explain the crime-age pattern. For example, peer effects which are stronger at young ages or family commitments which are more significant at higher ages, might constitute additional factors. Here, neither peer effects nor home production time are incorporated into the model and thus the findings have to be interpreted as partial effects.

Note that the model explicitly allows for the possibility that employed individuals commit crime. Indeed, descriptive statistics show that 94.5% of the young men in the sample who were generating income from criminal activity were employed at the same time (Grogger, 1998). This number is rather striking and one might have expected the share of individuals who commit crime to be significantly higher among the unemployed than among the employed. These findings support the hypothesis that low wages may be a stronger predictor of crime than unemployment.

Gould *et al.* (2002) argue that wages measure the opportunity cost of crime and should therefore be a valid predictor for criminal propensity. Three mechanisms can be distinguished. First, there is a substitution mechanism: If legal and criminal activity are substitutes, lower wages incentivise individuals to substitute work by crime. Second, there is an income mechanism: If wages are low, individuals are incentivised to top up their income from employment by income from criminal activity. Third, there is an inverse deterrence mechanism: If the wage represents the opportunity cost of crime after being caught and punished, lower wages increase the propensity of crime relative to higher wages.

In their paper, Gould *et al.* (2002) study the causality between increasing crime rates and decreasing wages for low-educated, low-paid workers. Their empirical strategy is based on three distinct approaches: First, they use annual county-level data from 1979 to 1997 to estimate a fixed effects model of the aggregated crime rate on state-level average wages and unemployment rates for non-college educated men. Second, they estimate the model using ten-year changes both in the crime rates as well as the averages wages and unemployment rates, now at the metropolitan area level. Third, they use individual-level data and include control variables for personal characteristics. OLS as well as instrumental variable estimations are carried out, with area-specific industrial composition, aggregated industrial trends as well as aggregated demographic changes in industries as instruments.

The findings in the paper suggest that negative wage trends account for more than 50% of the increase in crime rates (Gould *et al.*, 2002). The results are robust across the specifications and estimation methods. The authors point out that crime rates are long-term indices, and hence are more likely to be affected by long-term wage trends than by unemployment. This notion leads to the question whether unemployment in itself differentially affects crime: What is the effect of short-term unemployment versus long-term unemployment on crime? Is the stock of unemployed or the inflow into unemployment the more important predictor of crime rates? These questions are addressed to an extend in the subsequent chapters of this dissertation.

Machin and Meghir (2004) study the effect of aggregated wage rates on the propensity for crime in the case of the United Kingdom. They formulate a choice model of employment and crime and derive value functions for the four respective outcomes: Employment, crime, employment and crime, or neither employment nor crime. The expected value of any of these outcomes is scaled by the probability of not being caught after having committed a crime. Further, the propensity of crime depends on the returns to crime and to employment as well as on the amount of unemployment benefits, albeit not on the unemployment status per se.

The empirical analysis exploits a panel of recorded property and vehicle crime offences from police force area data (Criminal Statistics by the Home Office), containing 42 areas in the United Kingdom between 1975 and 1996. Wage information from the New Earnings Survey (NES) is matched by year and area. Following the notions from the theoretical model, low-wage workers are more likely to commit crime than high wage workers. Machin and Meghir (2004) consider the 25th percentile of the wage distribution in each area as a proxy of the aggregated low wage rate. Deriving the logistic probability for each of the four states leads to an expression of the probability of criminal activity. Aggregating by police force area and taking log odds ratios, a linear approximation of the crime rate for police force area i and time t is derived, where C denotes the crime rate, α and δ the fixed effects, r the returns to crime, w the returns to employment, b the benefit transfers, p the apprehension probability, X a set of control variables and η the residual:

$$\begin{aligned}\ln\left(C_{it}/1-C_{it}\right) = \alpha_t + \delta_i + \gamma_1 \cdot r_{it} + \gamma_2 \cdot w_{it} + \gamma_3 \cdot b_{it} + \gamma_4 \cdot C_{it-1} \\ + \gamma_5 \cdot p_{it} + \beta \cdot X_{it} + \eta_{it}\end{aligned} \tag{2.11}$$

Fixed effects and instrumental variable estimations are carried out and yield robust, significant negative wage effects with marginal effects between -0.066% and -0.096% for property crime and smaller but still significant effects for other crime types. As the authors point out, these effects are sizeable and again suggest that low wages act as a better predictor for crime than unemployment. Yet, compared to other European countries during that time period, long-term unemployment was to a lesser extent a worry in the United Kingdom. That raises similar questions as above: What is the impact of unemployment and low wages on crime in economies that are faced with generally longer unemployment spells? A cross-country analysis could yield highly interesting insights, although such a study would of course be challenging with respect to identification.

One of the mechanisms behind the increase in crime with lower wages is the income mechanisms outlined above. Further, one can argue that jobs at the very low end of the wage distribution are less time consuming than the jobs at the very top of the wage distribution. While part of that is a selection effect, there might be an additional time use or inverse incapacitation effect: Would the same individual commit less crime if he was in the same low paid job, but for more hours a week? Distinguishing these effects would be highly policy relevant.

An example for a continental European country analysis is the paper by Entorf and Spengler (2000). Using state level panel data form Germany[11], they explain crime rates by different determinants including demographic and urban factors, relative income and (youth) unemployment. Their empirical strategy relies on a static regression on the one hand, and a dynamic error correction model including state fixed effects on the other hand. Descriptive statistics suggest that crime rates are substantially higher in East German states than in West German states. Moreover, the estimations yield ambiguous results in terms of the impact of unemployment on crime, but also suggest a crime differential between East and West Germany. The authors do not have a consistent explanation for that differential which prompts additional research questions: To what extent are crime rates in East German regions higher due to a cultural or economic shock after the reunification? To what extent can weaker labour markets in East Germany after 1990 explain the relatively high crime rates? Or, alternatively, are there unobserved behavioural responses and changes in the perception of social justice after the reunification which was accompanied by a change in political and cultural paradigms? These are research questions that would be very interesting to tackle in future research.

Education and Crime

So far, I have discussed interactions between labour markets and crime. Education as a predictor of labour market outcomes is another important factor in order to explain criminal behaviour. In the following, I discuss examples of empirical research that is tailored towards quantifying the causal impact of education on crime.

Lochner (2004) derives a life-cycle time allocation model of crime and human capital investment. Individuals decide between crime, work and human capital investment while optimising their expected lifetime utility. He points out that wages

[11]The sample includes West German states from 1975 to 1996, and East German states from 1993 to 1996. Due to data validity concerns before and right after the reunification, the authors include East German states only from 1993 onwards.

as a predictor of crime neglect the fact that in particular the youth are still in a human capital investment period and do not yet receive the full returns to education. In particular, he establishes a causality between the skill and education levels and higher crime rates among young, low-skilled men.

In order to account for potential sources of endogeneity, Lochner (2004) distinguishes between high-skilled and low-skilled crime. Intuitively, the propensity for unskilled crime is negatively correlated with the current skill level: Higher skill levels potentially translate into higher wages which in turn implies high opportunity cost for unskilled crime. Yet, an increasing number of unskilled crime opportunities lowers the incentive for human capital investment. These mechanisms are less obvious for high-skilled crime, and one might in the extreme case even expect a positive correlation between skill levels and high-skilled crime propensities.[12] Using data for the U.S., Lochner (2004) finds a strong, negative effect of education on crime, confirming the human capital hypothesis. Moreover, he finds a strong, negative effect of cognitive ability on low-skilled crime as well as a positive effect of educational attainment on high-skill crime.

In line with these findings, Machin *et al.* (2011) establish three channels of educational impact on crime: Income effects driven by wages and opportunity cost of crime, time availability effects or incapacitation effects as well as patience and risk aversion effects.[13] Using data for the United Kingdom, they follow a similar empirical strategy to Lochner and Moretti (2004): In a quasi-experimental setting, they exploit variations in education due to legal changes in compulsory school leaving ages. Their strategy identifies the local effect of the additional year in education on crime for the lower tail of the education distribution.[14] The authors find a robust and significant negative effect of education on the property crime rate, a similar result to what is found in Lochner and Moretti (2004). Moreover, the results are in line with those found by Meghir *et al.* (2011) who use a compulsory school leaving age reform in Sweden to identify the relationship between educational attainment and crime, as well as parental educational attainment and crime. In both cases, they find a strong negative causal effect. The external validity of the quasi-experimental approach, which identifies local treatment effects, is in general limited to particular institutional settings. Yet, quasi-experimental research from different countries yields coherent conclusions so far.

[12] That might be the case for example for white-collar crimes such as tax evasion or corruption

[13] As known from the literature on the economics of education, there is a positive correlation between higher educational attainment and patience.

[14] The lower tail of the education distribution is likely to represent the relevant treatment group: Individuals who are affected by the legislative changes, as they would otherwise have dropped out of school.

Peer Effects and Crime

What is the empirical evidence for peer effects among offenders? The identification of peer effects in empirical economics of crime is threatened by the Manski reflection problem (Manski, 1993): Is the individual influenced by the peers or the peers by the individual? In other words, is the group of peers more likely to commit crime because of a criminal member or is that individual more likely to commit crime because of the group of peers? Glaeser *et al.* (1996) address the question theoretically as discussed above and empirically. In particular, they investigate to what extend the spatial variance of crime rates can be explained by social interactions, or rather by unobserved heterogeneity. In their empirical strategy, they use data from the FBI Uniform Crime Reports and the New York City Police Department and decompose the gap between actual and predicted crime rates. The results suggest that social interactions matter and account for the variance of inter-urban differences in crime rates.

Interestingly, the authors find that youth crime shows more dependency on social interactions. Moreover, having established the relevance of social interactions, Glaeser *et al.* (1996) point out that it is important to understand the underlying mechanisms. Discussing several candidates, they conclude that family instability is one likely medium, rather than schooling, unemployment or arrest rates. This being an interesting suggestion, the question about the precise channel remains: Are stronger moral or ethical ties important? Does family stability influence a individual's risk behaviour or opportunity cost of crime? Does family stability imply stronger commitments and therefore does it matter for time allocation?

2.4 Conclusion

This chapter reviews the existent literature on the economics of crime and labour markets. Moreover, it identifies gaps in the literature and open research questions. It focusses on theoretical and empirical dimensions of the interaction between crime and labour market opportunities. The literature on the relationship between crime and labour markets yields evidence across countries that supports the hypothesis that labour markets indeed matter for crime outcomes. Still, there are open research questions with respect to underlying mechanisms as well as the magnitudes of the effects. Both of these are crucial for broader policy implications.

Chapter 3. Crime Scars: Recessions and the Making of Career Criminals

This chapter is based on the discussion paper "*Crime Scars: Recessions and the Making of Career Criminals*", available in the IZA, CEP and CEPR discussion paper series. I would like to thank my co-authors Brian Bell and Stephen Machin for their contributions.

3.1 Introduction

Do the labour market conditions which the young encounter when they first leave school play a role in initiating and forming criminal careers? Think of two otherwise identical school leavers who leave high school in 2010, one in North Dakota and the other in Michigan. Whilst both have completed education and try to get a job, the North Dakota school leaver faces a state unemployment rate of only 3.8 percent compared to 12.7 percent in Michigan. At the margin, the Michigan youngster is more likely to proceed down the wrong path (no luck finding a job, no welfare to fall back on, hanging out with similarly unfortunate juveniles, trouble with the police, some petty larceny etc.) than the North Dakota youngster.

Indeed, this is just the benchmark Becker (1968) model in action: As youths leave school, they face a trade-off between legal and illegal activities. At higher unemployment rates, the expected returns to legal activity, i.e. work, are lower. All else equal, this encourages some youths to commit crime that would otherwise have successfully avoided such a result in a more buoyant labour market.

What might happen as these same youngsters age? Two obvious mechanisms link the experience when they enter the labour market with later ones. First, earlier experiences of crime might increase the stock of criminal knowledge and potentially reduce the costs of participating in subsequent criminal activity. Second, a previous criminal record, and thereby less on-the-job human capital accumulation, might reduce the expected wage in the legal labour market. Both effects can be expected to increase the likelihood that the individual eventually becomes a career criminal.

There is a substantial body of criminological evidence that points to the importance of the experience of youths for understanding crime patterns. Almost two hundred years ago, Adolphe Quetelet showed that crime in early nineteenth-century France peaked when individuals were in their late teens (Quetelet, 1831[1984]). Subsequent research has confirmed the strong age-crime pattern, with crime peaking in the late teens and declining quite rapidly after that.[1] Unsurprisingly, the same patterns emerge in our data. Figures 3.1 and 3.2 plot the average male offender rate by age for the U.S. and UK from 2000-2010.[2] The peak in the offender rate occurs at age 17 or 18 and declines reasonably smoothly from then on.

[1]See Hirschi and Gottfredson (1983) who develop the notion that crime-age profiles are invariant over time and space, and the subsequent body of research trying to refute this claim as for example Greenberg (1985), Hansen (2003) and a meta-study by Pratt and Cullen (2000).

[2]Full details on the data used in the charts are provided in the following sections of this chapter. The chart shows the average offender rate, measured in arrests for the U.S. and convictions in the UK, and defined as the number of offenders divided by the respective population in each age group. The data is averaged over the time period 2000-2010.

Note, however, that the offender rate at age 29 is still a lot higher than at age 39, showing that criminality is not *just* a feature of teenage years. Existing evidence points to strong links between criminality in teenage years and subsequent criminal behaviour.[3] We find that in our data, for example, 72 percent of males aged over 25 in the UK who were convicted of a crime in 2002 had a criminal record that went back to their teenage years. Thus, factors that increase criminal behaviour for juveniles have scope to raise the lifetime criminal participation rate. The focus of this paper is on whether the state of the labour market at labour market entry is such a factor.

In pursuing this research question, our analysis contributes to two distinct strands of literature. First, there has been an extensive, though partly unresolved, debate over the link between recessions and crime. This literature has primarily focused on the issue of whether crime rates, and in particular property crime rates, are countercyclical. The evidence tends to suggest that the place where one can identify effects from unemployment to crime is for young adults.[4] Thus, Gould *et al.* (2002) examine the impact of contemporaneous unemployment and wages on the criminal behaviour of less educated young males. Exploiting a panel of U.S. counties, they find significant effects for both wages and unemployment on property and violent crime. Fougère *et al.* (2009) find strong effects from youth unemployment, but not from overall unemployment, on crime in France, while Grönqvist (2013) uses Swedish register data to show a strong and precisely estimated link between youth unemployment and crime, both for property and violent crimes.

Second, there is a growing literature on the effects of first entering the labour market during recessions on outcomes later in life. That literature so far has focused on whether such workers experience sustained long-run negative consequences. Early contributions by Ellwood (1982) and Gardecki and Neumark (1998) find somewhat contrasting evidence on whether initial labour market experience affected subsequent outcomes, with Ellwood finding significant effects on wages but not on future spells of unemployment, while Gardecki and Neumark found little evidence of a sustained negative effect. More recently, Hershbein (2012) finds that a recession reduces starting wages of high-school graduates by about 6 percent, but that this penalty fades away within six years. Oreopoulos *et al.* (2012) exploit a large Canadian longitud-

[3]For more details, see for example the many papers which are cited in the review of Nagin and Paternoster (2000) that frames the positive link between past and future criminality in terms of individual heterogeneity and state dependence.

[4]Indeed, Freeman (1999) notes the relationship across the whole population to be "fragile, at best". More recent reviews confirm this, and therefore more focus can be placed on youth crime and unemployment in order to identify labour market effects on crime (see, for example, Mustard (2010) or Buonanno *et al.* (2011)).

inal dataset to show that the cost of a recession for new graduates is substantial and long lasting. A typical recession, defined as a 5 percentage point increase in the unemployment rate, is associated with an initial loss of earnings of about 9 percent that halves within 5 years, and eventually fades to zero by 10 years. The economic mechanism operates via initial placements with lower paying employers and succeeding recoveries through gradual job mobility to better firms. Graduates in the lower quintile of the ability distribution suffer permanently lower wages, while the more able graduates quickly bounce back. Similar results are reported by Kahn (2010) who uses longitudinal data on U.S. college graduates, though some of her results suggest that the wage penalty is longer lasting. By contrast, Benedetto *et al.* (2010) find no evidence of a persistent impact of graduation-year unemployment on earnings using U.S. social security earnings data.[5]

Taking a somewhat different approach, Oyer (2006, 2008) examines the career paths of particular occupations, namely economists and investment bankers, to assess the importance of initial labour market conditions. He shows that stock market conditions at the time of graduation have a strong effect on whether MBA students directly take jobs at Wall Street, or instead pursue alternatives such as jobs in consulting firms. Further, he shows that starting a career in investment banking directly after graduation causes a person to be more likely to stay in the job and earn significantly more. These effects are substantial in size, amounting to several million dollars in present value.

Outside of the labour market literature, labour market entry conditions have been shown to impact other later in life outcomes. Maclean (2013), for example, finds that males who graduate from high school during a recession show worse health outcomes at age 40 than those graduating in a more auspicious labour market. This is true for both self-reported health measures and objective measures of physical and mental health. Giuliano and Spilimbergo (2014) show that those who enter the labour market during a recession are more likely to believe that success in life depends more on luck than effort, and support more government redistribution. Again, these effects are seen to be long lasting. The protective effect of education for cohorts who graduate in recessions is studied by Cutler *et al.* (2015) in their analysis of Eurobarometer data. They report evidence of lower wages and life satisfaction together with higher obesity and a greater propensity to smoke and drink later in life for individuals who graduate in recession years, with higher education levels significantly moderating these negative outcomes.

[5] Also, see the international comparison of unemployment entry effects on labour market outcomes in the U.S. and Japan by Genda *et al.* (2010).

The results which are reported in this chapter uncover a more disturbing long-run effect of recessions. In the U.S. and in the UK, based on a variety of individual and cohort level data sources, we find evidence of a systematic empirical link between crime and entry-level unemployment. It very clearly shows that young people who leave school in the midst of recessions are significantly more likely to lead a life of crime than those entering a buoyant labour market. Moreover, these effects are seen to be long lasting and substantial. Thus, as other economic and social outcomes are significantly affected by the state of the business cycle at the time when individuals potentially enter the labour market, so is criminal activity. We conclude that recessions do play a role in the making of career criminals as crime scars from higher entry level unemployment rates are both long lasting and substantial.

The rest of the paper is structured as follows. In section 3.2 we discuss potential mechanisms that link initial conditions at labour market entry and the future path of criminal behaviour, as well as the underlying dynamics in order to motivate our empirical research. In section 3.3 we discuss the empirical strategy and the data sources both for the U.S. and the UK. We present the cohort panel results and individual-level evidence in sections 3.4 and 3.5, respectively. Section 3.6 concludes by summarising the key findings in this chapter.

3.2 Theoretical Background

In the standard Becker (1968) economics of crime model, individuals act as rational decision makers and choose between legal and illegal activity. Their choice is based on the expected returns to both options. In this simple yet powerful framework, returns to legal activity are solely determined by the market earnings from employment whereas returns to illegal activity take into account the potential crime payoff, the probability of getting caught and the expected sanction if caught. If the expected return to illegal activity outweighs the expected return to legal activity, the individual chooses to commit crime.

In the Becker model, higher unemployment reduces the returns to legal activity. Thus, individuals facing unemployment or higher risk of unemployment may become more likely to commit crime than they would have been otherwise. That effect is expected to be higher for youth who typically are less attached to the legal labour market than older individuals further on in their careers.

The model has proved valuable in highlighting the economic incentives associated with criminal activity and its basic predictions on incentive and deterrence effects on crime has received substantial empirical support.[6] Its weakness and limitation for our purposes is that it is explicitly static. Individuals make a one-off decision to commit crime or work in the legal sector. There is no process through which decisions made in the current period have implications both for future decisions and for the choices available to the individual in later periods.

Mocan *et al.* (2005) develop a dynamic model that links recessions, human capital and crime.[7] Individuals are lifetime utility maximisers where the source of utility from consumption and income comes from both the legal and the criminal sector. Individuals have endowments of legal and criminal human capital, which depreciate over time. Both types of human capital rise with experience in the sector and are increased by investment in the respective sectors. The individual's income is a function of human capital and rates of return in both sectors. In each period, the individual solves a dynamic stochastic optimisation problem: First, they decide how much time to allocate to legal and criminal work and second, they decide on the optimal level of consumption.

Crime is risky in the sense that a criminal faces a certain probability of being caught and sent to prison. The probability of prison depends on the skill of the criminal as measured by criminal human capital and the amount of time spent in the criminal sector as measured by experience in the sector. While legal human capital may decline in prison in addition to depreciation effects, for example due to reputation effects, criminal human capital may increase if criminals in prison learn from each other.

In this model, recessions impact on crime through the respective dynamic evolutions of both legal and criminal human capital. In that sense, the long-term impact of recessions on crime differs with the length and the depth of a recession. In a recession, the returns to legal human capital fall. Following the arguments from the standard Becker (1968) model, involvement in criminal activity rises depending on the relative and absolute returns to crime. If involvement in criminal activity increases, the criminal human capital stock is expected to grow while the legal human capital stock depreciates. Once the recession ends, returns to legal human capital increase again, and the relative returns to criminal activity decrease.

In a short recession, the stock of legal human capital typically remains significantly higher than the stock of criminal human capital, and the individual exits the criminal sector. Basically, in such a short recession, the individual is encouraged to

[6]See for example the reviews of Freeman (1999) and Chalfin and McCrary (2015), together with the introduction of Cook *et al.* (2013).

[7]For alternative dynamic models of crime participation see Flinn (1986), Lee and McCrary (2009) and Lochner (2004).

get involved in criminal activity, but is not exposed to these conditions for a long enough period to develop sufficient criminal capital in order to yield higher returns in the crime market than in the legal market once the recession ends.

If an individual is exposed to an unexpectedly long recession, the decision between legal and illegal activity changes in the same way as in a short recession. However, the individual's criminal human capital stock grows over a longer time period whereas the legal human capital stock is expected to decline even more than in a shorter recession. These two effects may result in higher returns to criminal activity than to legal activity even after the recession ends. We expect more permanent effects of a recession on criminal behaviour in that case. In addition, with higher involvement in criminal activity, the chances of being caught and imprisoned will rise. As explained above, if imprisoned, an individual's criminal human capital stock may rise further in absolute terms, and certainly rises further relative to legal human capital. In that situation hysteresis can occur and trigger criminal careers.

The mechanisms explained above are likely to be stronger for these individuals with initially low levels of legal human capital. New entrants to the labour market have developed less legal human capital and thus are less attached to the legal labour market. In our empirical analysis, we thus look at cohorts entering the labour market in different economic conditions and estimate the effect of entering the labour market in a recession on subsequent crime outcomes.

In the criminology literature there has been extensive focus on the concept of a criminal career and how it develops with age (see Piquero *et al.* (2003)). A criminal career is often characterised by various stages: onset, persistence, escalation or specialisation and desistance.[8] Sampson and Laub (1995, 2005) characterise crime as a product of persistent individual differences and local life events. They find that incarceration in later life is strongly related to the difficulty in securing stable work as individuals entered young adulthood.

Our research question of whether labour market entry conditions matter for crime fits naturally into this framework. Unemployment at labour market entry, a local life event, can contribute to the onset of criminal behaviour and/or can encourage the persistence of those youths that have already begun a criminal career. The long-run effect of unemployment at labour market entry then depends on the persistence and desistance effects. There has been less research on the duration of criminal careers. One study (Piquero *et al.*, 2003) finds that, for offenders with two or more offences, the average duration of criminal careers was 10.4 years.

[8] Criminological research that place a focus on particular stages of these crime dynamics includes Eggleston and Laub (2002), Elliott (1994) and McGee and Farrington (2010).

In the discussion thus far we have implicitly assumed that unemployment at labour market entry causes the criminal career to begin at that point, or to intensify for those youths already active in crime. A complementary alternative would be that entry unemployment has delayed effects on criminal behaviour. Zara and Farrington (2010) study a group of late-onset offenders who commit their first crime aged 21 or over. They find a significant effect of high unemployment at age 16-18 as a predictor of subsequent offending relative to a non-offending control group. To address this in our empirical analysis, we consider an approach that is flexible enough to permit differential timing of the effects of labour market entry unemployment effects on crime.

3.3 Empirical Strategy and Data

This section describes the empirical strategy in order to identify the impact of entering the labour market during recessions on long-term criminal behaviour. First the modelling approach and second the different data sources are described in detail.

3.3.1 Modelling Approach

Our empirical analysis exploits both individual micro-level data and panel data on year-of-birth cohorts over space and time. The data are discussed in more detail below. For the micro-data, we observe cross-sections of individuals and can identify those who are incarcerated in the U.S. and those who report having ever been arrested in the UK. Each individual data entry can be matched to the unemployment rate at the time of the individual's labour market entry in the area they live. That allows us to estimate probability models to explore whether this has an effect on criminal outcomes in later life.

For the panel data, we observe age/birth cohorts as they enter the labour market and follow them through their (potentially) working lives up to age 39. Our unit of analysis is defined at the year-of-birth cohort (c), region (r), and calendar year (t) level, where region refers to states in the U.S. and to standard regions in the UK. We can estimate the long-run effect of initial labour market conditions by exploiting the regional variation in entry unemployment rates across cohorts using the following equation:

$$\ln(\mathrm{CR})_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \beta \mathrm{UR}_{cr,0} + \gamma X_{crt} + \epsilon_{crt} \tag{3.1}$$

In equation (3.1) the dependent variable $\ln(\mathrm{CR})$ is the logarithmic crime rate for the cohort, region and time cells. We include fixed effects for the cohort, region, time and age that are respectively denoted by α_c, α_r, α_t and α_a. X is a set of control variables (defined below) and ϵ is an error term. Labour market entry occurs at date 0, hence $\mathrm{UR}_{cr,0}$ denotes the cohort-region specific unemployment rate at that date.

The first pertinent feature of equation (3.1) is that, in common with a number of other applications when cohorts of different ages are followed over time, it is well known that one cannot separately identify age, cohort and time effects. We follow the standard approach of including a full-set of age, cohort and time fixed effects and arbitrarily dropping one additional cohort effect. Alternatively, we could have required the cohort-effects to sum to zero (Deaton, 1997). Our results are robust to this alternative. Secondly, in order to adjust for cohort compositional differences, we include the X set of covariates at the level of our unit of analysis. In particular, we adjust for the average share of immigrants, male graduates, black males, married males and females per cohort in the region over the sample period.[9]

The model in (3.1) is restrictive in that it assumes that subsequent unemployment rates experienced by the cohort have no effect on their criminal behaviour. In effect, the model allows us to estimate the average effect of entering the labour market in a recession on crime, given the usual pattern of regional unemployment that cohorts experience after entry. For the focus of this chapter, we are arguably more interested in the effect of entry unemployment net of subsequent labour market conditions. To isolate this effect, we can include regional unemployment rates experienced by the cohort in the years after labour market entry. We measures these as $\mathrm{UR}_{cr,i}$, where $i > 0$ is the number of years since entry. This gives us a second, more general, model to estimate:

$$\ln{(\mathrm{CR})}_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \beta\mathrm{UR}_{cr,0} + \delta_i\mathrm{UR}_{cr,i} + \gamma X_{crt} + \epsilon_{crt} \tag{3.2}$$

where i can theoretically take any value up to the latest year observed since labour market entry. For example, when $t = 0$ corresponds to age 16, i could run from 1 to 23 years subsequent to entry up to our maximum age of 39. A fully saturated unemployment rate model would allow each unemployment rate that the cohort experienced in every year of their labour market experience to affect their crime rate. However, we restrict the coefficients on the i-dated unemployment rates

[9] The specific control variables included are to account for demographic correlates of crime and changing patterns of immigration. For examples of research papers directly studying the connections between crime and immigration see Bell *et al.* (2013), Bianchi *et al.* (2012) or Mastrobuoni and Pinotti (2015).

to affect the cohort crime rate only when the cohort reaches that point in the life-cycle. For example, the coefficient on regional unemployment five years after the cohort enters the labour market is restricted to be zero until the cohort actually reaches five years of experience. This ensures that future unemployment rates cannot affect current crime, which is intuitively sensible.

Next, we introduce dynamics by further generalising equations (3.1) and (3.2) to permit the main coefficient of interest β on the initial unemployment rate to vary with labour market experience/years since assumed labour market entry.[10] This enables us to see to what extent the average effect of entry unemployment on a cohort occurs because of early scarring effects that erode as time since labour market entry increases or because of more persistent effects across a cohort's life-cycle:

$$\ln\left(\mathrm{CR}\right)_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \sum_{e=1}^{E} \beta_e [\mathrm{I(Exp{=}e)} \cdot \mathrm{UR}_{cr,0}] + \gamma X_{crt} + \epsilon_{crt} \tag{3.3}$$

This specification allows β to vary with potential labour market experience Exp, for experience groups e = 1,...,E, and measures the extent to which any effect of initial unemployment on criminal behaviour persists as length of time since labour market entry increases.

Our final, and most general estimating equation, additionally allows for the unemployment experienced after labour market entry to have permanent or transitory effects:

$$\begin{aligned}\ln\left(\mathrm{CR}\right)_{crt} = \alpha_c + \alpha_r + \alpha_t + \alpha_a + \sum_{e=1}^{E} \beta_e [\mathrm{I(Exp{=}e)} \cdot \mathrm{UR}_{cr,0}] \\ + \sum_{e=i}^{E} \delta_{ie} [\mathrm{I(Exp{=}e)} \cdot \mathrm{UR}_{cr,i}] + \gamma X_{crt} + \epsilon_{crt}\end{aligned} \tag{3.4}$$

In equation (3.4) both the β's and the δ's are allowed to depend on the length of time that passes since the initial and the subsequent unemployment rate were experienced by the cohort. Again, the effects of subsequent unemployment are restricted to be zero until the cohort reaches the relevant age.

Note that for all the models (3.1) to (3.4), identification comes from within-cohort

[10] Potential experience here is defined as years since labour market entry, i.e. age - [age at year t=0] with t=0 being the assumed labour market entry age as defined below. Hence, the notation of the age/experience fixed effect in the estimating equations can be interchangeably used as either α_a or α_e.

variations in the labour market entry unemployment rates across states/ regions. We view this as the most convincing approach that can be taken to produce evidence with the available data and this therefore forms the basis of most of our results. However, it could be argued that removing the aggregate national unemployment rate at labour market entry, as implicitly done by including cohort fixed effects, removes much of the variation over time. To address this, we also report specifications using the national unemployment rate at labour market entry and including a quadratic cohort trend in order to account for changing cohort quality.

3.3.2 Details of U.S. Data

The data for the empirical analysis come from U.S. and UK data sources, respectively. As mentioned above, both individual level data as well as cohort level data are used. In the following, the U.S. data are described in more detail.

U.S. Panel Data

For the U.S. panel analysis, criminality is measured in arrests. The use of arrests data is motivated by two considerations: First, consistent annual incarceration data at the state and cohort level simply do not exist in the United States, see for example Pfaff (2011). Second, it is of interest to measure criminality broadly and check that the results are robust. Therefore, we use arrest data from the FBI Uniform Crime Reports (UCR). The UCR reports the number of arrests by year, state, age, gender and type of crime. The original data identifies the number of arrests by law enforcement agencies within states. We construct a state-level panel on arrests by aggregating the number of arrests over law enforcement agencies within a state. The resulting sample runs for all years from 1980 to 2010.

We obtain the number of arrests for property and violent crimes by respectively aggregating arrests over crime types. Our measure for property crime includes arrests for burglary, larceny, vehicle theft and arson, while our measure for violent crime includes arrests for murder, rape, robbery and assault. We produce arrest rates by dividing the number of arrests by the annual population in the observational unit, and scale by 1,000 for ease of interpretation. Population data is retrieved from the U.S. Census population estimates.

We sample males aged 16 to 39 from 1980 to 2010. The UCR data are grouped by age category. From age 16 up to the age of 24, the number of arrests is measured by single age year. For ages 25 and above, the arrests are aggregated to the number of arrests in a five-year age bracket, i.e. 25 to 29, 30 to 34, and 35 to 39. In order to be able to track the number of arrests per year-of-birth cohort, we therefore disaggregate the arrest measure to the number of arrests by single age year by dividing the arrest count by five. The underlying assumption is that year-of-birth cohorts are homogenous in terms of the number of arrests within the respective age bracket. In order to be able to track the number of arrests per year-of-birth cohort, we therefore disaggregate the arrest measure to the number of arrests by single age year by dividing the arrest count by five for the older age groups. The underlying assumption is that year-of-birth cohorts are homogenous in terms of the number of arrests within the respective age bracket.

Since participation in the UCR programme is voluntary for the law enforcement agencies, data are partly missing either for the whole state or for a number of law enforcement agencies within a state. Data for some states are systematically missing, and hence we exclude these states from our analysis.[11] Data for some states are missing for a limited number of years only. For example, Florida reports arrests until 1995, but not afterwards. Since there is no evidence that would suggest that these states differ significantly in terms of unemployment rates, we exclude the respective years only and keep the non-missing years of these states as observations in the sample, leading to an unbalanced sample. In the example above that means that we include Florida in our sample until 1995.

Moreover, the UCR reports the total population for each law enforcement agency in the reported year. Aggregating the UCR population count to the state-year level and comparing that number to official population counts allows us to identify state-year observations that cover arrests for less than 95 percent of the state population. Since these arrest counts are likely to underreport the true number of arrests in that state and year, we exclude the respective observations from our sample. Whenever single state-year observations are missing in the resulting sample, we impute values using a linear interpolation method. Our results are robust to excluding imputed observations.

U.S. Individual-Level Data

The individual level data on U.S. incarceration comes from the U.S. decennial Census and American Community Survey (ACS) data. We study all males aged 18-39 from the 5 percent samples of the 1980, 1990 and 2000 Census and the 2008-2012

[11] We exclude the following states: Indiana, Louisiana, Mississippi, Montana, Nebraska, New Hampshire, New Mexico, New York, Ohio, South Dakota, and Washington. For example, New York is excluded since New York City (specifically the NYPD) systematically does not report arrests to the FBI, and thus arrest data at state level would be heavily undercounted.

ACS from IPUMS-USA.[12] We identify the institutionalised population using the Group Quarters (GQ) variable, and obtain additional covariates from the Census data including race, marital status, veteran status and education.

The Group Quarters variable consistently identifies the following categories: Non-group quarter households, institutions (correctional institutions, mental institutions, institutions for the elderly, handicapped and poor), or non-institutional group quarters (military, college dormitory, rooming house, other). Yet, the Group Quarters variable is only available at a detailed enough level to uniquely identify those in correctional facilities in the 1980 sample. In subsequent Censuses as well as the ACS, the institutionalised population includes the following categories: correctional facilities, nursing homes and mental hospitals, and juvenile institutions. Fortunately for our purposes, the share of the total institutionalised population accounted for by those in correctional facilities is very high in our sample.

Table 3.1 shows the institutionalised male population by Group Quarter type and age.[13] In 2000, for example, 95.3 percent of institutionalised males aged 18-39 where in correctional facilities. Two key points come from table 3.1. First, incarcerated males aged less than 18 years are much less well identified. This is due to the fact that juvenile facilities are an important component for this group. We therefore restrict our analysis of the Census data to those individuals who are aged between 18 and 39. Second, the 1980 Census has a less tight correspondence between institutionalisation and incarceration. Yet, this is the Census year that provides the full Group Quarter coding within the micro files. Hence, as a robustness test we are able to use only the correctional facility definition in the 1980 Census. In the main specification, we prefer to use the broader institutionalised measure across all years in order to maintain consistency. This approach follows the approach suggested by Borjas *et al.* (2010).

U.S. Unemployment Data

For both the UCR and the Census data, our samples comprise year-of-birth cohorts that run from 1941 to 1994.[14] Assuming that individuals enter the labour market at age 16 to 18, labour market entry for each cohort in the sample would therefore occur between 1957 and 2010. We use data on state-level annual insured

[12] Integrated Public Use Microdata Series.

[13] Note that this data comes from published aggregate Census reports that do break up the categories, but is not available in the IPUMS data release.

[14] Our first year of data on arrests/incarceration is 1980 and the oldest age we consider is 39, so this cohort was born in 1941. Similarly our data ends in 2010 and the youngest age is 16, so this cohort was born in 1994.

unemployment rates from 1957 until 2010 which are available from the Unemployment Insurance Financial Data Handbook provided by the U.S. Department of Labour, Employment & Training Administration on their website, and match them both to the UCR and the Census data.[15]

Two issues arise with the U.S. data. First, since we link the current arrest rate for a particular cohort in a given state to the initial entry unemployment rate of that cohort in the same state, we assume that cohorts do not substantially move across states over time. That means for example that we assume that the criminal behaviour of the 30 year-old cohort in California in the year 2000 is affected by the unemployment rate in California in the year 1986, when that cohort entered the labour market. The empirical validity of this is subject to no inter-state mobility since school-leaving age. If there is mobility, but it is random since school exit, the estimates will merely be noisy. However if mobility is driven by self-selection, the coefficient of interest may be biased. Following Dahl (2002) we present robustness tests based on mobility data from the U.S. Census.

Second, in our empirical work for the U.S. we use the average unemployment rate that the cohort experienced at ages 16 to 18 as our measure of entry unemployment. This is motivated by the observation that the majority of arrested criminals have low educational attainment and generally do leave school at or around the compulsory school leaving age. In the U.S. Census data used in the micro-data analysis, 86 percent of those incarcerated over the 1980-2010 sample had high school or less, i.e. less than 12 years of education, as their highest level of education. Further, since school-leaving ages differ slightly across time and states and unemployment within a cohort-state observation is autocorrelated, we use the average unemployment rate over ages 16 to 18 in order to characterise the state of the labour market that the cohort first experiences. An alternative would be to use the unemployment rate at age 16, or indeed age 17 or 18, only. We show that our results are robust to these alternative definitions of labour market entry unemployment.

3.3.3 Details of UK Data

Our analysis is based not only on U.S. data, but also on UK data. Similar to the case of the U.S., we exploit both cohort-level data as well as individual level data,

[15] Unfortunately, that kind of data does not allow us to disaggregate entry unemployment rates by age, i.e. consider youth unemployment, nor to provide measures of the duration of unemployment. As an alternative, the Bureau of Labor Statistics provides state level unemployment rates based on the Current Population Survey from 1977 onwards, to which we data read from the graphs in Blanchflower and Oswald (1994) back to 1963. This robustness check yields very similar results to the use of insured unemployment rates.

which are described in the following.

UK Panel Data

Crime data for the UK panel data come from the Offenders Index Database (OID) and the Police National Computer (PNC). Here, criminality is measured in the number of convictions. This has the advantage of capturing actual offenders, of course subject to wrongful conviction, rather than the proportion of a particular cohort that come into contact with the police as in the case of arrest data in the U.S.

The OID is a 4-week sample of all convictions in all courts across England and Wales, with the sample weeks evenly spread across the year. The data contains a unique personal identifier which allows us to remove multiple convictions for the same individual, i.e. in the sample in a given year an individual is either convicted or not, and provides data on gender, date of birth, region of conviction (10 regions) and offence category. The OID sample runs from 1980 to 2002. From 2003 to 2010, the OID has been superseded by the PNC. While we do not have access to the individual level data of the PNC, the Ministry of Justice have provided us with an extract of the number of individuals convicted in each year, broken down by individual year of age, gender, region of conviction and offence category. This allows us to merge the two datasets and to produce a panel covering the years 1980 to 2010.[16]

We obtain the number of convictions for property and violent crimes by aggregating convictions over crime types. As such, our measure for property crime includes burglary, theft and handling of stolen goods and criminal damage, while our measure for violent crime includes violence against the person, sexual offences and robbery. We produce conviction rates by dividing the number of convictions by the annual population in the observational unit (year-of-birth by region), and scale these conviction rates by 1,000 for ease of interpretation. Population data are obtained from the ONS population estimates. As with the U.S. data, the sample covers convictions from 1980 to 2010 for 16-39 year-old males. Therefore, individual year-of-birth cohorts again run from 1941 until 1994.

UK Individual-Level Data

Our individual-level data for the UK comes from the British Crime Survey (BCS). The BCS is a large, annual cross-sectional survey of 45,000 individuals which is used

[16]The PNC data is actually provided for the time period 2000 to 2010 which allows us to examine the overlap between the OID and PNC between 2000 and 2002. Our analysis of this overlap suggests a very high concordance between the two sources.

to construct measures of crime victimisation. It is nationally representative and contains extensive personal demographics.

From 2001 onward, each year a sub-sample of respondents complete a supplementary survey that, among other things, covers contact with law enforcement agencies. In particular, respondents are asked whether they have ever been arrested by the police. Yet, there is no information on the type of crime for which they were arrested nor on the eventual outcome. In addition there is no information on when the arrest occurred, i.e. a 65-year old may have been arrested last week or 50 years ago. However, as we will use conviction data in the UK panel analysis, it is useful to have an alternative measure of criminal behaviour to evaluate robustness, as for example in Lochner and Moretti (2004). In addition, the survey data provides a broad array of personal characteristics including educational attainment, ethnicity, marital status, housing status and employment and income measures. We sample all males aged between 16 and 65.

UK Unemployment Data

As with the U.S. data, we assume that individuals enter the labour market at the school leaving age. Hence, we consider unemployment rates at labour market entry from 1957 until 2010. The unemployment rate data from 1975 onwards comes from the Labour Force Survey. Prior to 1975 the unemployment rate is derived from the claimant count data. This latter measure covers only those registered as unemployed and is therefore a more narrow definition than that in the Labour Force Survey which covers all those actively seeking employment in the previous two weeks. However, the unemployment rate here is measured for males only and the discrepancy between the two alternative measures prior to the 1980s is small. We thus assume that our measure is a valid measure of unemployment.

In contrast to the U.S., there is a standard national school-leaving age in the UK. We use this compulsory school leaving age to date labour market entry for each cohort, rather than taking the average unemployment rates of ages 16 to 18. However, we show that our conclusions are robust to this alternative measure of labour market entry age. In the main specification, the age of labour market entry is hence assumed to be 15 for those leaving school by 1972 and 16 for those leaving from 1973 onward in order to reflect the change in compulsory school leaving age introduced in the UK in 1973.

3.4 Cohort Panel Evidence

In the following, the results from the empirical analysis of the cohort panel data, as described in section 3.3, are presented. The discussion of the U.S. results is followed by the discussion of the UK results.

3.4.1 United States

We begin our analysis of the U.S. panel data by presenting evidence on the average effect of initial labour market conditions on criminal activity. In terms of the equations above, this specification refers to equation (3.1), which restricts the coefficient β to be the same across all experience groups. The results are shown in table 3.2. Here, the dependent variable is the logarithm of the crime rate, where the crime rate is defined based on arrest rates as explained above. Columns (1), (3) and (5) consider the national unemployment rate at labour market entry while columns (2), (4) and (6) use the state unemployment rate at labour market entry - our preferred specification. All regressions include year, state and age fixed effects as well as a set of variable to control for cohort composition. The national unemployment rate specifications control for a quadratic cohort trend, while the state unemployment rate specifications include a full set of cohort fixed effects. The regressions are weighted by the population of the observational unit and robust standard errors are clustered at the state-cohort level.

Columns (1) and (2) of table 3.2 show a strong positive estimated coefficient on the entry unemployment rate, whether we use the national or state-level variation in entry unemployment. For the state-level entry unemployment rate specification, shown in column (2), the average arrest rate for a cohort entering the labour market in a recession is estimated to be around 10.2 percent higher than for a similar cohort entering into a normal labour market. This is based on a 5 percentage point increase in unemployment as a measure of recession relative to normal conditions. The estimate is statistically significant at the 1 percent level.

This amounts to a substantial estimate of labour market entry effects on crime, but in some respects the average effect of recessions may not be the most relevant parameter of interest. Indeed, within a cohort, there will be a substantial share for which the marginal effect is zero, since their optimal decision will be unaffected, i.e. they are at an interior solution that results in no illegal behaviour and the recession does not move them across the threshold. Thus the estimated average effect is a combination of a zero effect for a potentially large share of the cohort and a substantial effect for those who are close to the threshold between legal and illegal action in the absence of a recession. Indeed, the results from the analysis of the individual-level Census data presented in section 3.5 suggest that this is the case, as the estimated entry-level unemployment effects are seen to be much larger for the less educated.

The remaining columns of table 3.2 show results for property crime and violent crime, using both national and state unemployment variation. The results suggest very similar and statistically significant effects in all cases. In all subsequent results only those specifications that use state-level unemployment rates are reported, as we view this as providing the most convincing identification.[17]

The specification used in table 3.2 implies that subsequent unemployment rates do not matter, or at least are orthogonal to the entry unemployment rate. There is no obvious reason for this to be the case and hence we follow the earnings study of Oreopoulos *et al.* (2012) in allowing for subsequent unemployment rates to affect our outcome of interest, crime, in addition to the entry unemployment rate. Hence, the average unemployment rates for ages 19-21, 22-24 and 25-27 are included. In essence this means that for a particular cohort we allow for their crime path to be explained by both the unemployment experienced when entering the labour market and the unemployment rates they experience over the following 10 years.[18]

Table 3.4 presents the results of this exercise which corresponds to equation 3.2. It is perhaps most useful to focus on column (3) where we allow for two changes, breaking the age 16-18 unemployment rate into its component parts and allowing for subsequent unemployment rates. On the first of these, when we allow for separate estimated effects for any individual year of unemployment, the estimates are imprecise. However the p-value from a hypothesis test of the joint significance of the three individual year effects is significant at the 1 percent level. The reason is that there is a high degree of autocorrelation in the within-cohort unemployment rate. The strong persistence in the autocovariances of unemployment rates within a cohort-state group is shown in figures 3.3 and 3.4, respectively. Therefore, we prefer to either use the age 16 effect alone, recognizing that it is picking up effects for age 17 and 18 as well, or to use the three-year average. As columns (1) and (2) show, it matters little which we choose.

The second key result of column (3) is that none of the subsequent three-year

[17]We have also broken down property and violent crime into more disaggregated measures of crime types, i.e. breaking down violent crime into murder, rape, assault and robbery and property crime into burglary, theft and arson. We find there to be significant positive estimates of entry level unemployment rates for all crimes with the exception of murder. See table 3.3 for details.

[18]We have also experimented with including unemployment rates prior to school-leaving age. Their additional inclusion leaves the estimated impact of entry unemployment intact, remaining positive and statistically significant.

average unemployment rates that the cohort experiences have an individually significant effect on arrests, although they are all negative. This helps us to better understand a puzzle in the literature which we referred to in the introduction: The overall link between crime and unemployment appears fairly weak in many studies. Our results show that the key effect from unemployment on a cohort's crime trajectory is the early experience of unemployment rather than the average unemployment experienced over the life-cycle.

The results in tables 3.2 and 3.4 demonstrate a statistically significant and economically substantial effect of initial unemployment conditions on the arrest rates of cohorts over their entire lifetime. Moreover, we are interested in examining the persistence of this effect: Is the entry unemployment effect primarily driven by a very large impact on crime in the early years after labour market entry that subsides as the young age and go on to establish a stable legal career? Or is the effect persistent, with some of those affected by harsh labour market conditions at labour market entry pushed into a criminal career that becomes self-perpetuating for the reasons discussed in section 3.2? In order to answer that question, we allow the coefficient on initial unemployment to vary by years since labour market entry as described by equation 3.3.

We group experience into four categories (0-5, 6-11, 12-17 and 18-21 years) and otherwise use an identical regression specification as for the previous table. Experience is normalised to 0 for ages 16 to 18. The results are shown in table 3.5 with columns (1) and (2) showing results for all crimes, and columns (3) and (4) for property and violent crime respectively. Column (1) is estimated without controlling for subsequent unemployment rates, whereas columns (2) to (4) allow for these to be interacted with experience dummies as specified in equation 3.4. There are strong positive effects of entry unemployment on arrests in the early years in the labour market that fall as experience increases. However, even a decade after leaving school there remain significant positive effects from entry unemployment on arrests, particularly for property crime: Juveniles who leave school in a recession have higher arrest rates during their first few years in the labour market and higher arrests rates over a decade later than juveniles who leave school in a buoyant economy.

An alternative specification to examine the persistence of entry unemployment is to allow for the interaction term with experience to vary by individual years of experience rather than to group experience into year brackets as done for the estimations shown in table 3.5. Figure 3.5 plots the estimated coefficients together with 95 percent confidence intervals for every year of labour market experience, yet without allowing for subsequent unemployment rates. Figure 3.6 shows the respective results when one allows unemployment rates later in life to enter the regression. There is a clear drop in the effect after the first few years of labour market entry, yet the individual year estimates suggest a consistent and longer lasting scarring effect.

As previously discussed, one may have potential concerns about inter-state mobility. More precisely, the presence of mobility raises the question as to what is the correct, or in other words best measured entry unemployment rate for cohort c at time t in state s? Thus far we have assumed it was the unemployment rate in state s at the time that cohort c left high-school. Yet, this ignores mobility and if potential criminals are likely to move across state boundaries, this could be of concern. Some of those in cohort c at time t in state s will have completed high-school in state k and entered the labour market there. For this part of the cohort, the correct entry unemployment rate is of course the unemployment rate in state k at the time cohort c left high-school. Dahl (2002) makes the same point with respect to estimates of state-specific earnings returns to education, which he shows differ substantially across states. His solution to this mobility problem is to use reported migration flows across states to correct the estimated returns. We follow broadly the same procedure here: We use the 5 percent US Census for 1980, 1990 and 2000 and the 2010 ACS to calculate for each cohort c in state s the distribution of states-of-birth, and use the unemployment rates at age 16 only. Assuming that state-of-birth and state-at-16 are highly correlated, we generate a mobility-adjusted entry unemployment rate for cohort c in state s as:

$$\mathrm{UR}_{cs} = \sum_{k=1}^{K} p_{csk}\mathrm{UR}_{ck} \tag{3.5}$$

where p is the proportion of cohort c in state s that were born in state k.

Table 3.6 reports estimates using this mobility-adjusted entry unemployment rate. The results are robust to the new specification, in that a positive and substantial entry-level unemployment rate effect on crime remains. The result for all crimes as reported in the upper panel of the table is similar, and a little bigger in magnitude at 2.470 compared to 2.039 from table 3.2, but very much corroborates the earlier results. In fact, the estimated coefficients on property and violent crime increase a little, too. Moreover, if we apply the mobility adjustment to the age 16, rather than age 16 to 18 entry unemployment rate, the results as shown in the lower panel of table 3.6 remain robust. Hence, this robustness check offers a useful corroboration of our main results as, if anything, we appear to marginally underestimate the effect of initial unemployment at labour market entry on crime when we do not adjust for inter-regional mobility. Note, however, that the differences between mobility adjusted and non-adjusted estimates are not distinguishable from one another in terms of statistical significance.

3.4.2 United Kingdom

In this section, the corresponding results for the empirical analysis of the UK data are presented. We begin with the same specification as reported in table 3.2 for the U.S., the results for the UK being shown in table 3.7. Recall that these estimates reflect the average effect of initial labour market conditions on criminal activity, in this case convictions rather than arrests. As with table 3.2, we report estimates for total crime and for property and violent crime separately and for specifications using either the national or region-specific entry unemployment rate. The only specification difference compared to the U.S. analysis is that we allow cohort composition effects to have different coefficients in London compared to the rest of the UK. The differences in these estimated coefficients are statistically significant, suggesting that over time cohort composition and their effects on crime have differed substantially between London and the rest of the UK.[19]

As with the U.S. results, we find a statistically significant effect of entry unemployment on overall lifetime crime. Taking the estimated coefficient in Column (2), a recession that raises the unemployment rate by 5 percentage points would raise the lifetime conviction rate by 4 percent. We are somewhat skeptical about the magnitude of the effect when using national entry unemployment as the source of identification. The difficulty arises because we have to assume a specific functional form for the cohort effect, whereas, when regional entry unemployment is used, we can non-parametrically control for the cohort effect, since identification here comes from within-cohort variation across regions. To see the sensitivity of the results to this, note that the coefficient on national entry unemployment in column (1) of table 3.7 is 2.664 (0.189) when we allow a quadratic cohort trend. If instead we allow a quartic cohort trend this coefficient drops to 1.007 (0.189). Hence, we prefer to focus on the results that exploit within-cohort variation.

Next, we consider unemployment rates other than that at age 16. The results are shown in table 3.8. Columns (1) and (2) show that it matters little whether we use the age 16 unemployment rate or the age 16 to 18 average unemployment rate to capture entry effects. Columns (3) and (4) illustrate two key results. First, subsequent unemployment experiences seem to have little effect on overall crime

[19] An alternative would be to estimate the models using the regional dimension outside of London only. Such an analysis shows that this generates the same qualitative results as reported here, although the precision tends to be somewhat higher. We prefer to include London and to directly control for differences in the effect of cohort composition.

rates. This suggests that if youths get through their initial experience of the labour market without turning to crime, they are largely unaffected by subsequent unemployment experiences in terms of criminal behaviour. This is consistent with the theoretical model under which the rising level of legal versus criminal human capital increasingly reduces the chances of resorting to crime, and suggests important policy implications. Second, controlling for subsequent unemployment has no effect on the size of the entry unemployment effect.

Again, we are interested in the persistence of the entry unemployment effect. As for table 3.5 for the U.S., we split the data into four experience groups (0-5, 6-11, 12-17 and 18-23 years) and allow the entry effect to differ across these experience groups. Column (1) of table 3.9 reports the estimates without controlling for subsequent unemployment whilst columns (2) to (4) control for subsequent unemployment rates interacted with experience as specified in equation 3.4. The estimates show there to be a strongly persistent effect of entry unemployment on subsequent criminal convictions. Once again, the key message is that high entry unemployment contributes to significantly higher crime rates among affected cohorts that are long-lasting. Over a decade after entry, conviction rates remain significantly higher. For property crime, the influence eventually dies out after 15 to 20 years post-labour market entry experience while it remains, and indeed becomes quantitatively more significant, for violent crime.

Figures 3.7 and 3.8 show the year-by-year effect of entry unemployment as a cohort spends more time in the labour market. One key difference between the time-profile of the experience effects for the UK and the U.S. is that the entry unemployment effect for 16 and 17 year old cohorts is substantially higher than the average entry unemployment effect in the U.S., but not in the UK. An obvious explanation for this rests with our measure of criminality in the two countries: For the U.S. analysis we use arrests as a measure of criminality while we use convictions for the UK analysis. It seems likely that the detrimental effects of entry unemployment will take substantially more time to feed through to convictions than to arrests: Youths may be frequently arrested, but avoid the courts until a tipping point has been reached. In any event, the effects are very similar across the two countries from the age of 18 onwards.

Table 3.10 focuses on whether all recessions are alike. One feature of the labour market common to European countries over the last forty years, but almost completely absent for the US until the Great Recession has been the incidence of long-term unemployment.[20] We might expect, and the model of Mocan *et al.* (2005)

[20]The subsequent chapter of this dissertation focuses on the impact of a first-time occurrence of

predicts, that recessions which are characterised by rising rates of long-term unemployment are much worse for potential scaring. Initially, of course, rising unemployment durations regarding the stock of currently unemployed is positive for new labour market entrants, since the stock of unemployed provides less competition for available vacancies. However, we might expect this effect to be fleeting before the negative effects of unemployment duration on new entrants takes its toll. To examine this we divide the entry unemployment rate into the short-term and long-term unemployment rate where short-term unemployment covers all those with a current unemployment spell of less than twelve months. For our entire sample, the average unemployment rate of 7.4 percent is made up of a short-term rate of 4.6 percent and a long-term rate of 2.8 percent. The results of table 3.10 show clearly that it is deep and long recessions which are characterised by high long-term unemployment that are particularly problematic in terms of crime.[21]

3.5 Individual-Level Evidence

In the previous section, the results of the cohort panel data both for the U.S. and the UK have been discussed. The individual-level data analysis, as presented in the following, adds to the picture developed in the above.

3.5.1 United States

We begin the analysis with the analysis of the U.S. incarceration data. We use the state-at-birth to identify the state in which the individual went to school (Dahl, 2002) and hence restrict the data to those individuals born in the United States. Panel A in table 3.11 reports the key regression results based on the Census individual-level data and using a linear probability model. Column (1) reports the results for the full sample of males aged between 18 and 39 whilst the subsequent three columns focus on samples defined by educational attainment. All regressions include a full set of year, state of residence, state of birth and cohort effects, a quartic function in age as well as control variables for race, education, marital status and veteran status.

The estimated coefficient on the entry unemployment rate in column (1) is 0.031. The mean of the dependent variable is 0.028, i.e. 2.8 percent of males aged between 18 and 39 are incarcerated. Thus, entering the labour market in a time of recession, again defined as the unemployment rate being 5 percentage points higher than

long-term unemployment in the U.S. labour market on crime.

[21]Unfortunately, the U.S. data does not allow us to conduct a similar analysis.

normal, results in a 5.5 percent increase in the probability of being incarcerated at the time of subsequent census survey dates. As can be seen from the subsequent columns, this effect is almost entirely driven by a strong effect for high-school dropouts: A recession increases this group's probability of incarceration by 7 percent, from an already high mean of 8.4 percent. These are sizeable effects, keeping in mind that this is averaged over more than twenty years of the individual's post-school experience.

Finally, compared to the previous results, columns (3) and (4) display only weak effects for those who successfully graduate from high school and no effect at all for those with 4-years of college - who should of course not be affected by the unemployment rate at the compulsory school-leaving age. The results in Panel B show that redefining the 1980 measure of incarceration by explicitly excluding those not in correctional facilities (see section 3.3 for a detailed discussion) does not alter the conclusions from this analysis.

The results found from the individual-level data analysis have some additional and important policy implications: They suggest that policy which focuses explicitly on the least educated during periods of high unemployment would likely produce substantially more benefit on crime reduction within that group than the average estimate from the previously reported panel regressions would imply.

3.5.2 United Kingdom

For the UK, we study individual-level data on self-reported arrests. The data provide information on the age at which the respondent left full-time education and so allow us to precisely date the year of labour market entry. The data also provide an extensive set of personal characteristics, which we would expect to be correlated with criminal activity. There are two key disadvantages in using this micro data. First, there is the usual concern associated with the self-reporting of arrests. However, in the context of this study a potential measurement error from self-reporting would only bias our estimates if the self-reporting probability varied within a cohort *depending* on the initial entry unemployment rate. It seems to us hard to make such a case. Second, we have no information of when the arrest occurred, as the survey question is simply whether the individual has ever been arrested. Hence, the UK individual-level data allow us to estimate the average impact of initial entry unemployment on the probability of being arrested in adulthood, but does not allow us to investigate the time pattern of the persistence of such effects.

We estimate probit models with the dependent variable taking the value one if the respondent reports having ever been arrested by the police. We include survey year dummies and an extensive set of personal controls. Table 3.12 reports the results. Column (1) shows an estimated significant positive coefficient on the entry unemployment rate: A recession, again defined as a 5 percentage points higher than normal unemployment rate, is associated with a 5.7 percent increase in the probability of ever being arrested.

In the second column we restrict attention to those whose highest educational qualification was achieved at age 16 and who therefore definitely left education at age 16. Here, we can more closely link exit from education to the initial unemployment rate, and thus more likely obtain a sample that contains a larger fraction of individuals at risk of criminal behaviour. As expected, we find a substantially larger and more precisely estimated impact of entry unemployment for this group: Now, a recession raises the probability of ever being arrested by 8 percent.

The final column of the table displays the results of a placebo-type experiment. We examine the arrest record of individuals who report educational qualifications that required school attendance at least to age 18. This group should not have been directly affected by the unemployment rate when they were at age 16. Sure enough, we no longer find a positive effect for these individuals. Indeed, the estimated coefficient on the entry unemployment rate is indistinguishable from zero, although the standard error is large.

Overall, the individual-level analysis of the relationship between crime and entry-level unemployment produces results that are very similar to the cohort panel analysis of section 3.4. This is true for both countries, despite some differences in the nature of the data that is available. The individual-level data also permits us to study variations across individuals with different levels of education in more detail than the more macro cohort analysis which does not permit such differentiation. It is highly reassuring that the overall pattern of results are very consistent across the two approaches.

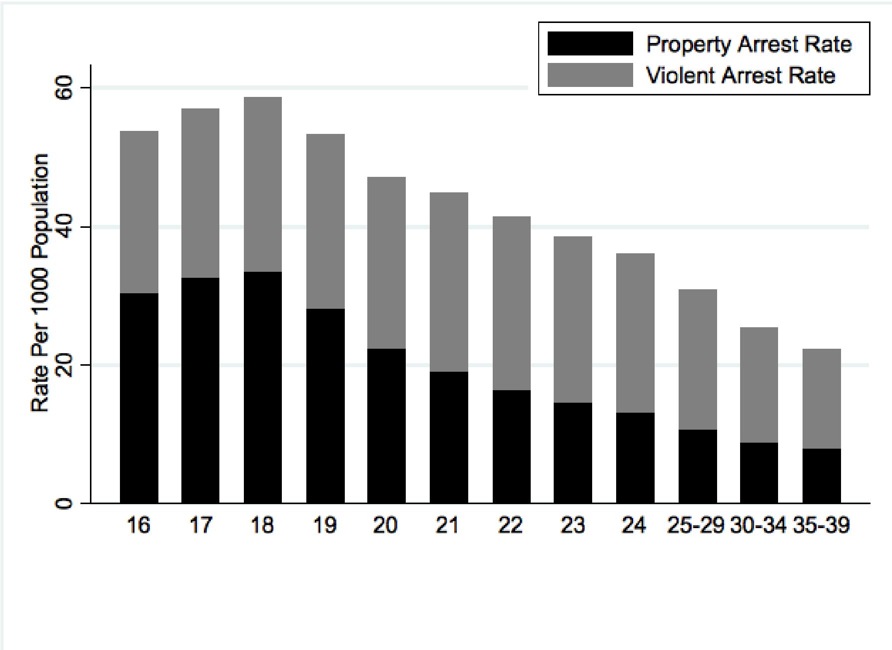
3.6 Conclusion

In this chapter, the first evidence is presented that recessions can lead to substantial and persistently higher rates of criminal behaviour among those likely to be most impacted by such conditions, namely those newly entering the labour market. In contrast to much of the evidence on the long-run effect of initial unemployment on wages and career trajectories, we find that the effect on criminal behaviour remains substantial, though attenuated, a number of years after labour market entry. These sizable and persistent entry level unemployment effects thus show that recessions can produce career criminals. One might argue that our results are also consistent with a one-time criminal event for individuals in a particular cohort that happens at different times since leaving school and that the probability of such a subsequent event is higher if entry level unemployment were higher. Such a view would however be in conflict with two key empirical findings in the criminology literature: Late-onset offending is extremely rare and prolific offenders account for a disproportionate share of total crime. Both are consistent with our interpretation of the results.

This evidence of a crime scarring effect from unemployment at the time of labour market entry emerges from empirical analysis of a range of different US and UK data sources, both at the level of the individual and from longitudinal analysis of age/birth cohorts over time. The evidence of crime scars demonstrates a rather more disturbing long-run effect of recessions, and adds to the research picture that the state of the business cycle when people leave school and enter the labour market can have profound and sizable impacts on economic and social outcomes across their life.

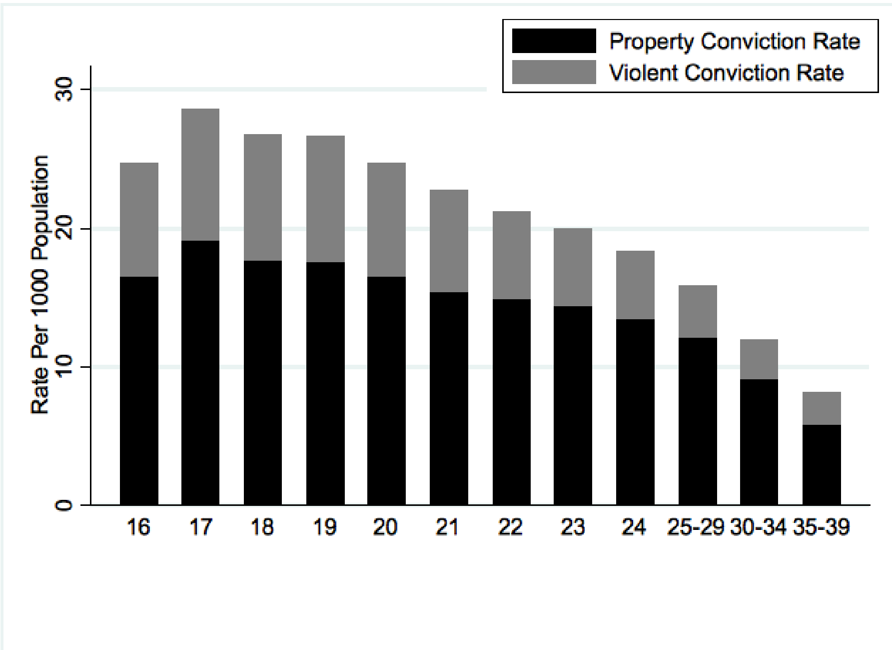
3.7 Figures

Figure 3.1: Male Offender Rates by Age, U.S.



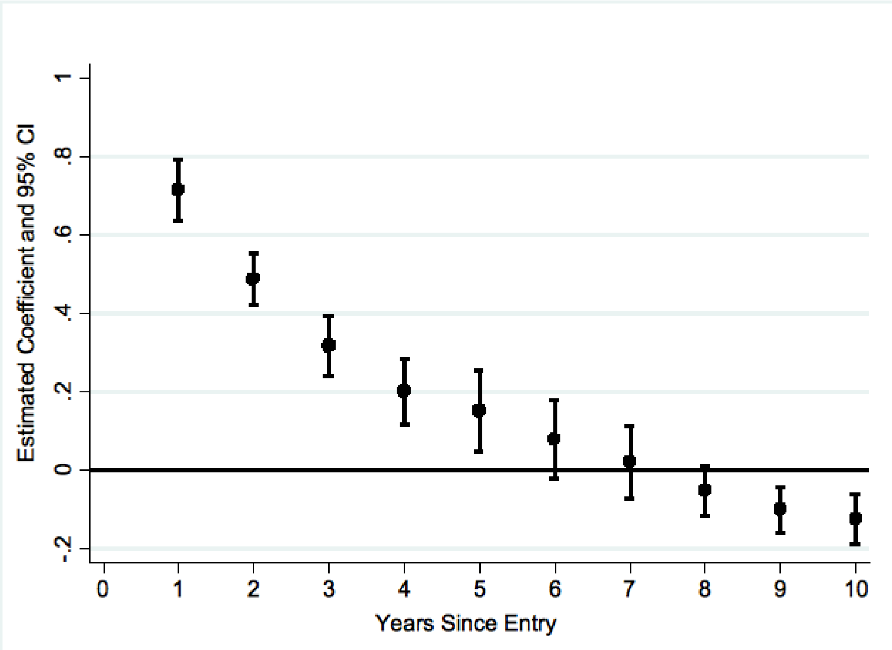
Note: The figure shows the male arrest rate for the U.S., separately by age and averaged over the period 2000-2010. *Source:* UCR and own calculations.

Figure 3.2: Male Offender Rates by Age, UK



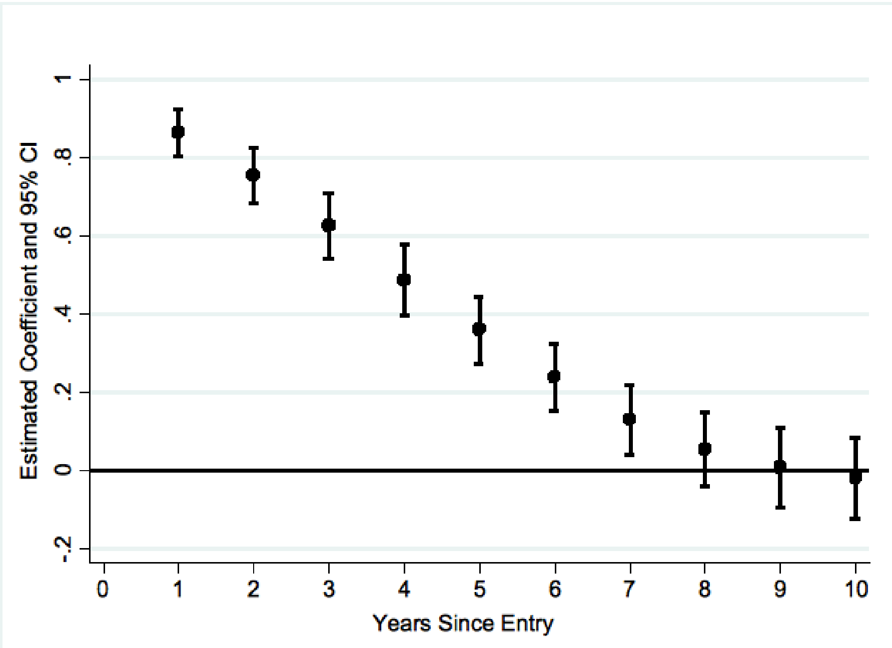
Note: The figure shows the male conviction rate for the UK, separately by age and averaged over the period 2000-2010. *Source:* OID/PNC and own calculations.

Figure 3.3: Autocovariance Structure of Unemployment Rates, U.S.



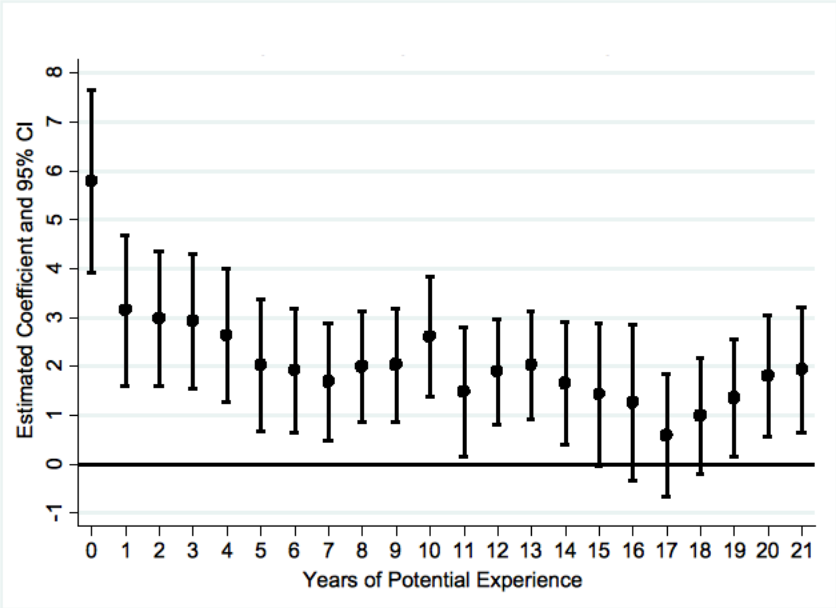
Note: The figure shows the auto covariance structure of the unemployment rates in the U.S. since labour market entry. *Source:* U.S. Department of Labor and own calculations.

Figure 3.4: Autocovariance Structure of Unemployment Rates, UK



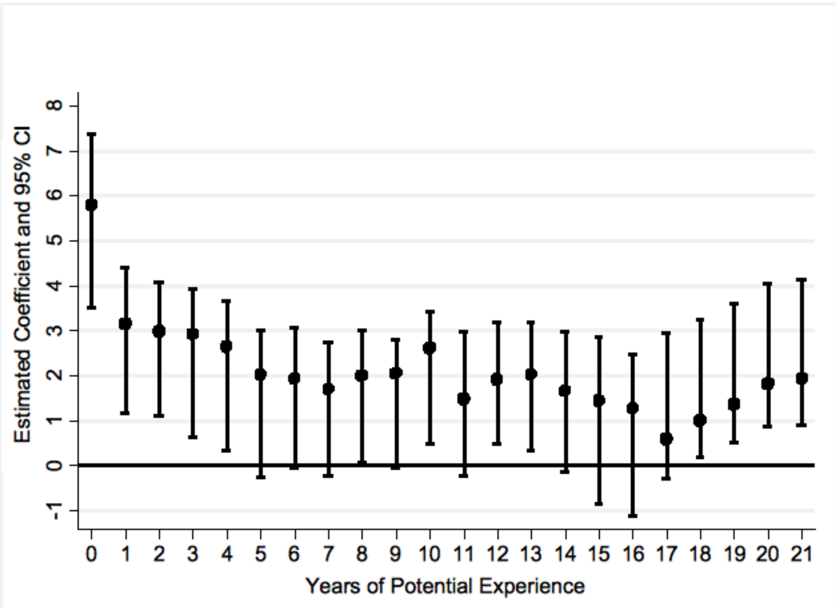
Note: The figure shows the auto covariance structure of the unemployment rates in the UK since labour market entry. *Source:* Labour Force Survey and own calculations.

Figure 3.5: Entry Unemployment Effects by Experience, U.S.



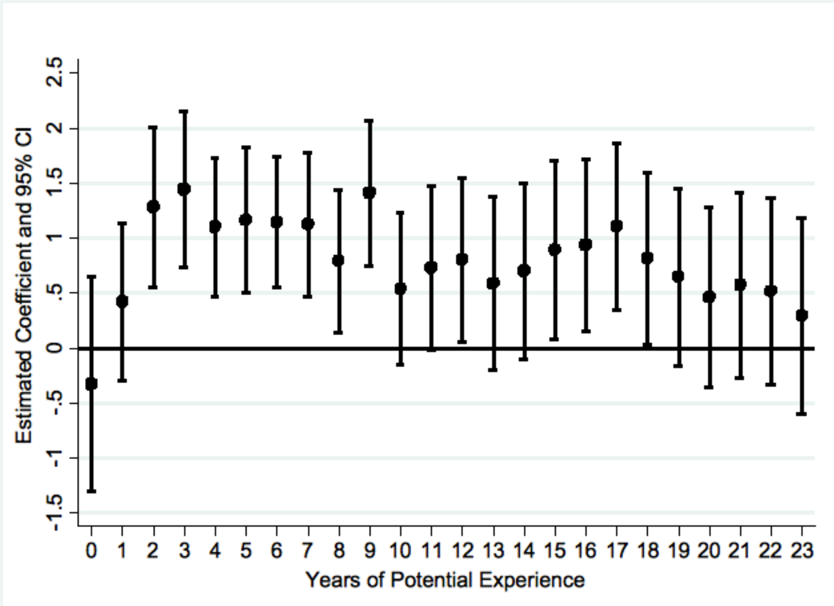
Note: The figure shows the estimated coefficients and 95% confidence intervals on the entry unemployment rate interacted with years of labour market experience for the U.S. The underlying regressions correspond to estimating equation 3.3. *Source:* UCR, U.S. Department of Labor and own calculations.

Figure 3.6: Entry Unemployment Effects by Experience, U.S.
Controlling for Subsequent Unemployment-Experience Interactions



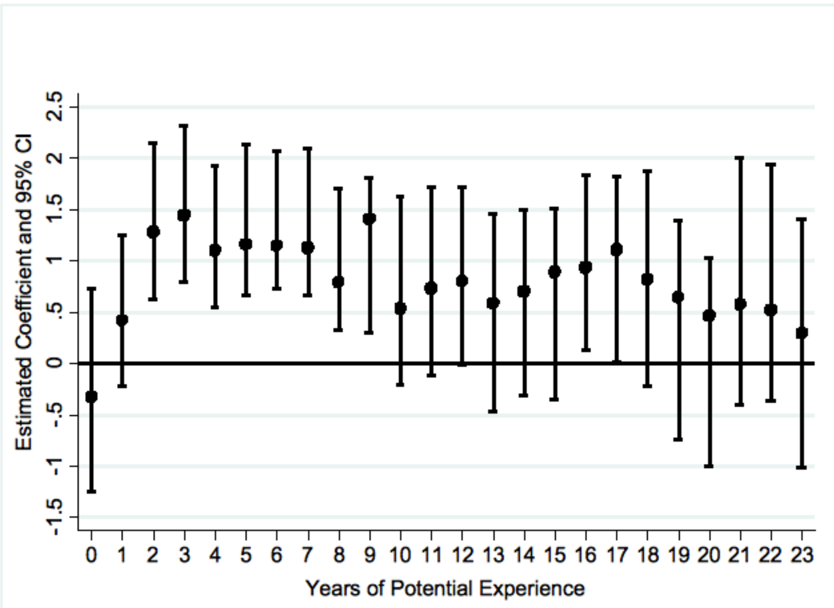
Note: The figure shows the estimated coefficients and 95% confidence intervals on the entry unemployment rate interacted with years of labour market experience for the U.S., whilst controlling for subsequent unemployment-labour market experience interactions. The underlying regressions correspond to estimating equation 3.4. *Source:* UCR, U.S. Department of Labor and own calculations.

Figure 3.7: Entry Unemployment Effects by Experience, UK



Note: The figure shows the estimated coefficients and 95% confidence intervals on the entry unemployment rate interacted with years of labour market experience for the UK. The underlying regressions correspond to estimating equation 3.3. *Source:* OID/PNC, Labour Force Survey and own calculations.

Figure 3.8: Entry Unemployment Effects by Experience, UK
Controlling for Subsequent Unemployment-Experience Interactions



Note: The figure shows the estimated coefficients and 95% confidence intervals on the entry unemployment rate interacted with years of labour market experience for the UK, whilst controlling for subsequent unemployment-labour market experience interactions.. The underlying regressions correspond to estimating equation 3.4. *Source:* OID/PNC, Labour Force Survey and own calculations.

3.8 Tables

Table 3.1: U.S. Male Population in Group Quarters by Type and Age, 1980-2010

	Total Institutionalised	Correctional Institutionalised	Correctional as Percent of Total
1980 Census			
All	1,232,120	439,720	35.7
15-17	68,300	8,460	12.4
18-21	123,320	89,600	72.7
22-24	104,060	80,240	77.1
25-39	301,980	205,780	68.1
1990 Census			
All	1,801,350	1,030,210	57.2
15-17	68,480	16,490	24.1
18-21	149,780	128,940	86.1
22-24	143,890	133,490	92.8
25-39	666,690	581,670	87.2
2000 Census			
All	2,534,060	1,806,260	71.3
15-17	87,200	18,960	21.7
18-21	221,660	202,470	91.3
22-24	201,060	195,660	97.3
25-39	951,660	911,050	95.7
2010 Census			
All	2,716,877	2,059,020	75.8
15-19	153,924	74,720	48.5
20-24	327,760	308,926	94.3
25-39	971,581	945,065	97.3

Note: The table shows the number of male individuals in group quarters by Census year and by age. Column (1) shows the total number of institutionalised individuals, column (2) the number of those who are in correctional facilities and column (3) shows the same figure as percentage of the total institutionalised population. *Source:* The numbers for 1980 are calculated from IPUMS data; those for 1990, 2000 and 2010 come from the U.S. Census Bureau.

Table 3.2: U.S. Cohort Panel Estimates, Basic Specification

	(1)	(2)	(3)	(4)	(5)	(6)
Crime Type:	All	All	Property	Property	Violent	Violent
National Entry U Rate	1.550***		1.419*		1.871***	
at Age 16-18	(0.506)		(0.732)		(0.519)	
State Entry U Rate		2.039***		2.115***		2.156***
at Age 16-18		(0.443)		(0.598)		(0.524)
State Fixed Effects	x	x	x	x	x	x
Year Fixed Effects	x	x	x	x	x	x
Quadratic Cohort Trend	x	-	x	-	x	-
Cohort Fixed Effects	-	x	-	x	-	x
Age Fixed Effects	x	x	x	x	x	x
Compositional Adjustment	x	x	x	x	x	x
Sample Size	19,429	19,429	19,429	19,429	19,429	19,429

Note: The table shows the regression results corresponding to estimating equation 3.1. The dependent variable is the logarithm of the male arrest rate, as calculated from the UCR data. The sample runs from 1980-2010. Individual year-of-birth cohorts run from 1941-1994. We assume that cohorts enter the labour market between the age of 16 and 18. All insured unemployment rates are measured as the average unemployment rate at the three potential years of labour market entry. All regressions include year, age and state fixed effects. We include control variables for cohort compositional adjustments (average share of immigrants, male graduates, black men, married men and females per cohort in that state 1980-2010). Columns (1), (3) and (5) include the cohort-level national unemployment rate at labour market entry and include a cohort linear trend. Columns (2), (4) and (6) include the cohort-level state unemployment rates and include cohort fixed effects. Standard errors are clustered at the state-cohort level and regressions are weighted by the male cell-population. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* UCR, U.S. Department of Labor and own calculations.

Table 3.3: U.S. Cohort Panel Estimates, Basic Specification by Type of Crime

Crime Type:	(1) Burglary	(2) Theft	(3) Arson	(4) Murder	(5) Rape	(6) Robbery	(7) Assault
State Entry U Rate at Age 16-18	2.977***	3.589***	4.692***	-3.094***	3.393***	2.225***	2.106***
	(0.652)	(0.759)	(0.735)	(1.164)	(0.627)	(0.759)	(0.638)
State Fixed Effects	x	x	x	x	x	x	x
Year Fixed Effects	x	x	x	x	x	x	x
Cohort Fixed Effects	x	x	x	x	x	x	x
Age Fixed Effects	x	x	x	x	x	x	x
Compositional Adjustment	x	x	x	x	x	x	x
Sample Size	19,429	19,429	19,429	19,429	19,429	19,429	19,429

Note: The table shows the regression results corresponding to estimating equation 3.1, separately by type of crime. For the specification, see details as for the column (2) specification of table 3.2. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* UCR, U.S. Department of Labor and own calculations.

Table 3.4: U.S. Cohort Panel Estimates,
Allowing for Subsequent Unemployment Rates

Crime Type:	(1) All	(2) All	(3) All	(4) All
State Entry U Rate at Age 16	1.525***		0.797	
	(0.381)		(0.570)	
State Entry U Rate at Age 17			0.236	
			(0.744)	
State Entry U Rate at Age 18			1.481**	
			(0.678)	
State Entry U Rate at Age 16-18		2.039***		1.382***
		(0.443)		(0.483)
State Entry U Rate at Age 19-21			-0.517	-0.384
			(0.536)	(0.541)
State Entry U Rate at Age 22-24			-0.882	-0.904*
			(0.538)	(0.538)
State Entry U Rate at Age 25-27			-0.783	-0.790
			(0.525)	(0.524)
p(sum of 16,17, 18 effects = 0)			0.007***	
State Fixed Effects	x	x	x	x
Year Fixed Effects	x	x	x	x
Cohort Fixed Effects	x	x	x	x
Age Fixed Effects	x	x	x	x
Compositional Adjustment	x	x	x	x
Sample Size	19,487	19,429	19,429	19,429

Note: The table shows the regression results corresponding to estimating equation 3.2. For the specification, see details as for the column (2) specification of table 3.2. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* UCR, U.S. Department of Labor and own calculations.

Table 3.5: U.S. Cohort Panel Estimates,
Effects by Labour Market Experience Groups

Crime Type:	(1) All	(2) All	(3) Property	(4) Violent
State Entry U Rate at Age 16-18*Exp(0-5)	3.609***	3.290***	1.481**	5.151***
	(0.626)	(0.702)	(0.717)	(1.124)
State Entry U Rate at Age 16-18*Exp(6-11)	1.962***	1.705***	0.965**	2.615***
	(0.535)	(0.617)	(0.737)	(0.821)
State Entry U Rate at Age 16-18*Exp(12-17)	1.475***	1.558**	2.151**	0.883
	(0.556)	(0.643)	(0.911)	(0.752)
State Entry U Rate at Age 16-18*Exp(18-21)	1.515***	2.421***	3.345***	2.032***
	(0.566)	(0.707)	(0.959)	(0.859)
State Fixed Effects	x	x	x	x
Year Fixed Effects	x	x	x	x
Cohort Fixed Effects	x	x	x	x
Age Fixed Effects	x	x	x	x
Compositional Adjustment	x	x	x	x
Allowing for subsequent U rates	-	x	x	x
Sample Size	19,487	19,429	19,429	19,429

Note: The table shows the regression results corresponding to estimating equations 3.3 and 3.4. For the specification, see details as for the column (2), (4) and (6) specifications of table 3.2. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* UCR, U.S. Department of Labor and own calculations.

Table 3.6: U.S. Cohort Panel Estimates,
Robustness Tests for Mobility and Age of Entry Unemployment

Crime Type:	(1) All	(2) Property	(3) Violent
A: Age 16-18 Entry U Rate			
Mobility Adjusted State Entry U Rate at Age 16-18	2.470***	2.016**	3.288***
	(0.609)	(0.771)	(0.776)
B: Age 16 Entry U Rate			
Mobility Adjusted State Entry U Rate at Age 16	1.857***	1.483**	2.621***
	(0.527)	(0.662)	(0.668)
State Fixed Effects	x	x	x
Year Fixed Effects	x	x	x
Cohort Fixed Effects	x	x	x
Age Fixed Effects	x	x	x
Compositional Adjustment	x	x	x
Sample Size Panel A	19,429	19,429	19,429
Sample Size Panel B	19,487	19,487	19,487

Note: The table shows the regressions results corresponding to the mobility adjustment of the entry unemployment rate as suggested in equation 3.5. For the specification, see details as for the column (2), (4) and (6) specifications of table 3.2. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* UCR, U.S. Department of Labor and own calculations.

Table 3.7: UK Cohort Panel Estimates, Basic Specification

Crime Type:	(1) All	(2) All	(3) Property	(4) Property	(5) Violent	(6) Violent
National Entry U Rate at Age 16	2.664***		3.443***		0.803***	
	(0.189)		(0.249)		(0.191)	
Region Entry U Rate at Age 16		0.812***		0.712**		1.531***
		(0.277)		(0.350)		(0.365)
Region Fixed Effects	x	x	x	x	x	x
Year Fixed Effects	x	x	x	x	x	x
Quadratic Cohort Trend	x	-	x	-	x	-
Cohort Fixed Effects	-	x	-	x	-	x
Age Fixed Effects	x	x	x	x	x	x
Compositional Adjustment	x	x	x	x	x	x
Sample Size	7,440	7,440	7,440	7,440	7,440	7,440

Note: The table shows the regression results corresponding to estimating equation 3.1. The dependent variable is the logarithm of the male conviction rate from the OID/PNC data. The sample runs from 1980-2010. Individual year-of-birth cohorts run from 1941-1994. We assume that cohorts enter the labour market at age 15/16. All unemployment rates are measured in the year of labour market entry. We include control variables for cohort compositional adjustments (average share of immigrants, male graduates, nonwhite men and married men in each cohort/region, 1980-2010), allowing for differential effects of composition in London. All regressions include year, age, and region fixed effects. Columns (1), (3) and (5) include the cohort-level national unemployment rate at labour market entry and include a cohort linear trend. Columns (2), (4) and (6) include the cohort-level region unemployment rates and include cohort fixed effects. Standard errors are clustered at the region-cohort level and regressions are weighted by the male cell-population. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* OID/PNC, Labour Force Survey and own calculations.

Table 3.8: UK Cohort Panel Estimates,
Allowing for Subsequent Unemployment Rates

Crime Type:	(1) All	(2) All	(3) All	(4) All
Region Entry U Rate at Age 16	0.812***		0.862*	0.815***
	(0.277)		(0.477)	(0.281)
Region Entry U Rate at Age 17			0.143	
			(0.555)	
Region Entry U Rate at Age 18			-0.228	
			(0.471)	
Region Entry U Rate at Age 16-18		0.770***		
		(0.286)		
Region Entry U Rate at Age 19-21			0.048	0.024
			(0.219)	(0.217)
Region Entry U Rate at Age 22-24			-0.102	-0.105
			(0.212)	(0.212)
Region Entry U Rate at Age 25-27			0.129	0.129
			(0.237)	(0.237)
p(sum of 16,17, 18 effects = 0)			0.009***	
Region Fixed Effects	x	x	x	x
Year Fixed Effects	x	x	x	x
Cohort Fixed Effects	x	x	x	x
Age Fixed Effects	x	x	x	x
Compositional Adjustment	x	x	x	x
Sample Size	7,440	7,410	7,410	7,410

Note: The table shows the regression results corresponding to estimating equation 3.2. For the specification, see details as for the column (2) specification of table 3.7. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* OID/PNC, Labour Force Survey and own calculations.

Table 3.9: UK Cohort Panel Estimates,
Effects by Labour Market Experience Groups

Crime Type:	(1) All	(2) All	(3) Property	(4) Violent
Region Entry U Rate at Age 16*Exp(0-5)	0.861***	0.968***	0.972***	1.034*
	(0.305)	(0.316)	(0.362)	(0.532)
Region Entry U Rate at Age 16*Exp(6-11)	0.914***	0.970***	1.050***	0.996**
	(0.284)	(0.298)	(0.365)	(0.444)
Region Entry U Rate at Age 16*Exp(12-17)	0.832**	0.810**	0.733	1.369***
	(0.343)	(0.358)	(0.448)	(0.435)
Region Entry U Rate at Age 16*Exp(18-21)	0.583	0.530	0.124	2.701***
	(0.369)	(0.406)	(0.502)	(0.504)
Region Fixed Effects	x	x	x	x
Year Fixed Effects	x	x	x	x
Cohort Fixed Effects	x	x	x	x
Age Fixed Effects	x	x	x	x
Compositional Adjustment	x	x	x	x
Allowing for subsequent U rates	-	x	x	x
Sample Size	7,440	7,440	7,440	7,440

Note: The table shows the regression results corresponding to estimating equations 3.3 and 3.4. For the specification, see details as for the column (2), (4) and (6) specifications of table 3.7. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* OID/PNC, Labour Force Survey and own calculations.

Table 3.10: UK Cohort Panel Estimates,
Short- and Long-Term Entry Unemployment Rates

	(1)	(2)	(3)
Crime Type:	All	Property	Violent
Region Entry Short-Term U Rate at Age 16	-1.188*	-1.008	-1.074
	(0.620)	(0.767)	(0.933)
Region Entry Long-Term U Rate at Age 16	1.687***	1.466***	2.673***
	(0.372)	(0.474)	(0.464)
Region Fixed Effects	x	x	x
Year Fixed Effects	x	x	x
Cohort Fixed Effects	x	x	x
Age Fixed Effects	x	x	x
Compositional Adjustment	x	x	x
Sample Size	7,440	7,440	7,440

Note: The table shows the regression results corresponding to estimating equation 3.1 when we differentiate between short- and long-term unemployment. For the specification, see details as for the column (2), (4) and (6) specifications of table 3.7. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* OID/PNC, Labour Force Survey and own calculations.

Table 3.11: U.S. Individual-Level Estimates, Census/ACS Incarceration Regressions, 1980-2010

Sample:	(1) All Males	(2) HS Dropouts	(3) HS Grads	(4) 4yr College
A. Aged 18 and Over				
State Entry U Rate at Age 16-18	0.031**	0.120**	0.017	-0.004
	(0.015)	(0.053)	(0.025)	(0.009)
B. Aged 18 and Over, 1980 Redefined				
State Entry U Rate at Age 16-18	0.026*	0.137***	0.013	-0.010
	(0.015)	(0.052)	(0.024)	(0.008)
Year Effects	x	x	x	x
State Effects	x	x	x	x
State/Race Effects	x	x	x	x
Cohort Effects	x	x	x	x
State of Birth Effects	x	x	x	x
Age Quartic	x	x	x	x
Sample Size	5,760,227	798,692	2,553,430	1,169,645

Note: The table shows the results from estimating probability models as outlined in section 3.3. The dependent variable is a dummy equal to 1 if the individual is institutionalised and 0 otherwise. The sample covers males aged 18-39 who are not in school, and born in the United States. Entry unemployment is the unemployment rate at age 16 in the state of birth. Data are from the 1980, 1990 and 2000 5 percent IPUMS US Census and the 2008-2012 IPUMS ACS. Regressions also include marital status, race, education and veteran status indicators. Standard errors are clustered at the state/cohort level and regressions are weighted with the Census person weight. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* U.S. Census/ACS, U.S. Department of Labor and own calculations.

Table 3.12: UK Individual-Level Estimates,
Self-Reported Arrest Regressions, 2001/2 to 2010/11

Sample:	(1) Ever Arrested	(2) Ever Arrested, Age 16 Qualification	(3) Ever Arrested Age 18+ Qualification
Region Entry U Rate at Age 16	0.246** (0.123)	0.508*** (0.159)	0.050 (0.208)
Year Dummies	x	x	x
Personal Controls	x	x	x
Mean of Dep. Var.	0.215	0.303	0.164
Sample Size	22,646	7,984	9,166

Note: The table reports the estimated marginal effects from a probit model as outlined in section 3.3. The dependent variable is a dummy if the individual has ever been arrested and 0 otherwise. Personal controls include age (10 categories), ethnic group (5 categories), education (9 categories where appropriate), student status, marital status (4 categories), income (18 categories), economic status (15 categories), number of children (10 categories), housing tenure (8 categories), years at address (9 categories), years in area (9 categories), and government office region (10 categories). The sample covers ages 16 to 65 and pools data from the British Crime Surveys, 2001-2002 to 2010-2011. Regressions use individual sample weights. Standard errors in parentheses are clustered at the government office region level. *Source:* British Crime Survey, Labour Force Survey and own calculations.

Chapter 4. Still Unemployed, What Next? Crime and Unemployment Duration

4.1 Introduction

Economists have thought of criminal activity as the supply side of crime markets (Freeman, 1999). In that sense, crime markets are considered to be alternative labour markets which offer illegal instead of, or in addition to, legal job opportunities. Think about two otherwise identical individuals, one of whom becomes unemployed and the other one remains employed. The unemployed faces lower returns on the labour market, and at the margin that person is more likely to accept illegal job opportunities and to engage in crime than the employed counterfactual (Becker, 1968).

Whilst the literature on the impact of unemployment on crime has grown considerably over the last two decades, the empirical evidence is still not indisputable.[1] In this chapter, I examine recent and unprecedented structural changes in the U.S. labour market and study the relationship between labour market conditions and crime in that context. To begin with, I estimate the causal effect of unemployment on crime exploiting quasi-experimental variation in unemployment benefit durations which is caused by temporary policy changes. In line with previous findings, I find that higher unemployment is linked to higher crime rates. More surprisingly, my results suggest that this effect is driven by the unemployment benefit extensions which are associated to longer unemployment durations and higher unemployment rates on the one hand and which exhibit a crime increasing effect on the other hand. Indeed, combining these observations I argue that the at first surprising effects on criminality can be explained by the dramatic increases in unemployment durations in the United States. Exploiting the given variation in unemployment and unemployment benefit durations, I demonstrate a contemporaneous but yet more dynamic relationship between unemployment and crime than found in the previous empirical literature.

To the best of my knowledge this is the first study of the recent structural changes on the U.S. labour market with respect to crime outcomes, and in particular the first attempt to evaluate the role of temporary unemployment benefit extensions in that context. The existing empirical literature has focused on the contemporaneous link between unemployment and crime, in particular for youth unemployment. The previous chapter of this dissertation presents evidence for a longer-lasting impact of unemployment on crime, based on the notion that the experience of unemployment triggers initial involvement in crime leading to criminal careers. This chapter con-

[1] See chapter 2 of this dissertation for a more detailed discussion of the literature. Alternatively, the interested reader might refer to the respective chapter for example in Mustard (2010), or more recently Chalfin and McCrary (2015) and Draca and Machin (2015).

tributes to that literature as, to the best of my knowledge, it is the first empirical paper to explicitly evaluate the relationship between unemployment duration and crime and to introduce a more dynamic dimension to the analysis.

Why should the probability to engage in crime depend on how long one has been unemployed for? The standard model of crime argues that unemployment, or a higher risk of unemployment, lowers the expected returns on the legal labour market and hence increases the probability of engaging in crime. Yet, the crime market is a risky alternative to the labour market: If one is caught for crime, then sanctions, income cuts and social stigma follow. Hence, a frictionless transition from entering unemployment and turning to crime is unlikely. Again, think about two otherwise identical individuals who enter unemployment. One of them finds a job and exits unemployment shortly after, and the other one remains unemployed. At the margin, the still unemployed individual is more likely to become criminal than the re-employed counterfactual: Economic conditions become more severe the longer the individual is unemployed for and human capital depreciation further decreases expected future wages. Expected legal returns decline and higher expected relative returns to illegal action now increase the propensity of crime (Becker, 1968, Cantor and Land, 1985).

Typically, there is initial support through unemployment benefits and from family or other social networks when one enters unemployment. Unemployment benefits are available for a fixed period of time only and when they expire, the individual faces a cut in income. That mechanism sets strong incentives to search for jobs and to exit unemployment before the known cutoff date. Again, think about two otherwise identical unemployed individuals, one of whom receives unemployment benefits for a longer time period. At the margin, that person is disincentivised, remains unemployed for longer and subsequently becomes more likely to commit crime than the re-employed counterfactual. However, compared to an otherwise identical unemployed individual who has expired unemployment benefits, that person is better off in financial terms: At the margin, the unemployed who receives benefits is less likely to commit crime than the counterfactual without benefits, everything else being equal.

Expected relative returns on the labour market are not only affected by personal unemployment but also by aggregate labour market conditions: If unemployment rates are high, competition for jobs is high and expected wages decrease. In turn, relative returns to crime increase. These effects might be stronger if average unemployment durations are long: Shorter unemployment durations might reflect search frictions that do not evidently lead to more crime. In contrast, longer durations might capture more substantial problems on the labour market and may thus be a predictor for criminal activity.

The remainder of this chapter is structured as follows. Section 4.2 summarises the existing literature. Section 4.3 presents a conceptual framework for my analysis, discussing the economic mechanisms that underlie the link between labour markets and crime and relating the research question to existing models of crime. In section 4.4, I describe the U.S. state level data from the Uniform Crime Reports and the Current Population Survey as well as the unemployment benefit extension policies. Section 4.5 outlines the empirical strategy. Empirical evidence on the causal effect of unemployment on crime is presented in section 4.6, the duration dependence of that relationship is discussed in section 4.7. Section 4.8 concludes.

4.2 Previous Literature

An early economic framework of crime and labour markets is provided by Becker (1968), Ehrlich (1973) and Block and Heineke (1975).[2] More recently, Raphael and Winter-Ebmer (2001) estimate the relation between unemployment and crime in the U.S. from 1971 to 1997. Using military spending and oil price shock instruments, they find elasticities of property crime rates with respect to unemployment rates between 2.8 and 5.0 percent. Gould *et al.* (2002) examine the link between crime rates of young men and unemployment. Using a U.S. panel of counties from 1979 to 1997, they also find significant positive effects. Further, they demonstrate that low wages are a better predictor for long-term crime patterns than unemployment. Grönqvist (2013) uses Swedish register data to examine the relation between youth unemployment and crime. The author finds strong positive effects and demonstrates that the relation between unemployment and crime is mostly predicted by an increase in available time and opportunities for crime. Bell *et al.* (2015) study scarring effects of unemployment at labour market entry on crime outcomes later in life. Using data both for the United States and the United Kingdom, they find substantial and persistent effects, as discussed in more detail in chapter 3 of this dissertation.

Using French data from 1990 to 2000 and an industrial structure type instrument, Fougère *et al.* (2009) find that the youth unemployment rate, but not the overall unemployment rate, is a strong causal predictor for crime rates. They find no significant association between the long-term unemployment rate and crime, however they demonstrate a positive link between not receiving unemployment benefits

[2]For details on the seminal economic models of crime as well as on a more extensive discussions of the studies mentioned in the following, see chapter 2 of this dissertation.

and crime. In contrast, Almén and Nordin (2011) use Swedish panel data on municipality level from 1997 to 2009 to examine the relationship between long-term unemployment and crime and find strong links using a corporate bankruptcies instrument.

In terms of the underlying economic mechanisms, this paper relates to the theoretical literature on dynamic models of crime. Burdett *et al.* (2003) derive an equilibrium search model of crime, inequality and unemployment. They find that the model generates wage dispersion and multiple equilibria. They use that multiplicity to explain different crime rates across otherwise similar neighborhoods, and to explain the relation between local labour market conditions and crime. In a follow-up paper, Burdett *et al.* (2004) extend that model by incorporating on-the-job-search, again detecting multiple equilibria. Further, they find that an increase in the unemployment insurance replacement rate increases unemployment and crime rates, respectively. Mocan *et al.* (2005) derive a dynamic model taking into account individual human capital decisions over the life-cycle, differentiating human capital into legal and criminal human capital. The model predicts that the relationship between unemployment and crime is asymmetric: Whereas the standard models predict that a decrease in unemployment leads to a decrease in crime, the authors find that this is not necessarily the case due to legal human capital depreciation and criminal knowledge accumulation.

This chapter examines recent structural changes in the U.S. labour market and temporary changes in unemployment benefit policies and studies the relationship between labour market conditions and crime in that context. Hence, the findings in the chapter relate not only to the literature on unemployment and crime, but also to a long-standing literature on the impact of unemployment benefits on unemployment durations as well as a more recent literature on the effects of unemployment benefits on criminal behaviour.

In terms of the effect of unemployment benefits on unemployment and unemployment duration, Katz and Meyer (1990) and Meyer (1990) find a spike in exit rates from unemployment close to the benefit exhaustion date. More recently and in the context of the recent changes in the U.S. labour market, Rothstein (2011) provides evidence that unemployment benefit extensions during the Great Recession had significant negative, albeit small effects on unemployment exit probabilities. The author finds that these effects are driven by the long-term unemployed, and that changes in the unemployment rate due to the extension policies are mainly attributed to reduced exits from the labour force. Farber and Valletta (2013) confirm these results, and find similar effects for a milder recession in the earlier 2000s.

Hagedorn *et al.* (2015) look at the macro effects of the unemployment benefit extensions during the Great Recession. In contrast to the other studies, the authors find that there are large effects of benefit extensions on unemployment which are driven by the response of the job creation. Schmieder *et al.* (2012) study the effect of unemployment insurance extensions on unemployment duration over the business cycle using German data. The authors find that the moral hazard effect of benefit extensions is smaller during recessions than in booms. Lalive *et al.* (2015) study spillover effects of unemployment insurance extensions in a quasi-experimental setting for Austria. The authors relate their findings to the temporary policy changes in the U.S. and argue that while existing studies find small macro elasticities of unemployment durations with respect to the benefit extensions this might be due to large search externalities and that the micro elasticities might be larger.

In terms of unemployment insurance and crime, Engelhardt *et al.* (2008) derive a search model including crimes and optimally determined contracts to study the effects of labour market and crime policies. The authors find that a more generous unemployment benefit system reduces the crime rate for the unemployed, and has a more ambiguous effect on the employed depending on job durations and jail sentences. Polito and Long (2014) study a job search model of unemployment, crime and social insurance with random criminal opportunities. Modeling these opportunities as a moral hazard problem, they find that under certain conditions decreasing unemployment benefits reduce the expected return to criminal action compared to job search. The model implies that it is optimal to front-load benefits and to reduce benefits over time. Machin and Marie (2006) empirically study the effect of a reform in the Jobseekers Allowance in 1996 on crime in England and Wales. The authors find evidence that the toughening of the unemployment benefit regime leads to higher crime rates.

4.3 Conceptual Framework

In the standard Becker (1968) economic model of crime, rational decision makers choose between legal and illegal options in order to maximise utility. The returns to legal activity are determined by expected returns to the specific activity, e.g. wages or unemployment benefits. The returns to illegal activity are based on the anticipated crime payoff, the probability of getting caught and the imminent sanction in the case of being caught. Agents who are caught for crime might lose returns on the legal labour market: If previously employed, they can lose their job; if previously unemployed, they can lose benefit eligibility; in both cases they can end up in prison and face labour market discrimination due to a criminal record.[3] The individual chooses to commit crime if the expected utility from illegal activity outweighs the expected utility from legal activity.[4] In that model, higher unemployment lowers the expected returns from legal activity and hence affects the relative returns to crime. At the margin, an agent who becomes unemployed, or indeed encounters a higher risk of unemployment, is more likely to commit crime than otherwise.

The model is explicitly static, yet the effect of unemployment is likely to vary with the duration of unemployment. Mocan *et al.* (2005) suggest a dynamic model of differential human capital and criminal activity. Assume that individuals are endowed with both legal and criminal activity specific human capital. Both evolve over time depending on participation in either market, on human capital depreciation, and on investment in legal human capital acquisition. Expected gains from either legal or criminal activity thus depend on both types of human capital and respective returns. During unemployment, the legal human capital stock and returns to legal human capital fall, hence involvement in criminal activity rises. Agents accumulate criminal know-how and the criminal human capital stock grows subsequently, increasing the expected returns to crime.

Here, unemployment impacts on illegitimate behaviour through the evolution of both legal and criminal human capital. In that sense, the effect varies over the duration of unemployment: During unemployment, legal human capital depreciates while the criminal human capital stock grows. If the unemployment spell is short, the agent has an initial incentive to enter the criminal sector, but exits unemployment before returns to criminal human capital are higher than returns to legal human capital. If the unemployment spell is sufficiently long, then the criminal human capital grows for longer and the legal human capital stock depreciates further, triggering more permanent criminal behaviour. The human capital mechanisms suggest the following hypothesis: Unemployment initially shifts the individual towards the threshold to crime, at the margin leading to a more persistent and potentially increasing propensity of crime the longer the unemployment spell lasts.

At the beginning of an unemployment spell the unemployed typically receives unemployment benefits. In many developed countries, the benefit amount depends on previous wages and the employment history. If the unemployment duration

[3]See for example Baert and Verhofstadt (2015) for a recent empirical study on labour market discrimination of former juvenile delinquents. In a field experiment, the authors find that labour market entrants who disclose a criminal record receive about 22 percent less callbacks than counterfactuals without a criminal record.

[4]The standard economic model described here applies to all types of crime. However, one may argue that it is more applicable to income generating crime such as property crime or drug dealing rather than to violent crime.

exceeds a cutoff length, individuals are no longer eligible for unemployment benefits and income is reduced to lower welfare benefits. All else equal, that further reduces returns to legal activity. This income mechanism supports the above hypothesis that at the margin the unemployed becomes more likely to commit crime the longer the unemployment spell lasts.[5]

Unsurprisingly, other than the two mechanisms described above there are alternative explanations for the associations between crime, unemployment and unemployment duration which include for example the following. First, the arguments above relate in a broader sense to the literature on the effect of low wages on crime:[6] Decreasing returns to legal activity imply decreasing opportunity cost of crime for longer unemployment durations. The potentially lost income from legal activity when being caught for crime decreases compared to shorter periods of unemployment. The inverse deterrence effect, here triggered by longer unemployment durations, leads to higher crime propensities. Second, the literature on the impact of education on crime discusses the role of patience.[7] A similar argument might be made here: If patience decreases over the course of an unemployment spell, e.g. prompted by unsuccessful job search, the valuation of relative returns to criminal action changes. At the margin, an individual might be shifted towards and across the crime threshold who otherwise would not have committed crime. Other behavioural patterns might change and affect crime behaviour in a similar way. Third, it has been shown that social networks play an important role in determining criminal behaviour: Once unemployed, social networks are likely to change. Entering new social networks which include other criminals can affect and trigger criminal behaviour.[8]

4.4 Data Description

The theoretical notions outlined above can be tested in an empirical framework. In this chapter, I use U.S. state level data to estimate the link between unemployment duration and criminal behaviour. In the following, I describe the different data sources and discuss the sample specifics.

[5]That argument implies that ceteris paribus unemployment benefits prevent individuals from committing crime. I will discuss this point at a later stage.

[6]See for example Grogger (1998) or Machin and Meghir (2004), as well as the discussion in chapter 2 of this dissertation.

[7]See for example Lochner (2004), Lochner and Moretti (2004) or Machin *et al.* (2011), as well as the discussion in chapter 2 of this dissertation.

[8]See for example Glaeser *et al.* (1996) or Glaeser *et al.* (2003), as well as the discussion in chapter 2 of this dissertation.

4.4.1 Crime Data

Data on criminality comes from the master files of the Uniform Crime Reporting programme, UCR hereafter. Since 1930, law enforcement agencies in the United States have been participating in gathering crime statistics through the Uniform Crime Reporting programme. The programme is administered by the FBI and participation is voluntary for all agencies.

The UCR report the monthly number of arrests by state, age, gender and type of crime. Types of crime include the FBI categories for property crime (burglary, larceny, vehicle theft, arson), for violent crime (murder, rape, robbery, assault) and for drug crime (substance sale and manufacturing, possession). The age variable in the original data indicates single age years up to the age of 25, and five year brackets for ages above 25 (25-29, 30-34, etc.). Here, the sample is restricted to the 16 to 39 year old population, reflecting typical crime demographics as well as potential labour market entry ages. Furthermore, I aggregate the crime data up to the age of 25 to the age groups 16 to 19-years old and 20 to 24-years old. The resulting sample is a monthly panel of the number of arrests by state, age group, gender and type of crime.

The arrest data is matched to population data in order to produce arrest rates. Population data is retrieved from the U.S. Census Bureau Population Estimates. The population is measured as the annual population by state, age and gender. I match the population data to the arrest data by year, state, age and gender, implicitly assuming that the population number is constant with respect to each month within a year.[9] Arrest rates are calculated as the number of arrests divided by the population count in the observational unit, scaled by 100,000 for the ease of interpretation.

Here, criminality is measured by arrests. Yet, the number of arrests does not necessarily equal the number of crimes. First, not all arrestees are offenders. To my knowledge consistent monthly data at a similar observational level as the arrest data does not exist for the U.S., neither with respect to incarceration nor with respect to convictions.[10] Second, not all crimes lead to arrests. If a crime is not detected or not reported to the police, no arrest can be observed. Victimisation data is more informative in that respect, but again to my knowledge consistent monthly data at a similar observational level as the arrest data does not exist for the U.S. Furthermore,

[9] To my knowledge, monthly population data is not available at the same level of observation. Thus, alternatives to this approach would be to either calculate arrest rates relative to total population numbers instead of observational cell specific population numbers, or to linearly interpolate population numbers for each month in one year.

[10] See for example Pfaff (2011).

victimisation data is typically collected from surveys and hence is subject to its own type of measurement error.

Figure 4.1 shows the correlation between U.S. victimisations and arrests per 1,000 U.S. population as published by the Bureau of Justice Statistics. As expected, the number of victimisations is higher than the number of arrests, but there clearly is a positive correlation between both measures of criminality. That suggests that arrests are an informative measure of criminality.[11] A particular problem may arise if petty crimes lead to less arrests than felonies. The notion that individuals become more likely to commit crimes while unemployed may be more applicable to petty crime than to felonies, in particular with respect to first time offenders. If unemployment triggers more petty crimes than felonies and if the likelihood of an arrest for petty crimes is lower, the results in this paper would be downward biased.

For a number of states, the arrest data is either systematically missing or not provided in every month within a year. States with systematically missing data are excluded from my sample. Also excluded are states for which the arrest data covers less than 95% of the state population.[12] States with partly missing data are included for the non-missing time periods and excluded otherwise, leading to an unbalanced sample. Where it is appropriate, I impute single missing values using linear interpolation. Figure 4.2 and Table 4.1 provide more details on the geographical distribution of the sample.

4.4.2 Unemployment Data

Unemployment data comes from the Current Population Survey, CPS hereafter. The CPS is a monthly cross-sectional survey of U.S. households conducted by the Bureau of Census for the Bureau of Labor Statistics. As such, the CPS provides information about the employment situation of surveyed individuals and households. The dataset contains information on the duration of an ongoing unemployment spell of the interviewed person as well as a large number of other labour market and demographic variables. The CPS is the basis for official unemployment statistics by the Bureau of Labor Statistics. Here, unemployment is measured according to the common definition by the International Labor Organiszation (ILO) as being not

[11] If arrest rates are in constant proportion to true unobserved crime rates, the proportionality factor is an additive component of the logarithmic arrest rate. Further, if the unobserved proportionality factor is independent of the explanatory variable in the regression model, then the crime-arrest measurement problem is equivalent to the case of classical measurement error in the outcome variable which does not result in an estimation bias (see Imbens and Hyslop (2001)).

[12] There is no evidence that suggests that the excluded states differ systematically from the included states in terms of unemployment durations. Hence, unbalanced sample or selection biases seem to be unlikely.

employed, available and looking for work. Unemployment duration is measured as the elapsed duration of the ongoing unemployment spell.

In order to match the observational level of the arrest data, I compute unemployment rates and average unemployment durations as well as averages for compositional control variables by month, state, gender and age group. The unemployment rate is defined as the percentage of the labour force who is unemployed. Compositional control variables include the share of married persons, the share of native born persons, the share of high-school graduates and the share of the black population.

For this study, I calculate the average unemployment duration per observational unit where unemployment duration is measured as the length of the ongoing unemployment spell at the time of the interview. There are two main concerns with that type of data. On the one hand, reported unemployment spells are uncompleted, i.e. right-censored. Right-censoring means that the average unemployment duration is underestimated. On the other hand, unemployment spells that occur between two interview dates are not observed. Thus, short unemployment durations are underrepresented which leads to length-biased sampling and to an overestimation of the average unemployment duration.[13] The empirical results are tested using the median instead of the average unemployment duration, but otherwise rely on the validity of the duration measurement.

4.4.3 Unemployment Benefit Extensions

The U.S. unemployment insurance system consists of a federal-state unemployment insurance programme that provides temporary financial assistance to individuals who are unemployed and meet the eligibility criteria. Guidelines for eligibility, benefit amounts and benefit periods are given by federal law, whereas the exact legislation is determined by state law. Eligibility requirements include conditions on wages and time worked during a certain period. Generally, eligibility is based on employment in covered work for a base period of 12 months. Only workers who are unemployed "through no fault on their own" meet eligibility requirements. The benefit amount is determined based on a share of an individuals's recent earnings. The maximum benefit period typically lasts for a maximum of 26 weeks, but can vary between states.[14] Yet, the individual benefit period can vary substantially and,

[13]For a more detailed discussion of problems with the measurement of unemployment durations in the CPS see for example Kiefer *et al.* (1985) or Kiefer (1988).

[14]For the considered sample period, the benefit period amounts to 26 weeks in all states except for Massachusetts and Washington, where it amounts to 30 weeks. See the online unemployment insurance state law information by the Bureau of Labor Statistics and the Department of Labor for further details.

dependent on eligibility criteria, can be as short as one week.

In times of economic downturn and high unemployment and based on state-level criteria the maximum potential benefit duration (PBD hereafter) can be extended. During the sample period 2003 to 2011 different benefit extension policies have been in place. Information on unemployment benefit extension policies is retrieved from weekly trigger reports published by the United States Department of Labor, and the Bureau of Labor Statistics. The trigger reports indicate whether states have triggered onto an extension programme, and indicate the number of weeks of extension for the states who have triggered on. The reports are published on a weekly basis. In order to match the observational level of the crime and unemployment data, the median maximum potential benefit duration per state and month is calculated.

Figure 4.3 and table 4.2 summarise the unemployment benefit extension policies and trigger mechanisms for the different policies as they are described in the following. A general extension programme, the Extended Benefit programme (EB hereafter), is activated by trigger mechanisms based on the levels of the insured unemployment rate (IUR hereafter) and the total unemployment rate (TUR hereafter) in the respective state. Depending on the level of IUR and TUR the maximum number of weeks of unemployment benefits may be extended by 13 weeks or 20 weeks in cases of particularly high unemployment. EB benefits are financed to equal parts by the federal government and by the state government.

Additional extension programmes are put in place in times of economic crisis. The Temporary Extended Unemployment Compensation (TEUC hereafter) is implemented between March 2002 and March 2004. The TEUC provides additional weeks of federally-funded unemployment benefits to workers who have exhausted regular unemployment benefits. In all states, a maximum of 13 additional weeks is available. In states that trigger onto the so-called TEUC-X programme, up to another 13 weeks of unemployment benefits are made available.

In June 2008, the Emergency Unemployment Compensation 2008 (EUC08 hereafter) programme has been signed into law. The programme consists of different tiers which are implemented at various points in time. Tier 1 of EUC08 provides 13 additional weeks of unemployment benefits to eligible individuals in all states, and 20 additional weeks respectively from November 2008 onwards. Tier 2 provides 13 weeks in states with high unemployment. High unemployment is again determined by trigger thresholds of the insured and the total unemployment rates. Tier 2 is changed to 14 weeks in all states independent of trigger mechanisms from November 2009 onwards. Also in November 2009, Tier 3 and Tier 4 are enacted. Under Tier 3, based on IUR and TUR trigger mechanisms, up to 13 additional weeks of unemployment benefits are made available. Under Tier 4, additional 6 weeks are provided in states that fulfill again higher unemployment trigger criteria. In addition to the tiers of EUC08, the EB programme is still in place during that period.[15] Thus, if a state triggers onto all tiers and the EB programme is in effect, up to 99 weeks of unemployment benefits are available to an eligible individual.

In order to be eligible for claiming EUC08 benefits, regular unemployment benefits have to be exhausted and the unemployed individual is required to have been in insured employment for at least 20 weeks or to have the equivalent in insured wages in the base period. If eligible, the EUC08 extended benefit amounts to the equivalent of the unemployed's regular weekly unemployment compensation scheme.

The EUC08 programme is a federally funded programme. From February 2009 onwards, states were not allowed to actively reduce unemployment compensation benefits through changes to benefit amounts. Alternatively, some states responded by reducing the baseline benefit period. In the data used in this study, these reductions in the baseline unemployment benefit period are taken into account.

The potential benefit duration can exceed the actual benefit duration due to benefit eligibility criteria. Figure 4.4 shows the national average duration of persons collecting unemployment insurance benefits. For the graph, these benefits include regular unemployment insurance as well as EUC08 extended benefits. Clearly, the *actual* benefit duration is shorter than the *potential* benefit duration for the reasons above. However, one can see a steep increase in the average actual benefit duration with the introduction of the extended benefits.

The trigger mechanisms for the policies and for the tiers within the policies are explicit, as outlined in table 4.2. They are based on a combination of exact thresholds with respect to the current and sometimes also the lagged insured and total state unemployment rates, and thus not trivial to anticipate. Figure 4.5 shows the share of states that offer *any* unemployment benefit extensions between 2003 and 2011. The strict trigger thresholds create variation in the maximum benefit duration across states and over time, as shown in figure 4.6. As expected, most variation evolves during the recessions towards the beginning and the end of the sample period.

4.4.4 Sample and Sample Descriptives

The sample ranges from January 2003 to December 2011, and is restricted to the 16 to 39 year old population. That results in a sample of 24,297 data points at the state, month, age group and gender level. Table 4.3 provides sample sum-

[15]States governments decide whether EB or EUC08 is paid first in the respective state.

mary statistics. The average number of arrests per 100,000 population amounts to 103 for property crime, 117 for violent crime and 107 drug related crime. Standard deviations are larger for drug crime than for the two other types of crime. The average unemployment duration amounts to 18.6 weeks with a relatively high variation, compared to an average potential benefit duration of 48.97 weeks. The mean unemployment rate, averaged over the observational cells, is 9.97%. That unemployment rate is higher than the general BLS state unemployment rate, as the sample is restricted to younger age groups. In terms of compositional control variables, the average share of married persons in the sample is about 37%, the average share of native born persons is about 86%, the average share of high-school graduates is about 78% and the share of the black population is about 11%.

Figures 4.7 to 4.12 show the sample distribution and time trends of the U.S. average arrest rates with respect to property, violent and drug crime. The distribution of the arrest rates is right skewed for all three crime types: Crime is a rare event, and low arrest rates are more common than high arrest rates. Applying logarithms to the arrest rates in the regressions, the distribution is more centred around zero for all three types of arrest rates. The time trends of the average arrest rates suggest an upwards trend in the property crime rate from 2006 onwards.[16] For the violent and drug crime rates, no trends are evident from the graphical analysis. Yet, it is noteworthy that the arrest rates shown in the graphs are averages over the states in the sample and do not necessarily reflect the state specific crime trends that are used in the regression analysis.

Figure 4.13 shows the unemployment rate as well as the unemployment in- and outflows for the U.S. from 2003 to 2011. The graph shows a steep increase in the unemployment rate from 2008 onwards that coincides with an increase in the inflows into unemployment and with constant outflows from unemployment. Figure 4.14 shows the average state unemployment duration as measured in weeks within the sample. Most strikingly, there is a very large increase during the Great Recession: While the average duration amounts to about 15 weeks at the beginning of the sample period, the graph shows an almost 15 week increase in the *average* unemployment duration. Two observations are particularly interesting here: First, the increase in average unemployment rates starts around January 2008 whereas the increase in average unemployment durations seems to start about one year later in January 2009. Second, while unemployment rates are elevated during an earlier recession at the beginning of the sample period, average unemployment durations do

[16]Note that the sample period follows a longer period of a sharp decline in crime in the U.S. The trends seen in the graphs can be understood as a snapshot of longer term trends, and do not offset the overall decline in crime over the last decades.

not seem to be similarly affected then as they are later during the Great Recession. These observations highlight the dramatic changes in the U.S. labour market during the last recession. Whereas European labour markets have experienced long-term unemployment for much longer, this is a more recent phenomenon for the U.S.

4.5 Empirical Strategy

In the following section, I outline the empirical strategy for the estimations in this chapter. First, the causal effect of unemployment on crime is estimated exploiting quasi-experimental variation in unemployment benefit durations caused by the temporary policy changes as described above. Let CR_{tsag} denote the arrest rate for period t, state s, age group a and gender g. The dependent variable in the regression model is the logarithmic arrest rate $\ln(\mathrm{CR})_{tsag}$, separated by crime type for property, violent and drug crime. The explanatory variable of interest is the logarithmic unemployment rate $\ln(\mathrm{UR})_{tsag}$ for period t, state s, age group a and gender g, defined as the percentage of the labour force who is unemployed. Let X_{tsag} be a matrix of observable characteristics to account for compositional differences between the data cells and let α_s, α_g and α_a denote fixed effects for state, gender and age group. Further, let $f(t_s)$ be a quadratic state-specific time trend, and ε_{tsag} the error term. Standard errors are clustered at the state level, and regressions are weighted by the population of the observational unit. The fixed effects model, exploiting variation within and across states, can be written as:

$$\begin{aligned}\ln(\mathrm{CR})_{tsag} = \beta_0 + \beta_1 \ln(\mathrm{UR}_{tsag}) + \beta_2 \ln(\mathrm{UR}_{t-1,sag}) + \beta_3 X_{tsag} \\ + \sum_{i=s,g,a} \alpha_i + f(t_s) + \varepsilon_{tsag}\end{aligned} \tag{4.1}$$

The fixed effects take unobserved heterogeneity problems into account which occur when underlying unobserved differences between data cells are correlated with the variables of interest. For example, if cultural differences between the sampled regions systematically affect crime and labour market attitudes, one region may display higher crime rates *and* worse labour market conditions independent of the effect of interest for this study.

Still, ex ante the direction of causality between crime behaviour and unemployment is not obvious leading to reversed causality concerns. For the type of data used in this analysis, one might be mostly concerned about a recession bias (Cook and Zarkin, 1985, Raphael and Winter-Ebmer, 2001). In particular, recessions not only affect labour markets but also the quality and quantity of criminal opportunities: Potential victims for example have less income and consume less during recessions than they would do during more buoyant times. If that is the case, then there is procyclical variation in criminal opportunities which creates a downward bias in the estimates of the impact of the (tautologically procyclical) unemployment rate and crime.[17] In order to address that identification problem, I employ an instrumental variable strategy using quasi-experimental variation in unemployment benefit durations.

Let PBD_{ts} denote the maximum potential benefit duration in state s at time t. That measure includes both the baseline plus the extension benefit duration, and hence varies from 26 up to 99 weeks. The variation in the benefit durations stems from quasi-experimental variation in the timing and the magnitude of the benefit extensions within and across states. Using notation corresponding to the fixed effects model, the reduced form of the model with respect to the instrument can be written as:

$$\begin{aligned}\ln(\text{CR})_{tsag} = \delta_0 + \delta_1 \text{PBD}_{ts} + \delta_2 \ln(\text{UR}_{t-1,sag}) + \delta_3 X_{tsag} \\ + \sum_{i=s,g,a} \alpha_i + f(t_s) + \varepsilon_{tsag}\end{aligned} \tag{4.2}$$

The reduced form estimates the direct link between the potential benefit duration, varying over space and time with the extensions, and the crime rate. Two competing mechanisms connect benefit extensions and criminal activity. On the one hand, it has been argued in the literature that benefit extensions decrease unemployment exit probabilities. Think about two otherwise identical unemployed individuals, one of whom receives unemployment benefits for longer than the other person. At the margin the person who receives the benefits is disincentivised, remains unemployed and becomes more likely to commit crime than the re-employed counterfactual. On the other hand, compared to an otherwise identical unemployed individual who has expired unemployment benefits, that person is better off in financial terms: At the margin, the unemployed who receives benefits is less likely to

[17] In general, there might be concerns over and above these reflections. First, individuals might choose to participate in crime based on unobservable characteristics which correlate with equally unobservable characteristics determining unemployment. If the unobservable characteristics are positively correlated with unemployment and are also positively correlated with participation in criminal activity, the fixed effects model is upwards biased. Second, firms choose locations. If crime rates in a local labour market are high, firms may not enter that market but choose different local labour markets with lower crime rates. In that case, the existing crime rate impacts on the current and future local labour market conditions, again leading to a reversed causality problem. Yet, these concerns are less applicable to the context of this study given the type of data I use.

commit crime than the counterfactual without benefits, everything else being equal. That is directly linked to the income mechanism outlined in section 4.3: The benefit extension delays the negative income shock caused by benefit exhaustion, and thus increases returns to legal activity compared to the counterfactual situation.[18] The reduced form link between crime and unemployment benefit extensions is the sum of both effects: If $\delta_1 > 0$, the disincentive effect exceeds the income effect. If $\delta_1 < 0$, the income effect exceeds the disincentive effect. Ex ante, the sign of the overall effect is uncertain and remains to be determined empirically.

The first stage of the initial model links the unemployment rate for period t, state s, age group a and gender g to the maximum potential benefit duration in state s at time t:

$$\begin{aligned}\ln(\mathrm{UR})_{tsag} = \gamma_0 + \gamma_1 \mathrm{PBD}_{ts} + \gamma_2 \ln(\mathrm{UR}_{t-1,sag}) + \gamma_3 X_{tsag} \\ + \sum_{i=s,g,a} \alpha_i + f(t_s) + \varepsilon_{tsag}\end{aligned} \tag{4.3}$$

As described in more detail in section 4.4, the benefit extension trigger mechanisms are based on statewide total and insured unemployment rates: If there is a large inflow into unemployment, unemployment rates increase and eventually exceed pre-defined thresholds triggering extension policies. *Conditional* on the previous unemployment situation, one can argue that the policy is then as good as randomly assigned. For example, consider a state with an unemployment rate of 4.99% in one month and an unemployment rate of 5.01% in another month. The overall economic conditions in these two months are likely to be comparable. However, if the benefit extension trigger was based on an unemployment rate of 5%, individuals would face different potential benefit spells.

Moreover, benefit extensions are implemented at the state level. The validity of the instrument is supported by local labour markets within a state being differently affected by economic conditions: The individual labour market experience is in general independent from the policy which averages over all local labour markets within that state. Unfortunately, the type of data I have does not allow me to allocate individuals to local labour markets other than at the state level. Yet, a similar, but due to autocorrelations somewhat weaker argument can be made with respect to age and gender specific unemployment rates.

As outlined before, benefit extensions decrease unemployment exit probabilities.

[18] These reflections directly relate to the debate in the literature about the moral hazard losses of unemployment benefits on the one hand and the insurance gains on the other hand.

While the benefit extensions are enacted as a response to an increasing inflow into unemployment, that implies that the relative outflow from unemployment decreases and the unemployment rate increases as a consequence. Indeed, that is what can be observed in the data as discussed above and illustrated in figure 4.13. In order to incorporate these arguments into the empirical analysis, I thus condition on the lagged unemployment rate. The two-stage least squares model can according to the above be written as:

$$\begin{aligned}
\ln(\mathrm{CR})_{tsag} &= \beta_0^* + \beta_1^* \ln(\mathrm{UR}_{tsag}) + \beta_2^* \ln(UR_{t-1,sag}) + \beta_3^* X_{tsag} \\
&\quad + \sum_{i=s,g,a} \alpha_i + f(t_s) + \varepsilon_{tsag} \\
\ln(\mathrm{UR})_{tsag} &= \gamma_0 + \gamma_1 \mathrm{PBD}_{ts} + \gamma_2 \ln(\mathrm{UR}_{t-1,sag}) + \gamma_3 X_{tsag} \\
&\quad + \sum_{i=s,g,a} \alpha_i + f(t_s) + \varepsilon_{tsag} \qquad (4.4)
\end{aligned}$$

In order to be a valid instrument, potential benefit durations have to be exogenous with respect to crime rates. If potential benefit durations and crime both directly correlated with recessions, the exogeneity assumption would be violated and one would expect a recession bias. Cook and Zarkin (1985) discuss four possible linkages between crime and business cycles: Legitimate opportunities, criminal opportunities, use of criminogenic commodities and the criminal justice system response to crime. The empirical model captures the macro-elasticity of crime supply with respect to unemployment conditional on the lagged unemployment rate, and thus captures the crime response to a change in legitimate opportunities. Criminal opportunities may increase during recessions if the government decreases spending on the criminal justice system. In the empirical analysis, this can be taken into account by controlling for the number of law enforcement employees in order to rule out related confounding factors. If the identifying assumptions hold, β_1^* is the elasticity of the crime rate with respect to unemployment.

4.6 Unemployment and Crime

The empirical results corresponding to the estimating equations (4.1) and (4.4) are shown in table 4.4 for property crime, table 4.5 for violent crime and table 4.6 for drug crime. Columns (1) and (2), respectively, report the results for the OLS estimation with and without control variables, columns (3) and (4) for the 2SLS estimation. Column (4) is the preferred specification for the reasons discussed in the previous sections.

The OLS estimations yield mixed results across the three different crime types. Whilst for property as well as for violent crime the coefficients are close to zero and imprecisely estimated, a higher unemployment rate is associated with significantly more arrests for drug crime. Yet, these associations do not have a causal interpretation due to potential biases as outlined above. Indeed, confirming the arguments listed in section 4.5 the results reveal a substantial downward bias of the OLS compared to the 2SLS estimations.

For property crime, the 2SLS estimation yields a 0.15 elasticity of the arrest rate with respect to the current unemployment rate which is statistically significant at the 1% level. In order to interpret the magnitude of the elasticity, imagine a fictional state with a property crime arrest rate of 100 per 100,000 population and an unemployment rate of 10%. Assume that during a recession the unemployment rate increases by 5 percentage points to 15%. The results suggest that such an increase is linked to an increase in the property crime arrest rate by 7.5 arrests per 100,000 population. The results are in line with the arguments above: Higher unemployment and even more so an increase in unemployment at a given level of unemployment is linked to an increase in property crime. The corresponding elasticities for violent crime and drug crime are not significantly different from zero.

In the exactly identified case, the two-stage least square estimator is equivalent to the indirect least square estimator. Hence, it equals the ratio of the reduced form coefficient on the instrument to the first stage coefficient:

$$\beta_1^* = \frac{\delta_1}{\gamma_1} \tag{4.5}$$

In order to better understand the mechanisms behind the impact of unemployment on crime, I thus discuss the reduced form and the first stage results in more detail in what follows.

4.6.1 Reduced Form: Unemployment Benefits and Crime

The reduced form estimates the link between the potential benefit duration and the crime rate as described in equation (4.2). In that model, δ_1 can be interpreted as the semi-elasticity of the crime rate with respect to the potential benefit duration. Figure 4.15 illustrates the reduced form effect of the instrument on property crime rate, figure 4.16 the effect on the violent crime rate and figure 4.17 on the drug crime rate, respectively. Figure 4.15 suggests that the property crime rate partially correlates with the benefit duration. For violent crime and drug crime, that link is not evident from visual inspection.

Panel A in table 4.7 shows the corresponding estimation results. Column (1) displays the results for property crime, column (2) for violent crime and column (3) for drug crime. For ease of interpretation, potential benefit duration is measured in months rather than in weeks for all reduced form specifications.[19] The results suggest that a one month increase in the potential benefit duration increases the property crime arrest rate by 0.34% and has no significant impact on the violent or drug crime arrest rate. That corresponds to an elasticity at the mean of 0.04. Using the fictional example from above, the results translate into an increase in the property crime arrest rate by 1.02 arrests per 100,000 population if the potential benefit duration is extended by 12 weeks.

As discussed above, the overall reduced form effect is the sum of competing disincentive and income effects. Finding that $\delta_1 > 0$ suggests that the disincentive effect outweighs the income effect: At the margin, individuals are disincentivised by longer benefit durations with respect to job search, remain unemployed and become more likely to commit property crime than they would have been otherwise.

A Placebo Test

The reduced form estimation suggests that the benefit extensions are associated with incentives for criminal behaviour. That is surprising at first, and one might be concerned that the effect of the unemployment benefits on crime is confounded with underlying and unobserved factors which correlate with the recession period. Hence, a placebo analysis is conducted in order to assess the validity of the findings. In particular, I construct a placebo test by matching the benefit extensions during the Great Recession to an earlier, and in terms of unemployment rates comparable recession at the beginning of the 1980s, where the matching is based on months into the recession. That allows me to estimate the effect of "fake" benefit extensions during a recession that is similar to the Great Recession but without such extension policies taking place at the time. The estimating equation for the placebo test can then be written as follows, where ER designates the earlier recession in the 1980s and GR the Great Recession:

$$\ln(\text{CR})^{ER}_{tsag} = \delta^*_0 + \delta^*_1 \text{PBD}^{GR}_{ts} + \sum_{i=s,g,a} \alpha_i + f(t_s) + \varepsilon_{tsag} \tag{4.6}$$

[19]A month is defined as four weeks in that specification.

If the reduced form effect of the benefit extensions on crime, δ_1 was driven by underlying factors related to the business cycle during the Great Recession, one would expect the "fake" benefit extension to have a similar impact δ_1^* on crime during the 1980s recession, too.

Panel B and C in table 4.7 show the results for the placebo analysis. Panel B shows the results when I restrict the reduced form analysis to the Great Recession period only, whereas panel C shows the results for the actual placebo analysis as detailed above. For property crime, the Great Recession sample coefficient on the instrument amounts to 0.23% and hence is smaller than the full sample coefficient, but within the standard error. The placebo coefficient is not significantly different from zero and not within the standard error of the Great Recession sample coefficient. For both violent and drug crime the estimated coefficients are again not significantly different from zero, neither for the reduced form estimation nor for the placebo analysis.

The findings from the placebo analysis support the hypothesis that there is indeed a reduced form effect of the potential unemployment benefit duration on the property crime rate. That result is striking in itself and demonstrates a disturbing and unintended effect of extended unemployment benefit periods.

4.6.2 First Stage: Unemployment

The first stage of the model estimates the link between the potential benefit duration and unemployment as described in equation (4.3), and helps to understand the mechanisms behind the findings above. Here, γ_1 is the semi-elasticity of the unemployment rate with respect to the potential benefit duration. To reiterate, the variation in the potential benefit duration stems from the quasi-experimental variation in the timing and the magnitude of the benefit extensions within and across states. Increases in the state unemployment rate trigger the benefit extension, while the trigger thresholds as well as the magnitude of the extension are determined by federal law. Changes in federal legislation lead to non-smooth changes in potential benefit durations over time.

Figure 4.18 illustrates the first stage mechanism: An inflow into unemployment increases the unemployment rate. If the unemployment rate exceeds the pre-defined trigger threshold, the unemployment benefit period in that state is extended based on federal legislation. Yet, the benefit extensions decrease unemployment exit probabilities and relative outflows from unemployment. That is reflected by an increase in the average unemployment duration, and a subsequent increase in the unemployment rates. Conditional on the previous unemployment rate, the current unemployment rate thus depends - inter alia - on the benefit duration.

Table 4.8 shows the results which correspond to the first stage estimating equation (4.3). Columns (1) and (2) report the results for the first stage estimation with respect to the current unemployment rate with and without control variables. For ease of interpretation, the potential benefit duration is measured in months rather than in weeks for this specification.[20] The results suggest that a one month increase in the potential benefit duration increases the unemployment rate by 2.4%. In the fictional example above with an unemployment rate of 10% that translates into a 0.72 percentage point increase in the unemployment rate if the benefit duration is increased by 12 weeks.

The evidence in this paper so far has been based on unemployment rates as a measure of labour market conditions. Yet, the U.S. labour market has experienced unprecedented changes in terms of unemployment durations. As discussed above, unemployment durations have dramatically increased during and after the most recent recession leading to ongoing policy debates on how to reduce long-term unemployment. Following the same reasoning as above, disincentive effects of the extended unemployment benefit periods can partially explain these patterns. Table 4.8 shows the results for the first stage estimation with respect to the average unemployment duration instead of the current unemployment rate, as reported in columns (3) and (4).[21] The results suggest that a one week increase in the potential benefit duration increases the average unemployment duration by 0.76%. If for example the average unemployment duration is 20 weeks and the potential benefit duration is extended by 12 weeks, that translates into a 1.8 week increase in the *average* unemployment duration.

4.7 Unemployment Duration and Crime

The observations and arguments made above naturally lead to the question how the increase in unemployment durations in the U.S. labour market is linked to crime, or in other words to what extend the link between unemployment and crime is duration dependent. The empirical model follows the strategy described in section 4.5 and exploits the given quasi-experimental variation in unemployment and unemployment benefit durations. Let UD_{tsag} be the average unemployment duration for period t, state s, age group a and gender g, measured in weeks. The empirical model

[20] A month is defined as four weeks in that specification.

[21] The corresponding estimation equation accordingly reads: $\ln(\mathrm{UD})_{tsag} = c_0 + c_1\mathrm{PBD}_{ts} + c_2 \ln(\mathrm{UR}_{t-1,sag}) + c_3 X_{tsag} + \sum_{i=s,g,a} \alpha_i + f(t_s) + \varepsilon_{tsag}$.

can then be written as follows:[22]

$$
\begin{aligned}
\ln(\mathrm{CR})_{tsag} &= b_0 + b_1 \ln(\mathrm{UD}_{tsag}) + b_2 \ln(UR_{tsag}) + b_3 X_{tsag} \\
&\quad + \sum_{i=s,g,a} \alpha_i + f(t_s) + \varepsilon_{tsag} \\
\ln(\mathrm{UD})_{tsag} &= c_0 + c_1 \mathrm{PBD}_{ts} + c_2 \ln(\mathrm{UR}_{tsag}) + c_3 X_{tsag} \\
&\quad + \sum_{i=s,g,a} \alpha_i + f(t_s) + \varepsilon_{tsag}
\end{aligned} \tag{4.7}
$$

The empirical results for model (4.7) are shown in table 4.9 for property crime, table 4.10 for violent crime and table 4.11 for drug crime. Columns (1) and (2) report the results for the OLS estimation with and without control variables, columns (3) and (4) for the 2SLS estimation respectively. Column (4) is the preferred specification. Each table shows a specification using the average unemployment duration (Panel A), an interaction of that average unemployment duration with the unemployment rate (Panel B) and the median unemployment duration (Panel C).

For property crime, the 2SLS estimation yields a 0.114 elasticity of the arrest rate with respect to the average unemployment duration for the preferred specification. The coefficient is statistically significant at the 1% level. Again, imagine a fictional state with a property crime arrest rate of 100 per 100,000 population and this time with an average unemployment duration of 20 weeks. Assume that during a recession the average unemployment duration increases by 10 weeks to 30 weeks. The results suggest that such an increase is linked to an increase in the property crime rate by 5.7 arrests per 100,000 population. This is the *average* effect: The effect for more vulnerable population groups may be even larger.

When interacted with the unemployment rate, the elasticities are positive and statistically significant at the 1% level albeit smaller than in the initial specification. Similarly, the elasticity of the property crime arrest rate with respect to the median unemployment duration is smaller than the elasticity with respect to the mean unemployment duration. This can be explained by the right skewed distribution of unemployment durations: The median unemployment duration gives less weight to longer unemployment durations than the average unemployment duration, hence the smaller elasticity. Qualitatively, all results are in line with the arguments above: Longer unemployment durations are linked to higher property crime propensities. The corresponding elasticities for violent crime and drug crime are not significantly

[22] Note that in this model, one can explicitly control for the unemployment rate in order to avoid a policy bias in the estimated coefficients.

different from zero.

Compared to the estimation results of the OLS model, the magnitudes of the coefficients are substantially higher in the 2SLS estimation. The direction of the bias is consistent with the arguments made before as well as with the direction of the bias found in the literature on unemployment and crime. In terms of the magnitude of the bias, it is likely that the instrument captures a local effect: The instrument particularly affects individuals with longer unemployment spells which may in return lead to substantially higher results than found in the OLS model.

Robustness Checks and Further Results

In the following, I present the results of robustness checks and further analyses. As mentioned in the discussion of the instrument validity, one might be concerned about changes in the law enforcement system that confound the effect of unemployment and unemployment duration on crime. Table 4.12 shows the results of the 2SLS estimation for property, violent and drug crime when an additional control variable for the ratio of police employees per population per state and year is included in the regressions. The elasticities of the property crime rate both with respect to the unemployment rate (panel A) and the average unemployment duration (panel B) are robust to that specification. The corresponding elasticities for violent crime and drug crime again are not significantly different from zero.

Arguably, the ratio of police employees per population is an endogenous control variable. Hence, column (4) in panel C of table 4.12 shows the results of a regression of the police employment on the potential benefit duration. If local governments did respond to the policy with changes in the law enforcement system, one would expect to find a non-zero coefficient in that specification. However, the results show that the effect is close to zero and not precisely estimated.

The overall reduced form effect of the instrument on criminality is the sum of competing disincentive and income effects. In order to disentangle these two effects, one can include income control variables in the regression specification. The income information including income from unemployment benefits is available only for the CPS March sample which leads to a substantial reduction in sample size and quasi-experimental variation. Table 4.13 shows the estimation results for property, violent and drug crime excluding and including the income control variables, respectively. Column (1) shows the elasticities of the property crime rate with respect to the unemployment rate (panel A) and the average unemployment duration (panel B) when the income control variables are excluded, column (2) when the income control variables are included. In general, the elasticities on the reduced sample are larger than the elasticities on the full sample due to sampling bias. Still, the results illustrate that the elasticities of arrest rates with respect to the unemployment variables are larger once the income effect of the benefit extensions is taken into account.

So far, the elasticities of crime rates with respect to unemployment duration refer to the *average* effect. In order to better understand the duration dependence, one is interested in the effect of unemployment duration on crime at different points along the duration distribution. Table 4.14 shows the results when the sample is split into subsamples depending on whether the average unemployment duration is shorter than the baseline benefit duration, longer than the baseline benefit duration but shorter than the extended benefit duration, or longer than the extended benefit duration. For property crime, the results suggest positive elasticities for the first subsample while they are not significantly different from zero for the other two categories. The first group comprises individuals who are at the margin of exhausting regular benefits and hence are the most likely to be affected. Yet, sample sizes decrease drastically for the last two categories and hence one has to be cautious with regards to a stronger interpretation.

Table 4.15 presents the estimated elasticities when the sample is split by quintile of the unemployment duration distribution instead. The lowest quintile ranges from 0 to about 7 weeks of average unemployment duration, the middle quintile from 12 to about 18 weeks, and the highest quintile includes average unemployment durations of 28 weeks and more. While the elasticities for violent crime and drug crime are not significantly different from zero for any of the quintile subsamples with the exception of violent crime in the fourth quintile, the elasticity of the property crime rate with respect to the average unemployment duration amounts to 2.9% for the middle quintile and only 0.46% for the lowest quintile. For the fourth quintile, the effect decreases but is still significantly positive while for the highest quintile it is not significantly different from zero and fades out. The results suggest that there is an initial effect of unemployment on crime that is persistent for longer unemployment durations, but fades away with very long-term unemployment.

Table 4.16 shows the results by type of unemployment. The average unemployment duration here is the average unemployment duration by type of unemployment: Job losers, job leavers, re-entrants and new entrants. The elasticities of the property crime rate with respect to the average unemployment duration are positive and statistically significant for all types of unemployment. Yet, given the type of data it is not possible to identify which group commits crimes. Job losers are directly affected by the instrument, they fulfill at least one of the eligibility conditions to receive extended unemployment compensation. The overall impact of unemployment duration on crime is hence affected by the prolonged income compensation for that group. In terms of the other groups, elasticities are even higher which can be explained with externalities of the longer benefit duration: Higher unemployment rates and longer unemployment durations increase the competition for jobs. That lowers the relative returns to legal activity, and at the margin pushes individuals into the crime market instead.

The empirical literature on crime typically looks at gender specific samples. Table 4.17 shows the estimation results for different gender specifications with respect to property crime rates. Columns (1) and (2) in panel A show the results for separate male and female estimations. The estimated elasticities for the male sample are estimated imprecisely, and are not significantly different from zero. For females, the property crime elasticity is positive and statistically significant. These results might be surprising at first, but can be explained by a decreasing gender gap in property crime rates as shown in figure 4.19. Columns (3) and (4) show the estimated elasticities for the male and female sample separately for the property crime types burglary and larceny. The positive effect for the female sample is driven by a strong and positive effect of unemployment durations on larceny arrest rates, while the respective effect is positive but smaller for the male sample. Columns (1) and (2) in panel B show the results for the elasticity of the male property crime rate with respect to the average female unemployment duration and vice versa. Interestingly, the results suggest strong effects for female property crime rates with respect to male unemployment durations, suggesting the presence of cross-gender effects.

Typically, crime rates for younger age groups are higher than for older age groups, in particular for property and drug crime. Table 4.18 shows the results by the age group (16-19, 20-24, 25-29, 30-34 and 35-39). The elasticities with respect to property crime are positive and statistically significant for the younger age groups and robust between the age groups 16-19, 20-24 and 25-29. Further, for the 30 to 34 year-old group, the elasticity is much smaller and for the oldest group in the sample it is not statistically different from zero anymore. These results are consistent with typical crime demographics.

4.8 Conclusion

In this chapter, I study the relationship between labour market conditions and crime in the context of temporary unemployment benefit extensions and increasing unemployment durations in the United States. The identification of the crime elasticities with respect to unemployment rates and durations is based on variation in the timing and magnitude of unemployment benefit extensions within and across states. Exploiting that quasi-experimental variation in unemployment and unemployment benefit durations, I provide new evidence on the causal effect of unemployment on crime. In line with previous findings, I find that higher unemployment is linked to higher criminality. Moreover, I find that the effect is driven by unintended effects of the benefit extensions on crime and underlying increases in unemployment rates and durations. Linking these results to recent structural changes in the U.S. labour market, I find that the link between unemployment and crime is duration dependent. The main results are consistent with expectations from theoretical models on labour markets and crime.

There are two main limitations to the analysis. The first concerns the external validity of the quasi-experiment and is thus common to the literature using a similar methodology. The increases in unemployment durations in the United States, unlike in European countries, have been unprecedented and one might argue particular to the Great Recession. Here, I identify the impact of unemployment on crime from these changes and thus the analysis is likely to pick up a local effect. The second limitation refers to concerns about compositional changes in unemployment. It has been argued that the unemployment benefit extensions have led to reduced exits from the labour force. That means that the composition of the unemployed population changes with the policy compared to the pre-policy period. The implications for this analysis are not obvious ex-ante, and unfortunately the type of data which I have access to does not allow me to analyse that concern in more detail.

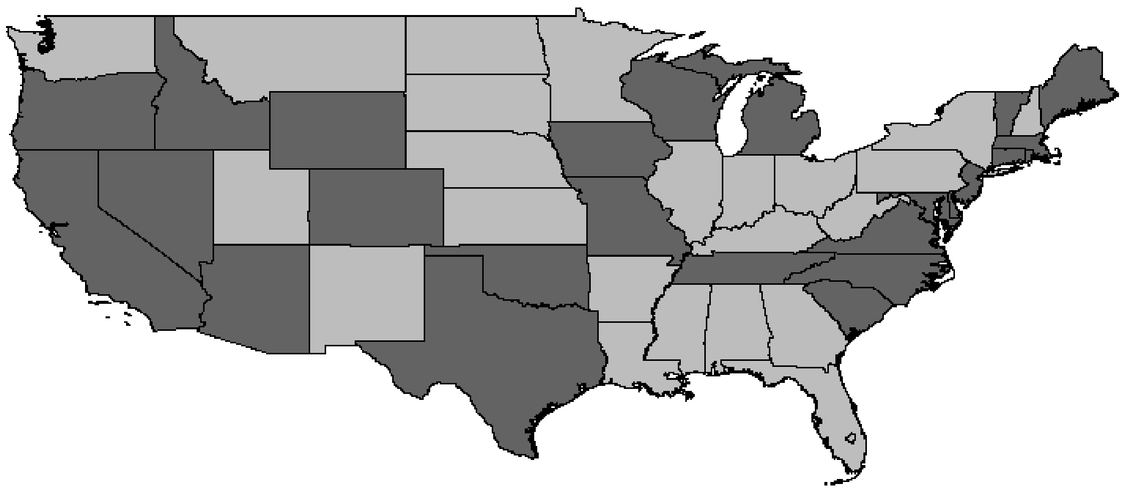
Overall, the chapter adds to the literature on labour markets and crime, and yields new insights into the causal relationship between unemployment and crime which can be important for well-targeted policy decisions. Moreover, the results suggest that there are unintended effects of benefit extensions on property crime rates. In terms of welfare considerations, it would be extremely interesting, but beyond the scope of this study, to conduct a cost-benefit analysis of the benefit extensions taking these effects into account.

4.9 Figures

Figure 4.1: Victimisation versus Arrest Counts

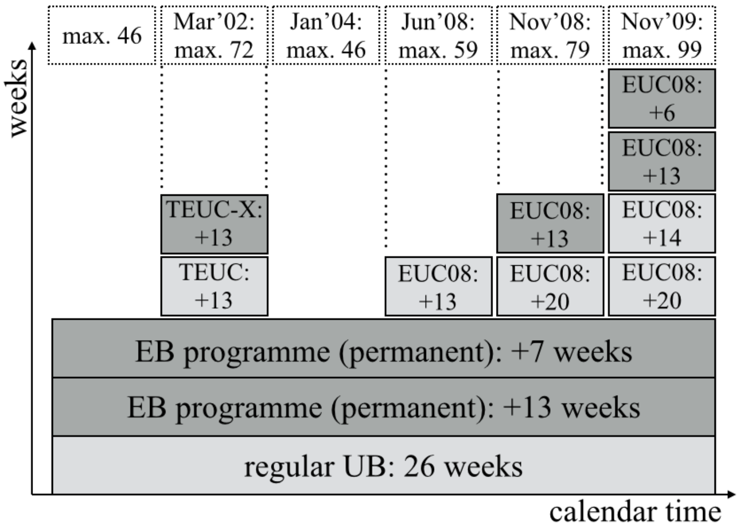
Note: The figure shows the number of victims versus arrests per 1,000 U.S. population for property and violent crime. *Source*: Bureau of Justice Statistics (BJS), Data Analysis Tools, 1993-2011 and own calculations.

Figure 4.2: Sample Geography



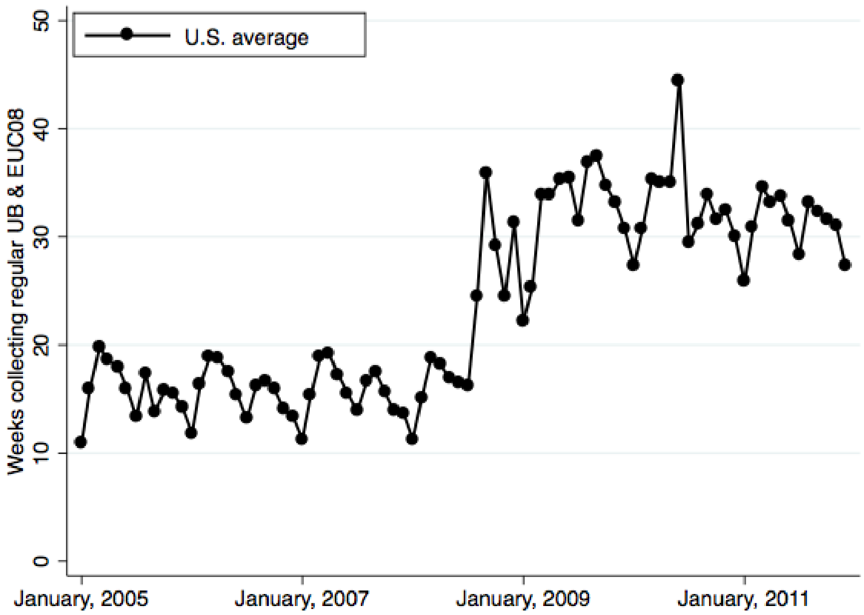
Note: The map shows the federal states of the United States without Alaska (in the sample) or Hawaii (out of the sample). The dark shaded areas represent states which are included in the sample, the light shaded areas show states which are excluded from the sample. See the data description in section 4.4 for details. *Source:* UCR and own calculations.

Figure 4.3: Potential Benefit Durations, Schematic Representation



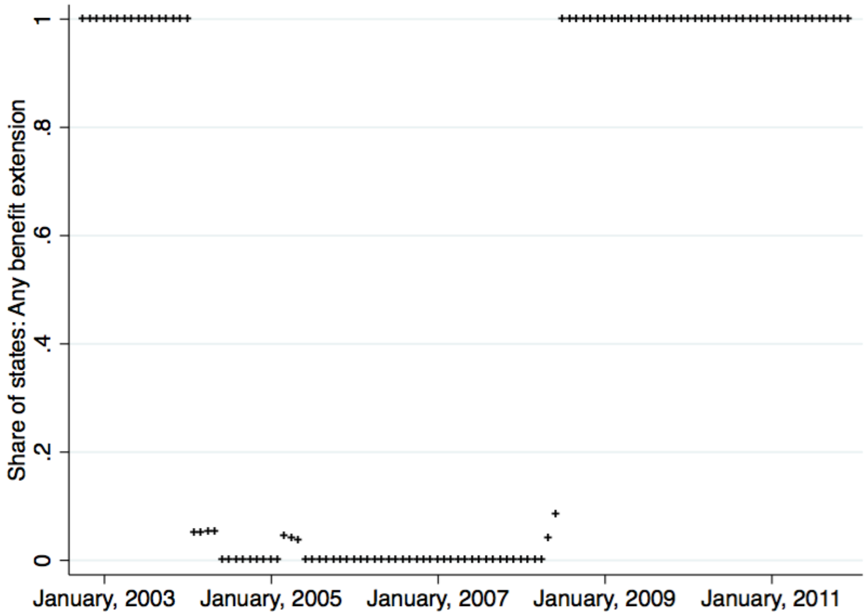
Note: The figure shows the a schematic representation of potential benefit durations in the U.S. between 2003 to 2011. The light shaded areas represent potential benefit durations which are implemented in all states and do not depend on trigger mechanisms. The dark shaded areas represent potential benefit durations which are implemented only if a state triggers on the respective policy. *Source:* U.S. Department of Labor and own calculations.

Figure 4.4: Actual Benefit Durations, U.S. Average



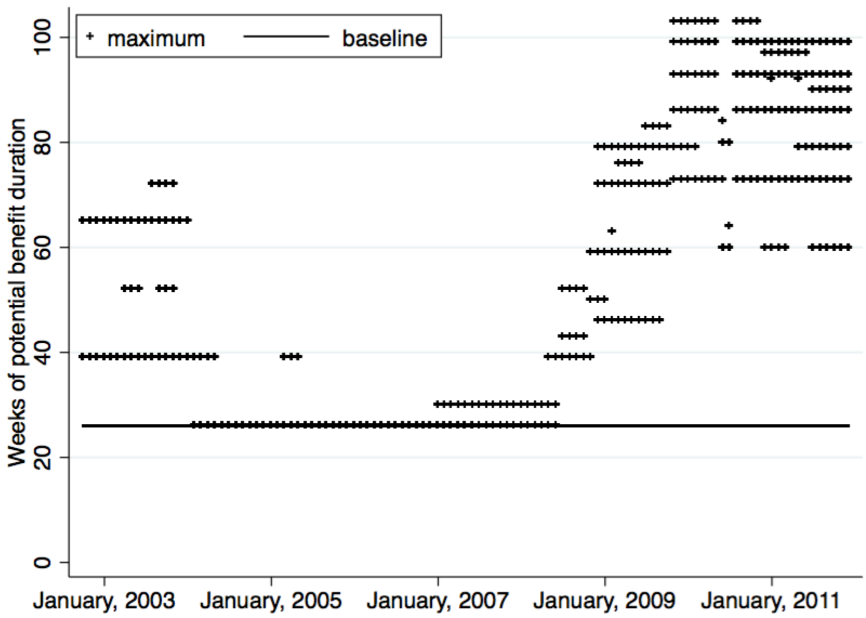
Note: The figure shows the monthly variation in the U.S. average duration of persons collecting unemployment insurance benefits between January 2005 and December 2011, where unemployment insurance here includes regular unemployment benefits as well as EUC08 benefits. *Source:* US Department of Labor and own calculations.

Figure 4.5: Variation in Share of States with Benefit Extension



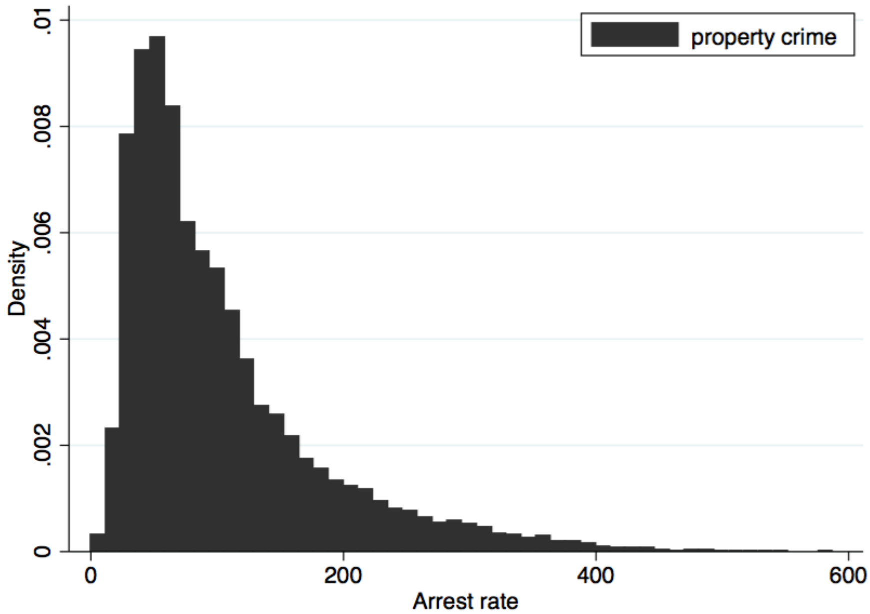
Note: The figure shows the variation in the share of states in the sample which have triggered onto *any* kind of benefit extension in the U.S. between 2003 and 2011. *Source:* U.S. Department of Labor and own calculations.

Figure 4.6: Variation in Potential Benefit Durations



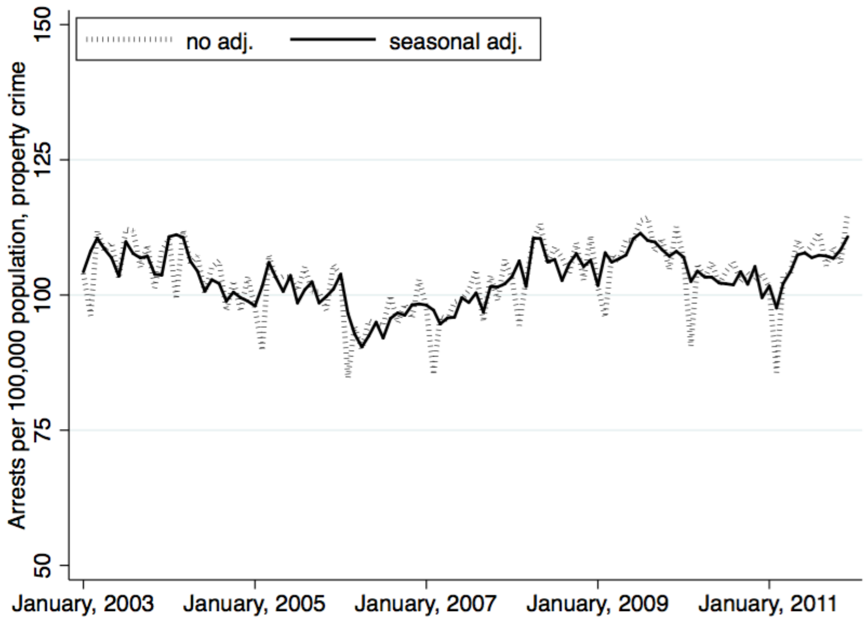
Note: The figure shows the variation in the maximum potential benefit duration for the U.S. between 2003 and 2011 across states in the sample. The horizontal line represents the baseline potential benefit duration. *Source:* U.S. Department of Labor and own calculations.

Figure 4.7: Arrest Rate Distribution, Property Crime



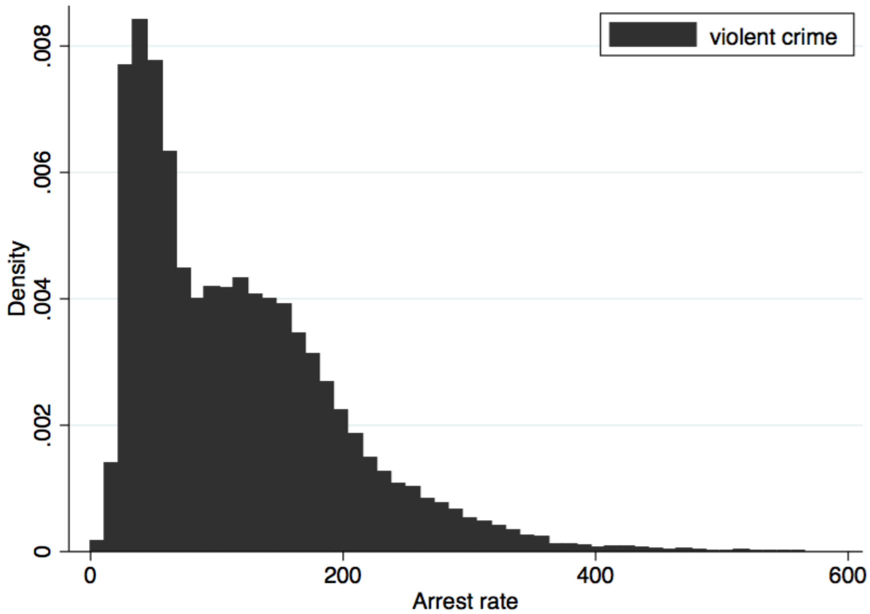
Note: The figure shows a histogram of the arrest rate for property crime per 100,000 population in the U.S. between 2003 and 2011. *Source*: UCR and own calculations.

Figure 4.8: Arrest Rate Trend, Property Crime



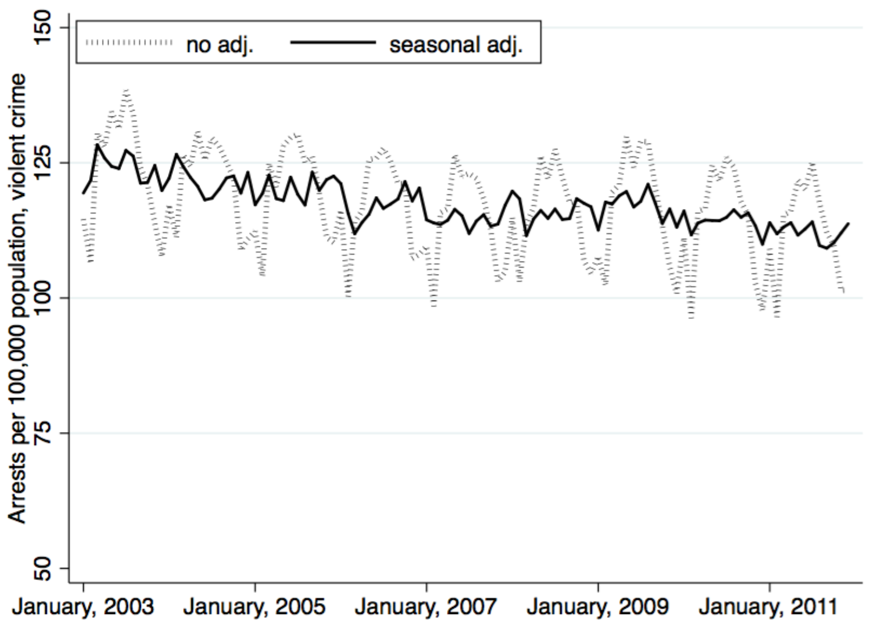
Note: The figure shows the trend of the average arrest rate for property crime per 100,000 population in the U.S. between 2003 and 2011. *Source*: UCR and own calculations.

Figure 4.9: Arrest Rate Distribution, Violent Crime



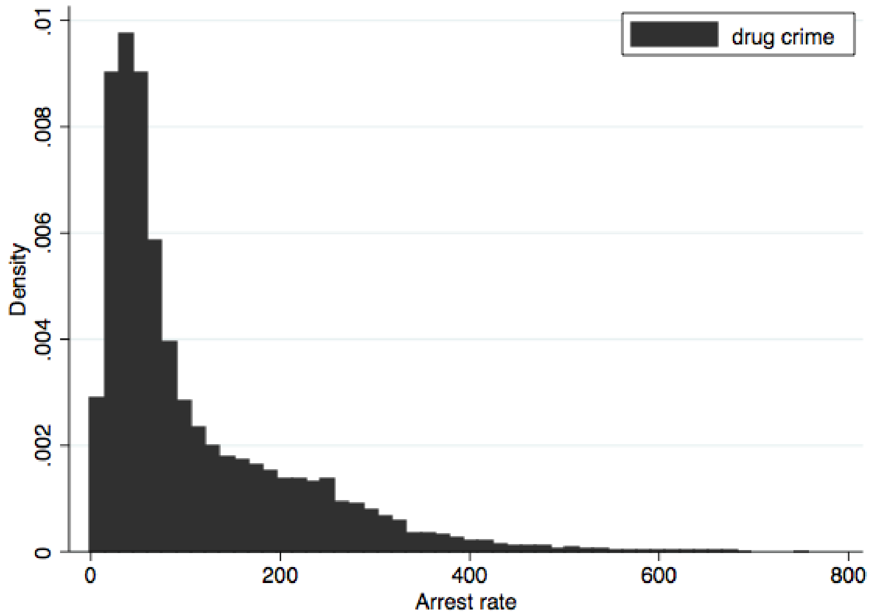
Note: The figure shows a histogram of the arrest rate for violent crime per 100,000 population in the U.S. between 2003 and 2011. *Source*: UCR and own calculations.

Figure 4.10: Arrest Rate Trend, Violent Crime



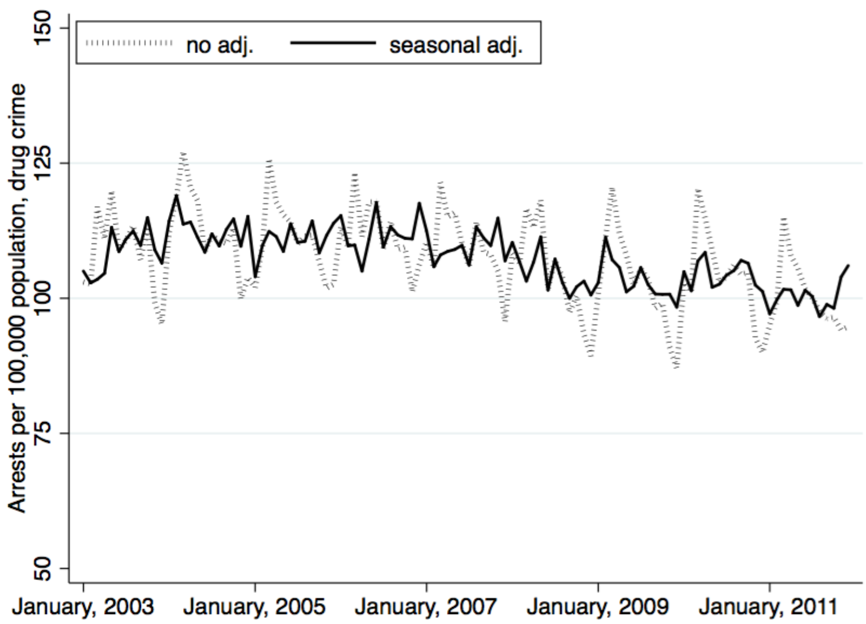
Note: The figure shows the trend of the average arrest rate for violent crime per 100,000 population in the U.S. between 2003 and 2011. *Source*: UCR and own calculations.

Figure 4.11: Arrest Rate Distribution, Drug Crime



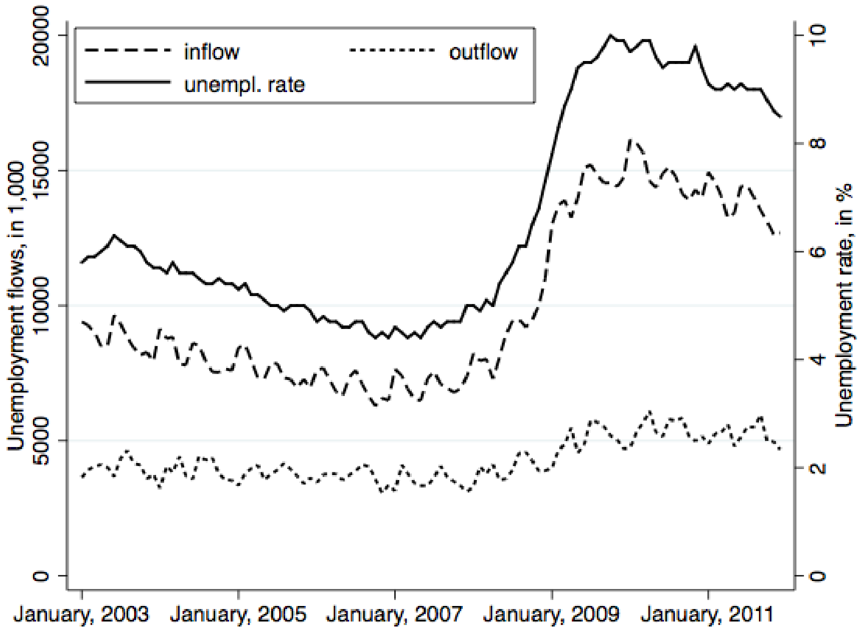
Note: The figure shows a histogram of the arrest rate for drug crime per 100,000 population in the U.S. between 2003 and 2011. *Source*: UCR and own calculations.

Figure 4.12: Arrest Rate Trend, Drug Crime



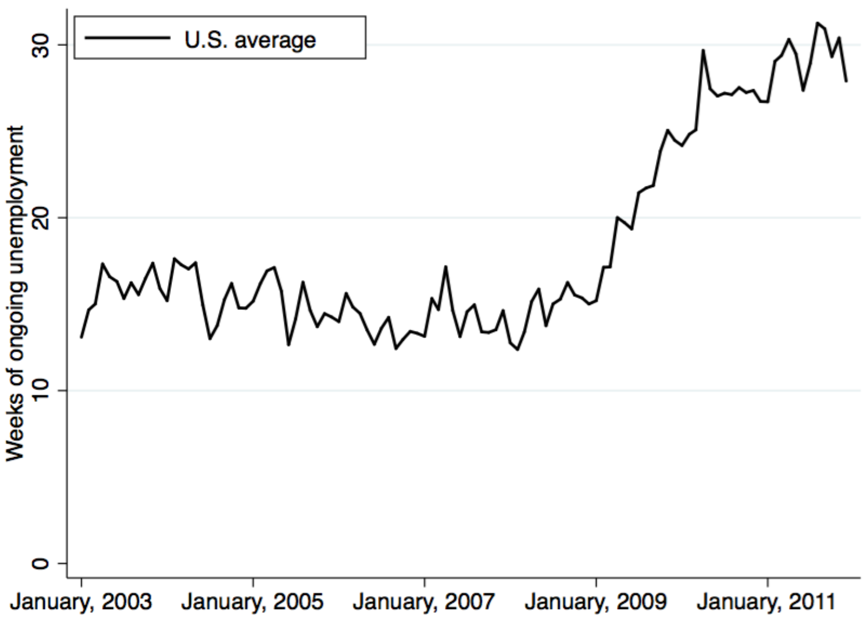
Note: The figure shows the trend of the average arrest rate for drug crime per 100,000 population in the U.S. between 2003 and 2011. *Source*: UCR and own calculations.

Figure 4.13: Unemployment Rate and Flows, U.S. Average



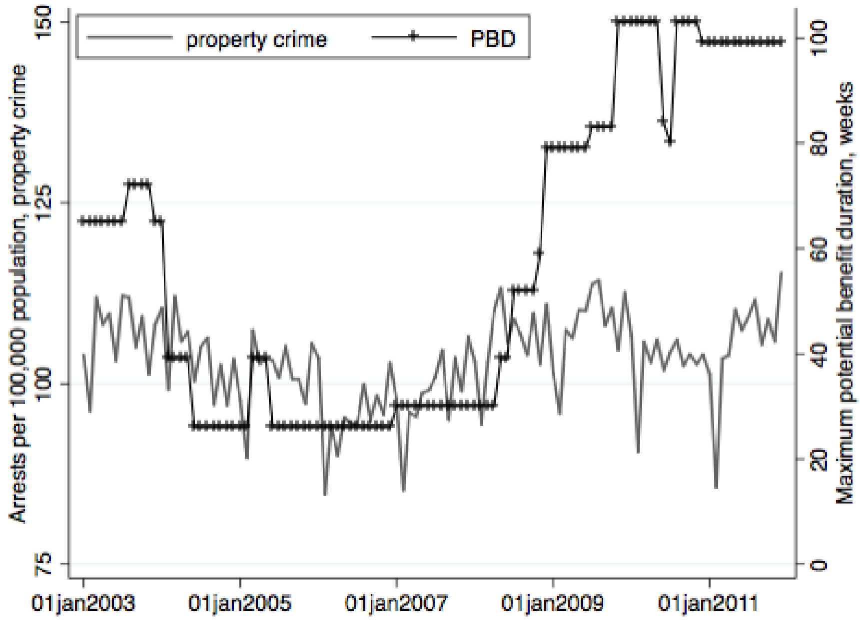
Note: The figure shows the average unemployment rate as well as the raw in- and outflows from unemployment in the U.S. between 2003 and 2011. *Source*: BLS and own calculations.

Figure 4.14: Unemployment Durations, U.S. Average



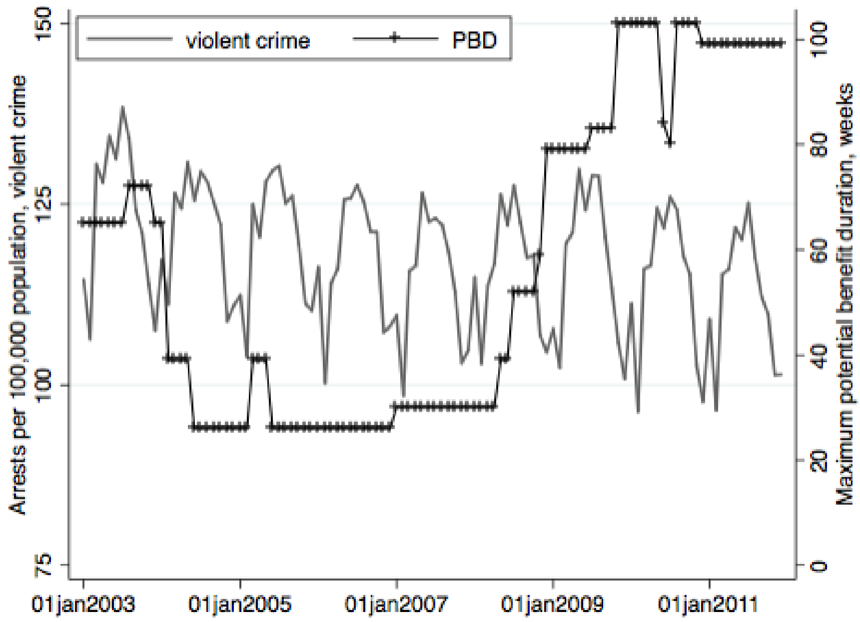
Note: The figure shows the average state unemployment duration in weeks in the U.S. fbetween 2003 and 2011. *Source*: CPS and own calculations.

Figure 4.15: Potential Benefit Duration and Property Crime



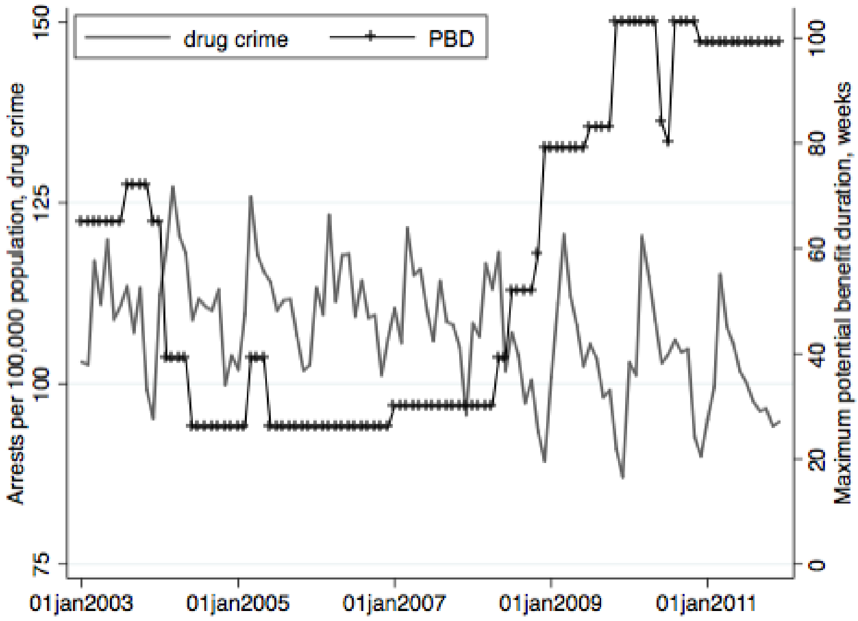
Note: The figure shows the average arrest rate for property crime per 100,000 population in the U.S. between 2003 and 2011, as well as the maximum potential benefit duration. *Source*: U.S. Department of Labor, UCR and own calculations.

Figure 4.16: Potential Benefit Duration and Violent Crime



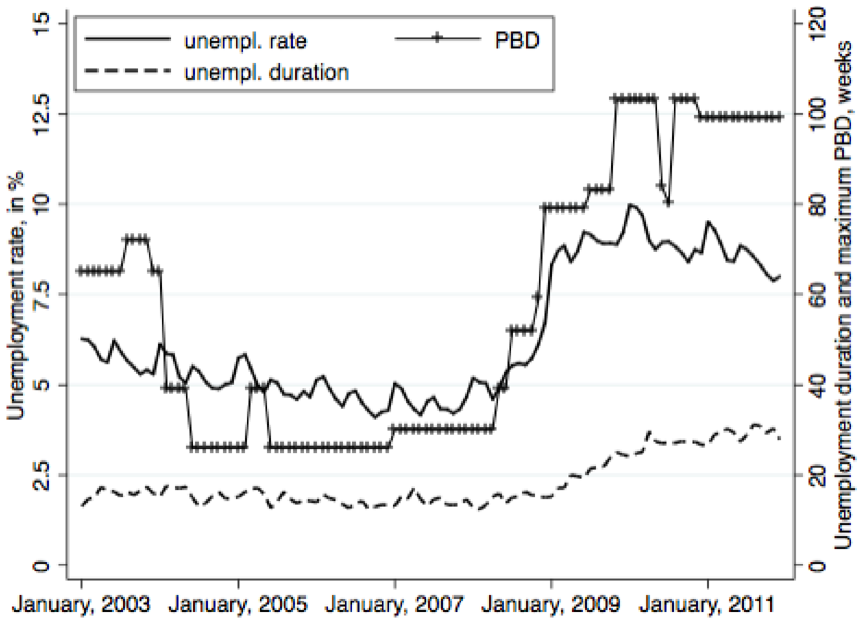
Note: The figure shows the average arrest rate for violent crime per 100,000 population in the U.S. between 2003 and 2011, as well as the maximum potential benefit duration. *Source*: U.S. Department of Labor, UCR and own calculations.

Figure 4.17: Potential Benefit Duration and Drug Crime



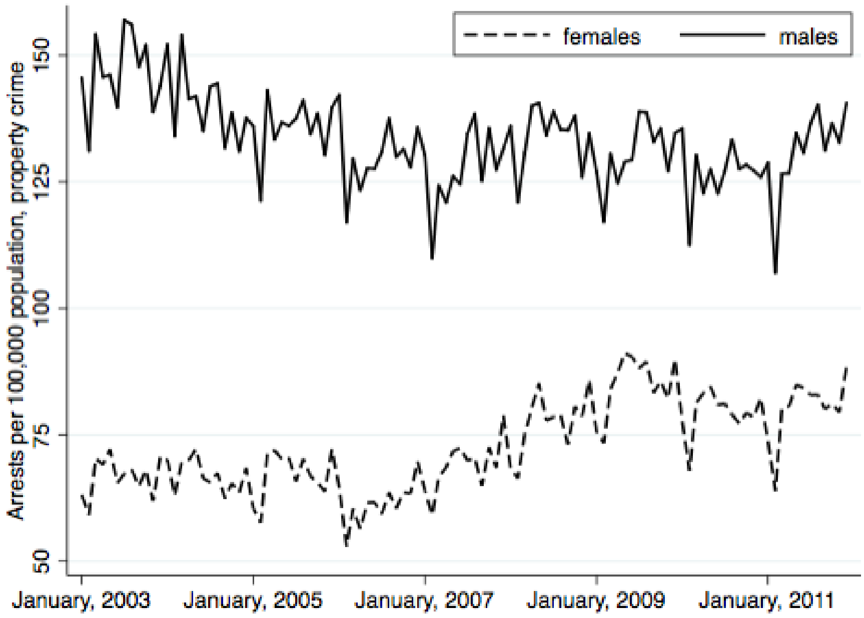
Note: The figure shows the average arrest rate for drug crime per 100,000 population in the U.S. between 2003 and 2011, as well as the maximum potential benefit duration. *Source*: U.S. Department of Labor, UCR and own calculations.

Figure 4.18: Potential Benefit Duration and Unemployment



Note: The figure shows the average state unemployment rate in percent and the maximum potential benefit duration as well as the average unemployment duration in weeks in the U.S. between 2003 and 2011.*Source*: U.S. Department of Labor, BLS, CPS and own calculations.

Figure 4.19: Arrest Rates by Gender, Property Crime



Note: The figure shows the trend of the average arrest rate for property crime per 100,000 population in the U.S. between 2003 and 2011 separately for women and men. *Source*: UCR and own calculations.

4.10 Tables

Table 4.1: Sample Geography

States in the sample	States out of the sample: Systematically missing data	States out of the sample: Less than 95% of the population covered by data
Arizona	Georgia	Alabama
California	Kentucky	Arkansas
Colorado	Minnesota	District of Columbia
Connecticut	New Mexico	Florida
Delaware	North Dakota	Illinois
Idaho	Utah	Indiana
Iowa		Kansas
Maine		Louisiana
Maryland		Mississippi
Massachusetts		Montana
Michigan		Nebraska
Missouri		New Hampshire
Nevada		New York
New Jersey		Ohio
North Carolina		Pennsylvania
Oklahoma		South Dakota
Oregon		Washington
Rhode Island		West Virginia
South Carolina		Hawaii
Tennessee		
Texas		
Vermont		
Virginia		
Wisconsin		
Wyoming		
Alaska		
= 26 states	= 6 states	= 19 states

Note: The first column lists the states that are included in the arrest data sample, the second column lists the states that are excluded from the sample due to systematically missing data and the third column lists the states that are excluded from the sample due to less than 95% of the data being covered by the data. For details, see the data description in section 4.4).

Table 4.2: Unemployment Benefit Extensions, U.S.

Programme	Time period	Extension period	Trigger mechanism
EB	permanent	mandatory 13 weeks	IUR $\geq$ 5% and IUR $\geq$ 120% of last year's IUR
		optional 13 weeks	IUR $\geq$ 6%
		optional 13 weeks	TUR $\geq$ 6.5% and TUR $\geq$ 110 % of either of 2 previous years'
		optional 20 weeks	TUR $\geq$ 8.0% and TUR $\geq$ 110 % of either of 2 previous years'
TEUC	Mar. 2002 - Jan. 2004	13 weeks	all states
TEUC-X		13 weeks	EB triggers
EUC08	June 2008 - Nov. 2008		
	Tier 1	13 weeks	all states
	Nov. 2008 - Nov. 2009		
	Tier 1	20 weeks	all states
	Tier 2	13 weeks	IUR $\geq$ 4% or TUR $\geq$ 6%
	Nov. 2009 - Feb. 2012		
	Tier 1	20 weeks	all states
	Tier 2	14 weeks	all states
	Tier 3	13 weeks	IUR $\geq$ 4% or TUR $\geq$ 6%
	Tier 4	6 weeks	IUR $\geq$ 6% or TUR $\geq$ 8.5%

Note: The table shows the different benefit extension policies during the sample period with the respective time period, extension period and trigger mechanisms. Abbreviations: Extended Benefits (EB), Temporary Extended Unemployment Compensation (TEUC), Emergency Unemployment Compensation 2008 (EUC08). *Source:* United States Department of Labor and Bureau of Labor Statistics.

Table 4.3: Descriptive Statistics, Estimation Sample

Variable	Observations	Mean	S.D.
Arrest rate, property crime	24,297	103.222	77.690
Arrest rate, violent crime	24,297	117.222	79.813
Arrest rate, drug crime	24,297	107.178	99.944
Unemployment duration	24,297	18.630	14.615
Potential benefit duration	24,297	48.972	27.605
Unemployment rate:			
- state-level (BLS)	24,297	6.432	2.359
- observational unit-level(CPS)	24,297	9.968	7.580
Share of married individuals	24,297	0.371	0.265
Share of native individuals	24,297	0.860	0.103
Share of high-school graduates	24,297	0.775	0.235
Share of black individuals	24,297	0.109	0.097

Note: The table shows the number of observations, means and standard deviations of the named variables in the sample. The unit of observation is at the state, month, age group and gender level (averages). *Source:* UCR, CPS, BLS and own calculations.

Table 4.4: Unemployment and Property Crime

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Y =	ln(PCR)	ln(PCR)	ln(PCR)	ln(PCR)
Z =			PBD	PBD
ln(UR)	-0.0012	-0.0003	0.0856***	0.1455***
	(0.0064)	(0.0047)	(0.0236)	(0.0404)
State trend: t, t^2	x	x	x	x
Fixed effects	x	x	x	x
Control variables	-	x	-	x
F-Statistic			>100	>100
Sample size	24,297	24,037	24,297	24,037
Control variables:				
Share married		-0.2250***		-0.1757
		(0.0748)		(0.0797)
Share native		0.0704		0.0246
		(0.1328)		(0.1331)
Share high-school		-0.0352		0.0203
		(0.1141)		(0.1132)
Share black		-0.2580		-0.3838**
		(0.1617)		(0.1633)
$\ln(\text{UR})_{t-1}$		-0.0023		-0.0625***
		(0.0039)		(0.0184)

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the lagged unemployment rate (period t-1). Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for property crime, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment rates are computed as the number of unemployed individuals as a percentage of the labour force in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.5: Unemployment and Violent Crime

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Y =	ln(VCR)	ln(VCR)	ln(VCR)	ln(VCR)
Z =			PBD	PBD
ln(UR)	0.0012	0.0007	0.0219	0.0308
	(0.0067)	(0.0049)	(0.0233)	(0.0379)
State trend: t, t^2	x	x	x	x
Fixed effects	x	x	x	x
Control variables	-	x	-	x
F-Statistic			>100	>100
Sample size	24,297	24,037	24,297	24,037
Control variables:				
Share married		-0.2599***		-0.2497***
		(0.0854)		(0.0822)
Share native		0.1462**		0.1367*
		(0.0704)		(0.0698)
Share high-school		0.0384		0.0498
		(0.0715)		(0.0682)
Share black		-0.0762		-0.0932
		(0.0898)		(0.0980)
$\ln(\text{UR})_{t-1}$		-0.0039		-0.0164
		(0.0055)		(0.0185)

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the lagged unemployment rate (period t-1). Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for violent crime, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment rates are computed as the number of unemployed individuals as a percentage of the labour force in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1.
Source: CPS, UCR and own calculations.

Table 4.6: Unemployment and Drug Crime

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Y =	ln(DCR)	ln(DCR)	ln(DCR)	ln(DCR)
Z =			PBD	PBD
ln(UR)	0.0317***	0.0181***	-.0562	-0.1151
	(0.0095)	(0.0058)	(0.0765)	(0.1220)
State trend: t, t^2	x	x	x	x
Fixed effects	x	x	x	x
Control variables	-	x	-	x
F-Statistic			>100	>100
Sample size	24,297	24,037	24,297	24,037
Control variables:				
Share married		-0.5567***		-0.6018***
		(0.1178)		(0.1156)
Share native		-0.7990		-0.7572
		(0.5203)		(0.5407)
Share high-school		-0.9098		-0.8605***
		(0.2421)		(0.1986)
Share black		0.1753		0.2902
		(0.2272)		(0.2113)
ln(UR)$_{t-1}$		0.0237***		0.0787*
		(0.0078)		(0.0436)

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the lagged unemployment rate (period t-1). Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for drug crime, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment rates are computed as the number of unemployed individuals as a percentage of the labour force in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.7: Reduced Form and Placebo Test

	(1) OLS	(2) OLS	(3) OLS
Y =	ln(PCR)	ln(VCR)	ln(DCR)
Panel A: Full sample, reduced form			
PBD	0.0034***	0.0008	-.0023
(in months)	(0.0009)	(0.0009)	(0.0030)
Panel B: Great Recession sample, reduced form			
PBD^{GR}	0.0023*	-0.0049*	0.0008
(in months)	(0.0012)	(0.0028)	(0.0022)
Panel C: 1980s sample, placebo test			
PBD^{GR}	-0.0018	-0.0011	-0.0027
(in months)	(0.0020)	(0.0069)	(0.0058)
State trend: t, t^2	x	x	x
Fixed effects	x	x	x
Sample size A	24,297	24,297	24,297
Sample size B	10,112	10,112	10,112
Sample size C	3,790	3,790	3,790

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. Fixed effects for the state, age group and gender are included. The dependent variables are the logarithmic arrest rates for property crime, violent crime and drug crime respectively, where the arrest rate is computed as the number of arrests per 100,000 population. The independent variable PBD is the potential benefit duration, measured in weeks. Panel A: January 2003 - December 2011. Panel B: December 2007 - June 2009. Panel C: May 1981 - November 1982. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.8: First Stage

	(1) OLS	(2) OLS	(3) OLS	(4) OLS
Y =	ln(UR)	ln(UR)	ln(UD)	ln(UD)
Panel A: Current unemployment rate				
PBD	0.0389***	0.0238***		
(in months)	(0.0015)	(0.0016)		
Panel B: Average unemployment duration				
PBD			0.0099***	0.0076***
(in weeks)			(0.0016)	(0.0015)
State trend: t, t^2	x	x	x	x
Fixed effects	x	x	x	x
Control variables	-	x	-	x
Sample size	24,297	24,037	24,297	24,297

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the lagged (panel A) or current (panel B) unemployment rate. Fixed effects for the state, age group and gender are included. The dependent variable is the unemployment rate computed as the number of unemployed individuals as a percentage of the labour force in the observational unit or the average ongoing unemployment duration in the observational unit. The instrument PBD is the potential benefit duration, measured in weeks. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS and own calculations.

Table 4.9: Unemployment Duration and Property Crime

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Y =	ln(PCR)	ln(PCR)	ln(PCR)	ln(PCR)
Z =			PBD	PBD
Panel A: Average unemployment duration				
ln(UD)	0.0093**	0.0097**	0.0842***	0.114***
	(0.0037)	(0.0036)	(0.0218)	(0.0316)
Panel B: Interaction with unemployment rate				
ln(UD) x ln(UR)				0.0465***
				(0.0118)
Panel C: Median unemployment duration				
ln(UD)				0.0824***
				(0.0214)
State trend: t, t^2	x	x	x	x
Fixed effects	x	x	x	x
Control variables	-	x	-	x
F-Statistic A			>100	>100
F-Statistic B				>100
F-Statistic C				>100
Sample size	24,297	24,297	24,297	24,297

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the lagged unemployment rate (period t-1). Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for property crime, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment rates are computed as the number of unemployed individuals as a percentage of the labour force in the observational unit. Unemployment duration is computed as the average/median ongoing unemployment duration in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.10: Unemployment Duration and Violent Crime

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Y =	ln(VCR)	ln(VCR)	ln(VCR)	ln(VCR)
Z =			PBD	PBD
Panel A: Average unemployment duration				
ln(UD)	0.0059**	0.0056**	0.0215	0.0204
	(0.0028)	(0.0026)	(0.0220)	(0.0291)
Panel B: Interaction with unemployment rate				
ln(UD) x ln(UR)				0.0077
				(0.0117)
Panel C: Median unemployment duration				
ln(UD)				0.0136
				(0.0210)
State trend: t, t^2	x	x	x	x
Fixed effects	x	x	x	x
Control variables	-	x	-	x
F-Statistic A			>100	>100
F-Statistic B				>100
F-Statistic C				>100
Sample size	24,297	24,297	24,297	24,297

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the lagged unemployment rate (period t-1). Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for violent crime, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment rates are computed as the number of unemployed individuals as a percentage of the labour force in the observational unit. Unemployment duration is computed as the average/median ongoing unemployment duration in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.11: Unemployment Duration and Drug Crime

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Y =	ln(DCR)	ln(DCR)	ln(DCR)	ln(DCR)
Z =			PBD	PBD
Panel A: Average unemployment duration				
ln(UD)	0.0176*	0.0116	-0.0552	-0.0876
	(0.0098)	(0.0082)	(0.0687)	(0.0888)
Panel B: Interaction with unemployment rate				
ln(UD) x ln(UR)				-0.0373
				(0.0373)
Panel C: Median unemployment duration				
ln(UD)				-0.0661
				(0.0670)
State trend: t, t^2	x	x	x	x
Fixed effects	x	x	x	x
Control variables	-	x	-	x
F-Statistic A			>100	>100
F-Statistic B				>100
F-Statistic C				>100
Sample size	24,297	24,297	24,297	24,297

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the lagged unemployment rate (period t-1). Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for property crime, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment rates are computed as the number of unemployed individuals as a percentage of the labour force in the observational unit. Unemployment duration is computed as the average/median ongoing unemployment duration in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.12: Robustness Test, Police Employment

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) OLS
Y =	ln(PCR)	ln(VCR)	ln(DCR)	police per capita
Z =	PBD	PBD	PBD	
Panel A: Current unemployment rate				
ln(UR)	0.1465***	0.0320	-0.1149	
	(0.0413)	(0.0381)	(0.1210)	
Police per capita	0.1002*	0.1231***	0.0220	
	(0.0562)	(0.0452)	(0.0857)	
Panel B: Average unemployment duration				
ln(UD)	0.1162***	0.0223	-0.0879	
	(0.0296)	(0.0286)	(0.0888)	
Police per capita	0.1566***	0.1307***	-0.0233	
	(0.0531)	(0.0493)	(0.0762)	
Panel C: Police per capita				
PBD				0.0006
				(0.0005)
State trend: t, t^2	x	x	x	x
Fixed effects	x	x	x	x
Control variables	x	x	x	x
Sample size A	24,037	24,037	24,037	
Sample size B	24,297	24,297	24,297	
Sample size C				24,297

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the lagged (panel A) or current (panel B) unemployment rate. Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for property, violent and drug crime respectively, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment rates are computed as the number of unemployed individuals as a percentage of the labour force in the observational unit. Unemployment duration is computed as the average ongoing unemployment duration in the observational unit. Police per capita measures the ratio of police employees to the population per state and year. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.13: Robustness Test, Income (CPS March Sample)

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Y =	ln(PCR)	ln(PCR)	ln(VCR)	ln(VCR)	ln(DCR)	ln(DCR)
Z =	PBD	PBD	PBD	PBD	PBD	PBD
Panel A: Current unemployment rate						
ln(UR)	0.225***	0.299***	0.114	0.135	0.133	0.122
	(0.0538)	(0.0977)	(0.0728)	(0.0853)	(0.216)	(0.262)
Income		0.000*		0.000*		-0.000***
		(0.000)		(0.000)		(0.000)
Welfare		0.0003***		-0.0004***		0.0008***
		(0.0001)		(0.0001)		(0.0003)
UB		-0.0002*		-0.0001		0.0001
		(0.0001)		(0.0001)		(0.0001)
Panel B: Average unemployment duration						
ln(UD)	0.172***	0.206***	0.0649	0.0722	0.110	0.105
	(0.0661)	(0.0949)	(0.0539)	(0.0583)	(0.185)	(0.208)
Income		0.000		0.000		-0.000***
		(0.000)		(0.000)		(0.000)
Welfare		0.0002		-0.0004***		0.0007***
		(0.0001)		(0.0001)		(0.0003)
UB		-0.000		-0.000		0.000
		(0.000)		(0.000)		(0.0001)
State trend: t, t^2	x	x	x	x	x	x
Fixed effects	x	x	x	x	x	x
Control variables	x	x	x	x	x	x
Income variables	-	x	-	x	-	x
Sample size	2,080	2,080	2,080	2,080	2,080	2,080

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the lagged (panel A) or current (panel B) unemployment rate. Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for property, violent and drug crime respectively, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment rates are computed as the number of unemployed individuals as a percentage of the labour force in the observational unit. Unemployment duration is computed as the average ongoing unemployment duration in the observational unit. The income control variables include average income, welfare income and unemployment benefit income. The sample is reduced to the CPS March sample only due to data availability. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.14: Unemployment Duration Dependence, Duration Intervals

	(1) 2SLS	(2) 2SLS	(3) 2SLS
Y =	ln(PCR)	ln(VCR)	ln(DCR)
Z =	PBD	PBD	PBD
Panel A: Average unemployment duration ≤ baseline PBD			
ln(UD)	0.216***	0.0903**	-0.0753
	(0.0419)	(0.0430)	(0.129)
Panel B: Average unemployment duration > baseline and ≤ extended PBD			
ln(UD)	0.0154	-1.873*	-0.0968
	(0.263)	(1.052)	(0.573)
Panel C: Average unemployment duration > extended PBD			
ln(UD)	-0.0230	0.0054	-0.249*
	(0.0594)	(0.0460)	(0.129)
State trend: t, t^2	x	x	x
Fixed effects	x	x	x
Control variables	x	x	x
Sample size A	18,627	18,627	18,627
Sample size B	3,950	3,950	3,950
Sample size C	1,720	1,720	1,720

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the current unemployment rate. Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for property, violent and drug crime respectively, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment duration is computed as the average ongoing unemployment duration in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.15: Unemployment Duration Dependence, Duration Quintiles

	(1) 2SLS	(2) 2SLS	(3) 2SLS
Y = Z =	ln(PCR) PBD	ln(VCR) PBD	ln(DCR) PBD
Panel A: First quintile, $\leq$ 7 weeks			
ln(UD)	0.462**	0.067	-0.704
	(0.229)	(0.252)	(0.429)
Panel B: Second quintile, $\leq$ 12 weeks			
ln(UD)	1.051	0.778	-1.741
	(0.814)	(0.790)	(1.493)
Panel C: Third quintile, $\leq$ 18.26 weeks			
ln(UD)	2.908***	0.556	-0.702
	(1.041)	(0.553)	(1.458)
Panel D: Fourth quintile, $\leq$ 28.14 weeks			
ln(UD)	1.160**	0.531**	-0.543
	(0.476)	(0.261)	(1.050)
Panel E: Fifth quintile, $>$ 28.14 weeks			
ln(UD)	-0.0932	-3.429	-3.987
	(0.774)	(3.663)	(3.854)
State trend: t, t^2	x	x	x
Fixed effects	x	x	x
Control variables	x	x	x
Sample size A	4,859	4,859	4,859
Sample size B	4,874	4,874	4,874
Sample size C	4,846	4,846	4,846
Sample size D	4,858	4,858	4,858
Sample size E	4,860	4,860	4,860

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the current unemployment rate. Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for property, violent and drug crime respectively, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment duration is computed as the average ongoing unemployment duration in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.16: Unemployment Duration Dependence, Type

	(1) 2SLS	(2) 2SLS	(3) 2SLS
Y =	ln(PCR)	ln(VCR)	ln(DCR)
Z =	PBD	PBD	PBD
Panel A: Job losers			
ln(UD)	0.0893**	0.0114	-0.0719
	(0.0316)	(0.0269)	(0.0760)
Panel B: Job leavers			
ln(UD)	0.137***	0.0265	-0.134
	(0.0330)	(0.0317)	(0.0978)
Panel C: New entrants			
ln(UD)	0.128***	0.0110	0.0072
	(0.0338)	(0.0298)	(0.0811)
Panel D: Re-entrants			
ln(UD)	0.129***	0.0154	-0.0791
	(0.0327)	(0.0330)	(0.101)
State trend: t, t^2	x	x	x
Fixed effects	x	x	x
Control variables	x	x	x
Sample size A	19,691	19,691	19,691
Sample size B	9,676	9,676	9,676
Sample size C	7,317	7,317	7,317
Sample size D	17,696	17,696	17,696

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished highschool, share of black population, and the current unemployment rate. Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for property, violent and drug crime respectively, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment duration is computed as the average ongoing unemployment duration in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.17: Robustness Test, Gender

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Y =	ln(PCR) Males	ln(PCR) Females	ln(PCR) Males Burglary	ln(PCR) Females Burglary	ln(PCR) Males Larceny	ln(PCR) Females Larceny
Z =	PBD	PBD	PBD	PBD	PBD	PBD
Panel A: Gender specific estimation						
$\ln(\text{UD})_{male}$	0.0151		-0.0927		0.0907**	
	(0.0500)		(0.0595)		(0.040)	
$\ln(\text{UD})_{female}$		0.241***		-0.164		0.332***
		(0.0343)		(0.102)		(0.040)
Panel B: Cross-gender estimation						
$\ln(\text{UD})_{male}$		0.166***				
		(0.0238)				
$\ln(\text{UD})_{female}$	0.0079					
	(0.0495)					
State trend: t, t^2	x	x	x	x	x	x
Fixed effects	x	x	x	x	x	x
Control variables	x	x	x	x	x	x
Sample size A	12,199	12,098	12,199	12,098	12,199	12,098
Sample size B	11,694	11,694				

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the current unemployment rate. Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for property, violent and drug crime respectively, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment duration is computed as the average ongoing unemployment duration in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Table 4.18: Robustness Test, Age Groups

	(1) 2SLS	(2) 2SLS	(3) 2SLS
Y = Z =	ln(PCR) PBD	ln(VCR) PBD	ln(DCR) PBD
Panel A: Aged 16-19			
ln(UD)	0.154***	0.0024	0.121
	(0.047)	(0.0608)	(0.142)
Panel B: Aged 20-24			
ln(UD)	0.186***	0.0165	0.0124
	(0.047)	(0.0378)	(0.0916)
Panel C: Aged 25-29			
ln(UD)	0.200***	0.0906*	-0.120*
	(0.070)	(0.0530)	(0.0695)
Panel D: Aged 30-34			
ln(UD)	0.0814*	0.0516	0.137*
	(0.043)	(0.0428)	(0.070)
Panel E: Aged 35-39			
ln(UD)	-0.0403	-0.0422	-0.348***
	(0.043)	(0.0393)	(0.0891)
State trend: t, t^2	x	x	x
Fixed effects	x	x	x
Control variables	x	x	x
Sample size A	4,988	4,988	4,988
Sample size B	4,933	4,933	4,933
Sample size C	4,856	4,856	4,856
Sample size D	4,710	4,710	4,710
Sample size E	4,750	4,750	4,750

Note: Standard errors are shown in parentheses. The level of observation is at the state, year, month, age group and gender level. The control variables include the share of married individuals, share of native born individuals, share of individuals who finished high-school, share of black population, and the current unemployment rate. Fixed effects for the state, age group and gender are included. The dependent variable is the logarithmic arrest rate for property, violent and drug crime respectively, where the arrest rate is computed as the number of arrests per 100,000 population. The instrument PBD is the potential benefit duration, measured in weeks. Unemployment duration is computed as the average ongoing unemployment duration in the observational unit. Standard errors are clustered at the state level. Regressions are population weighted. *** p<0.01, ** p<0.05, * p<0.1. *Source:* CPS, UCR and own calculations.

Chapter 5. Crime and Racial Profiling: New York's Stop-and-Frisk Policy

I would like to thank my co-authors Laura Jaitman and Stephen Machin for their contributions.

5.1 Introduction

In July 2014, Eric Garner, a 44-year old Afro-American, died in New York after a police officer reportedly put him in a "chokehold" following a suspicion of illegal cigarette sales. About one month later, the fatal shooting of Michael Brown, an 18-year-old Afro-American, triggered protests and unrest in Ferguson, Missouri. In April 2015 the death of Freddie Gray, a 25-year old Afro-American who died of injuries which he sustained during arrest, led to days of severe riots in Baltimore. Intense national and international media coverage of these events triggered an enhanced popular and political debate about racial discrimination in policing in the United States, in particular against the Afro-American population. The findings in an article by The Washington Post in May 2015 suggest that while half of the victims of fatal shootings by police in 2015 were white, among the unarmed victims two-thirds were Black or Hispanic.[1] Moreover, the statistics shown in the article reveal disproportional numbers of blacks being killed by the police when adjusting for population counts at the census tract level.

In particular the death of Eric Garner in Staten Island, New York City, gave rise to new and old concerns about the city's stop-and-frisk practices. The stop-and-frisk programme entitles police officers in New York City to stop, question and frisk suspects based on reasonable suspicion of criminal activity. The legal basis for police officers in the U.S. to stop, question and frisk citizens was established in 1968 when the U.S. Supreme Court ruled in *Terry v. Ohio* that a pedestrian stop was constitutional if there was a reasonable suspicion that a crime was about to be committed or in the process of being committed. In New York City, these stops were legally enacted in 1971 by Criminal Procedure Law §140.50. Leading up to the 1990s, stops were followed by frisks only under stricter requirements regarding a suspicion of a weapon or an escalation of the situation. Stop-and-frisk practices became more widely used in the 1990s when mayor Rudolph Giuliani and police commissioner William J. Bratton implemented the zero-tolerance strategy in New York City. The strategy was based on the so-called Broken Windows theory (Wilson and Kelling, 1982) which suggests that disorder generates and sustains more serious crime. In that context, the stop-and-frisk practices were expanded in order to target minor crime more aggressively and, based on this notion, to deter and reduce more serious crime.[2]

The policy has been controversial since then and in particular it has raised con-

[1] "Fatal police shootings in 2015 approaching 400 nationwide", The Washington Post, 30 May 2015. Link to the article: www.washingtonpost.com

[2] See section 5.2 for more details.

cerns about racial profiling, the majority of individuals who are stopped being black or belonging to another minority ethnicity.[3] While the advocates of racial profiling practices claim that the disproportional share of stops of blacks matches the disproportional high crime rates among the black population, opponents argue that the practices in New York City are racially discriminating against the black population.

In this chapter we examine the hypothesis of racial bias and the impact on crime in New York City. First, we use a number of different empirical strategies in order to identify racial bias in the stop-and-frisk practices. Second, we estimate the overall impact of stop-and-frisk practices on crime in New York City and in particular we investigate whether racial bias in policing affects crime rates or the probability of being arrested. We use precinct level data on stop-and-frisks and reported crime on the one hand, and individual level data on stop-and-frisks and subsequent arrests on the other hand. Our empirical strategy exploits quasi-experimental variation from a court decision in 2013 on the unconstitutionality of stop-and-frisk practices, as well as exogenous variation from police officer killings. Applying a range of estimation techniques we find that our results are qualitatively robust across the specifications: While we find evidence that supports the hypothesis that Afro-Americans face a disproportional probability of a stop-and-frisk encounter, our estimations suggest that there is no knock-on effect on crime.

Overall, this chapter contributes to the literature on identifying racial bias, to the literature on the effectiveness of stop-and-frisk practices to reduce crime as well as to the larger literature on the causal effect of general policing on crime. Is there any evidence for a racial profiling strategy in the stop-and-frisk practices which is discriminatory against ethnic minorities? What are the implications for criminal behaviour? The main difficulty here is to disentangle preference-based discrimination from statistical discrimination. Knowles *et al.* (2001) provide a model of police and motorist behaviour from which they derive an empirical test for preference-based discrimination in traffic stops. In their application, they use data from Maryland and find no support for the racial bias hypothesis. Related to racial bias in traffic stops, Grogger and Ridgeway (2006) develop a statistical test that is similar to an auditing study: If there is no racial bias, then traffic stop behaviour should be the same by day and by night, when the race of the motorist can not be seen before the stop. Applying their test to Californian data, the authors find no evidence that would support a racial bias hypothesis. Persico (2009) derives a model

[3]See for example the coverage of the issue by the New York Civil Liberties Union, or the topic page by The New York Times.
http://www.nyclu.org/issues/racial-justice/stop-and-frisk-practices;
http://topics.nytimes.com/top/reference/timestopics/subjects/s/stop_and_frisk/index.html

to identify preference-based discrimination in observational data. Antonovics and Knight (2009) follow a different approach and exploit variation in the race of the officer to identify racial bias in traffic stops and searches using data from the Boston Police Department. They find that conditional on being stopped, the probability of being searched is higher when the officer's race is different from the motorist's race.

Most related to our paper, Gelman *et al.* (2007) use police precinct-level data from the stop-and-frisk programme in New York City. They find that indeed Afro-Americans and Hispanics are stopped more often than Whites even after scaling by race-specific crime rates and controlling for precinct heterogeneity. Their findings suggest that there is racial bias in the case of New York City, contradictory to the findings from the traffic stops studies. In contrast, Weisburd *et al.* (2014) argue that stop-and-frisks are concentrated at crime hot spots. According to the authors, the racial disparities in the probability of being stopped and frisked can be explained by a hot spot focused policing strategy. In that sense, their argumentation suggests statistical rather than preference-based discrimination. Coviello and Persico (2013) develop a model that allows for two sources of bias at the police officer level on the one hand and at the police chief (precinct) level on the other hand. The authors find no strong evidence for racial bias in the stop-and-frisk practices in New York City. Legewie (2014) uses an event study design and provides statistical evidence that the killing of police officers triggers a disproportionate use of police force against ethnic minorities during stop-and-frisk incidents.

Whether or not the stop-and-frisk practices as part of the more aggressive policing under the zero-tolerance strategy have contributed to the significant decrease in crime in New York City over the last two decades is controversially discussed in the literature. In their article Kelling and Bratton (1998) strongly support the Broken Windows theory and argue that the decline in crime in New York City during the 1990s is mostly due to more effective and more aggressive policing with respect to misdemeanour crime. Using time-series data for New York City from 1970 to 1996, Corman and Mocan (2000) find evidence to support that general police-crime-deterrence hypothesis. Contrary to their findings, Harcourt and Ludwig (2006) do not find any causal evidence from observational data to support the Broken Windows theory for New York City. Moreover, evaluating the Moving-To-Opportunity experiment their study suggests that moving families to neighbourhoods with less social disorder does not lead to a reduction in their criminal behaviour, a finding that contradicts the Broken Windows theory. MacDonald *et al.* (2015) evaluate the effect of the impact zone programme in New York City with respect to crime. Again, the authors find little evidence that the policy can be causally linked to a crime reduction. Rosenfeld and Fornango (2014) explicitly evaluate the impact of the stop-and-frisk practices on annual, precinct level robbery and burglary rates in New York City. The authors do not find any convincing support for a crime-deterring effect.

This chapter also relates to the larger literature on the causal effect of policing on crime. Levitt (1997), Levitt (2002) and Evans and Owens (2007) document a crime reducing effect of policing in quasi-experimental settings. Other studies such as Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005) or Draca *et al.* (2011) exploit variation in police deployment triggered by terroristic events and find evidence for crime deterrence effects of policing. In addition to these quasi-experimental studies, there is a body of criminological literature studying the impact of police deployment to crime "hot spots", i.e. particularly high crime areas, in randomised controlled trials. For example, Sherman and Weisburd (1995) and Ratcliffe *et al.* (2011) evaluate experiments in Minneapolis and Philadelphia, respectively, both studies find that police deployment to crime hot spots significantly reduces local crime rates.

The remainder of the chapter is structured as follows. In the next section, we provide more detailed background information on the stop-and-frisk practices and racial profiling in New York City. In section 5.3, a theoretical model of racial bias is introduced that motivates our empirical analysis. The different data sources and the estimation sample are described in section 5.4 while the empirical strategy is explained in section 5.5. We discuss our estimation results in section 5.6. Section 5.7 concludes.

5.2 Stop-and-Frisk in New York City

In the following, we provide more details with respect to the history of stop-and-frisk policies in New York City as well as with respect to racial profiling and claims of racial bias.

5.2.1 Stop-and-Frisk

Stop-and-Frisk is a crime prevention strategy which is used by the New York City Police Department. As such, police officers are entitled to stop a person if there is a reasonable suspicion of criminal activity and to subsequently frisk that person if there is a reasonable suspicion that the person is armed and dangerous (Rudovsky and Rosenthal, 2013). When individuals are stopped by a police officer, first they are questioned. Then, they are either allowed to move on or they are frisked. In some cases, police officers subsequently use physical force, search the person, arrest the person or issue a summons (Jones-Brown *et al.*, 2013).[4]

In 1968, the U.S. Supreme Court ruled in *Terry v. Ohio* that a pedestrian stop was constitutional if there was a reasonable suspicion that a crime was about to be committed or in the process of being committed, and hence established the national legal basis for stop-and-frisk practices (Jones-Brown *et al.*, 2013). In New York City, these stops were legally enacted in 1971 by Criminal Procedure Law §140.50 which states the following about temporary questioning of persons in public places and search for weapons:[5]

> 1. In addition to the authority provided by this article for making an arrest without a warrant, a police officer may stop a person in a public place located within the geographical area of such officer's employment when he reasonably suspects that such person is committing, has committed or is about to commit either (a) a felony or (b) a misdemeanor defined in the penal law, and may demand of him his name, address and an explanation of his conduct. [...] 3. When upon stopping a person under circumstances prescribed in subdivisions one and two a police officer or court officer, as the case may be, reasonably suspects that he is in danger of physical injury, he may search such person for a deadly weapon or any instrument, article or substance readily capable of causing serious physical injury and of a sort not ordinarily carried in public places by law-abiding persons. If he finds such a weapon or instrument, or any other property possession of which he reasonably believes may constitute the commission of a crime, he may take it and keep it until the completion of the questioning, at which time he shall either return it, if lawfully possessed, or arrest such person. [...]

The policy has been controversial since then. In 1976, four different levels of legally permissible stop-and-frisk encounters were established in *People v. De Bour*. Here, level 1 requires the police officer to have an objective and credible reason to approach an individual to request information, while level 2 relates to the common law right of enquiry and requires a founded suspicion. A level 3 street encounter requires the police officer to have a reasonable suspicion in order to forcibly stop (and frisk, if there is a suspicion of a weapon being carried) the individual. The most serious street encounter, level 4, requires a probable cause against the individual and entitles the police officer to arrest and search the person.[6]

[4] A summons can be issued by the police officer for a range of misdemeanour crimes and then requires the suspect to appear at court at a given date and time. Common crimes for which summons are issued include for example consumption of alcohol on the street or disorderly conduct.

[5] See `http://ypdcrime.com/cpl/article140.htm`

[6] See for example (Jones-Brown *et al.*, 2013) and sources cited within that report for more details.

Leading up to the 1990s, stops were followed by frisks only under these relatively strict requirements regarding a suspicion of a weapon or an escalation of the situation. In the 1990s, New York City's mayor Rudolph Giuliani and police commissioner William J. Bratton adopted the stop-and-frisk strategy more widely as part of the CompStat and zero-tolerance strategies.[7] These strategies were inspired by the Broken Windows theory (Wilson and Kelling, 1982) which suggests that disorder (e.g. broken windows) generate and sustain more serious crime:

> "[...] if a window in a building is broken and is left unrepaired, all the rest of the windows will soon be broken. One unrepaired broken window is a signal that no one cares, and so breaking more windows costs nothing"

The theory is based on the notion that neighbourhoods that suffer from disorder may have limited social control and self-regulation and thus trigger more serious crime, these neighbourhoods predominantly being poor neighbourhoods. Hence, the stop-and-frisk practices were expanded in order to target minor crime more aggressively and, based on the ideas of the Broken Windows theory, to deter and reduce more serious crime. Favouring the idea of the broken windows theory, Kelling and Bratton (1998) indeed argue that more effective, targeted and aggressive policing, including stop-and-frisk practices, with respect to misdemeanour crime has vastly contributed to the big crime decline in New York City during the 1990s.

5.2.2 Racial Profiling

Over the last two decades, the stop-and-frisk policy has been subject to claims of racial profiling and racial bias. Racial profiling in the context of stop-and-frisk is a form of discrimination in which police enforcement is mainly based on a person's race or ethnicity (Legewie, 2014). Findings in the literature suggest that the probability of being stopped and frisked by a police officer substantially depends on the person's race as well as the racial composition of the neighbourhood. Supporters of the stop-and-frisk practices on the one hand have defended racial disparities in stop-and-frisks by racial disparities in crime rates, referring to the Broken Window theory and claiming reasonable and efficient police practices. Opponents of the stop-and-frisk practices on the other hand have argued that police officers racially discriminate against minorities and that such discrimination is not justified by statistical discrimination as the Broken Windows theory would suggest (for discussions,

[7] CompStat essentially is a management process which relies on collecting and analysing crime data in order to strategically and more effectively combat crime. It was introduced by William Bratton in New York City in 1994.

see for example Fagan (2002), Goldberg (1999), Kelling and Bratton (1998), or a more recent essay by Bergner (2014)).

The stop-and-frisk practices have not only led to political controversies but also to legal challenges. In 1999, the Center for Constitutional Rights (CCR) filed a class action lawsuit to challenge the NYPD for allegedly conducting stop-and-frisk practices without reasonable suspicions but with a racial bias. In *Daniels, et al. v. The City of New York*, the parties agreed on a settlement which resulted in requirements of stop-and-frisk data transparency as well as a written policy prohibiting racial profiling (Jones-Brown *et al.*, 2013). In 2008, the CCR filed another class action lawsuit against the City of New York, the police commissioner Raymond Kelly and mayor Michael Bloomberg being among the defendants, alleging unconstitutional stop-and-frisk practices by the New York Police Department based on race rather than reasonable suspicion. In August 2013, U.S. District Court Judge Shira Sheindlin ruled in *Floyd, et al. v. The City of New York* that the NYPD's stop-and-frisk practices were racially discriminatory and in that sense unconstitutional. The judge ruling was followed by a political and legal controversy when the City of New York appealed, and Judge Sheindlin was withdrawn from the case. In January 2014, the City of New York announced that it would drop its appeal. Later that year, mayor Bill de Blasio pledged for a reform of the stop-and-frisk strategy as well as a strong racial profiling bill.

5.3 Theoretical Framework

In this chapter, we examine the hypothesis of racial bias and the impact on crime in New York City. In order to give our analysis a theoretical framework, in the following we discuss a simple equilibrium model of racial profiling in police stops as well as some basic welfare considerations with respect to crime reducing effects of racial profiling.

5.3.1 A Simple Equilibrium Framework

The main challenge for an empirical test of racial bias in stop-and-frisk practices is to distinguish between statistical discrimination and preference-based discrimination: If racial disparities in the stop-and-frisk frequencies coincide with racial disparities in crime rates, the differences correspond to statistical discrimination. Yet, if there are racial disparities in stop-and-frisk frequencies over and above differences in crime rates, that suggests preference-based discrimination or racial bias.[8]

In a seminal paper, Knowles *et al.* (2001) develop an equilibrium model of racial bias in motor vehicle searches that leads to an empirical test of racial discrimination. While their model studies the behaviour of motorists when police officers stop vehicles and search for contraband, it is straightforward to generalise the model to pedestrian stops such as stop-and-frisk encounters in New York City. The following model is hence based on Knowles *et al.* (2001), with small adjustments to fit the purpose of our analysis.[9]

The model describes the behaviour of suspects and police officers in a setting where police officers can stop suspects. In the model, suspects are either black (B) or white (W). Let $r \in \{B, W\}$ denote the suspect's race. Let c denote characteristics other than race which the police officer can use in order to decide whether or not to stop the suspect. For example, c may include a particular type of clothing. Let c be distributed according to the distribution functions $F(c|B)$ and $F(c|W)$. Each police officer decides whether to stop a suspect of type (c, r). The cost of stopping a suspect of type (c, r) is t_r with $t_W, t_B \in (0, 1)$. The benefits of an arrest are normalised to 1. Let G denote the event that the suspect who is stopped is found guilty of a crime, for example illegally carrying a weapon. Further, let $\gamma(c, r) = P(\text{stop}|c, r)$ denote the probability that a police officer stops a suspect of type (c, r).

Suspects decide whether or not to commit a criminal offence, taking into account four possible scenarios: A suspect commits crime or not, and in either case may or may not be stopped by a police officer. If suspects do not commit crime, then their payoff is zero independently of the stop. Here, the model abstracts from cost for a suspect who is wrongly accused and stopped. If suspects commit crime, their payoff is $-j(c, r)$ if they are stopped and $v(c, r)$ else.[10] The expected payoff from crime for a suspect S can then be written as:

$$\Pi_S = \gamma(c, r) \cdot [-j(c, r)] + [1 - \gamma(c, r)] \cdot v(c, r) \tag{5.1}$$

Given the probability of being stopped, the suspect decides whether or not to

[8]See Arrow (1973) and Becker (1957) for the seminal contributions on the notions of statistical and preference-based discrimination.

[9]Subsequent papers extended the model to incorporate additional features. Dharmapala and Ross (2004) allow for the fact that the police observes a share of the population of suspects only, and accordingly scale the search probability by the probability of being observed by the police. Further, the authors allow for heterogeneity in the offense severity. Bjerk (2007) derives a theoretical model in which he allows for a noisy signal of the suspect's guilt. Anwar and Fang (2006) and Antonovics and Knight (2009) extend the model in Knowles *et al.* (2001) by introducing different races not only for suspects but also for police officers.

[10]Here, we abstract from mechanisms of being caught for crime other than being stopped.

commit crime while maximising the payoff.[11] If $\Pi_S > 0$, the suspect chooses to commit crime. If $\Pi_S = 0$, suspects in the model are willing to randomise between committing and not committing crime.

Let $P(G|c,r)$ be the probability that a suspect of type (c,r) commits crime. Taking that probability as given, police officers maximise their expected payoff taking into account the number of successful stops, i.e. the number of stops of suspects who are found guilty, as well as the cost of stopping suspects. Hence, they choose the stopping probabilities $\gamma(c,r)$ for each type (c,r) to solve the following optimisation problem:

$$\max_{\gamma(c,W),\gamma(c,B)} \sum_{r=W,B} \int [P(G|c,r) - t_r]\gamma(c,r)f(c|r)dc \tag{5.2}$$

As Knowles *et al.* (2001) point out, one can think of the term $\Pi_P = P(G|c,r) - t_r$ as the police officer's payoff from stopping a suspect of type (c,r). If $\Pi_P > 0$, police officers always stop a suspect of type (c,r). If $\Pi_P = 0$, police officers are willing to randomise between stopping and not stopping the suspect.

In an equilibrium, where police officers randomise over whether to stop a suspect and suspects randomise over whether to commit crime, it hence holds that both $\Pi_S = 0$ and $\Pi_P = 0$. Therefore, we know that in that equilibrium, for all c it is true that:

$$\gamma^*(c,r) = \frac{v(c,r)}{v(c,r) + j(c,r)} \quad \forall\, r \in \{B,W\} \tag{5.3}$$

and

$$P^*(G|c,r) = t_r \quad \forall\, r \in \{B,W\} \tag{5.4}$$

There are two types of discrimination which correspond to the concepts of racial discrimination, or racial bias, and statistical discrimination. Police officers are defined to be racially biased if they prefer stopping one suspect over another solely based on the suspect's race. In that case, police officers have a preference for discrimination and thus the cost of stopping a suspect depends on the suspect's race: $t_B \neq t_W$. In contrast, police officers statistically discriminate against suspects if they choose to stop suspects of one race with a higher probability $\gamma(r)$ than suspect of the other race, but yet without having a preference for discrimination. That implies that $\gamma(B) \neq \gamma(W)$, where $\gamma(r) = \int \gamma(c,r)dF(c|r)\,\forall\, r \in \{B,W\}$, while $t_B = t_W = t$.

[11] Note that the crime decision itself is similar to the setup in the seminal Becker (1968) model of crime, instead of the probability of being caught for crime taking into account the probability of being stopped.

Therefore, if police officers are not racially biased, then it follows from (5.4) that the probabilities of a suspect being guilty must be the same for each $r \in \{B, W\}$ and for all c:[12]

$$P^*(G|c, B) = t = P^*(G|c, W) \tag{5.5}$$

In order to construct an empirical test for racial bias, denote the frequency of a suspect of race r, who is stopped and found guilty as:

$$D(r) = \int P^*(G|c, r) \frac{\gamma^*(c, r) f(c, r)}{\int \gamma^*(s, r) f(s|r) ds} dc \tag{5.6}$$

Under the null hypothesis that police officers are not racially biased, (5.5) holds and hence $D(B) = t = D(W)$. This is an implication that can be tested in the data. In particular, under the null hypothesis that police officers are not racially biased it must then hold that $P(G = 1|c, r) = P(G = 1) \; \forall \; c, r$. Knowles *et al.* (2001) test that in an empirical framework using a hit rate approach. Our empirical approach differs from theirs, but the underlying notions of detecting racial bias are similar. We test for racial bias in the stop-and-frisk practices in a regression framework which is described in section (5.4).

5.3.2 Basic welfare considerations

The model above provides a theoretical framework for racial bias in the stop-and-frisk practices in New York City. Yet, no broader conclusions can be drawn from it with respect to welfare considerations. For example, the model explicitly abstracts from harm for a suspect who is wrongly stopped and accused of crime and does not say anything about the effectiveness of racial profiling as a police strategy. Yet, these are important questions in order to evaluate the stop-and-frisk policy as a public policy and in order to derive policy recommendations over and above concerns about racial discrimination. Durlauf (2006) provides a framework for the evaluation of racial profiling when it is considered as a public policy and welfare considerations are taken into account.

What are the welfare benefits of racial profiling in the stop-and-frisk practices? As outlined above, the policy is based on the notion of the Broken Windows theory and targets a reduction in crime through deterrence mechanisms. Minimising the overall crime rate $P(G)$ subject to the stop rate $\gamma = k \cdot \gamma(B) + (1 - k) \cdot \gamma(W)$, k being the share of the black population, yields a profiling strategy such that (Durlauf,

[12]Note that this does not imply that search intensities across races are the same, i.e. $\gamma^*(c, B) = \gamma^*(c, W)$. The model explicitly allows for statistical discrimination.

2006):

$$\frac{\partial P(G|W,\gamma(W))}{\partial \gamma(W)} = \frac{\partial P(G|B,\gamma(B))}{\partial \gamma(B)} \tag{5.7}$$

The condition relates the sensitivity of crime decisions to changes in the stop-and-frisk probability among blacks to those among whites. Thus, it differs from equation (5.5) which refers to the guilt rates per group. While in the model above, as in other models in the literature, racial profiling is interpreted as a police strategy based on statistical discrimination, here it is seen as a deterrence mechanism based on group-specific sensitivities of crime rates to the respective probability of being stopped.[13]

What are the welfare cost of racial profiling in the stop-and-frisk practices? First, there is individual-specific harm if an innocent person is stopped (and frisked) and if there exist feelings of humiliation, harassment or injustice. There is anecdotical evidence that this is indeed the case, and these issues have become very topical in the context of the recent social unrests in Ferguson and Baltimore for example. Second, as Durlauf (2006) argues, there is social harm if racial profiling leads to self-perpetuating beliefs and the partial promotion of social stigma.

Our empirical analysis examines welfare benefits in terms crime deterrence effects in the case of New York City, evaluating the impact of stop-and-frisk practices and racial profiling on crime rates in the city. Unfortunately, we cannot explicitly test for the welfare cost of racial profiling.

5.4 Data and Estimation Sample

In this section, we describe the different data sources and provide descriptive statistics that illustrate and motivate our empirical analysis.

5.4.1 Data Description

We use data from a number of data sources. First, we exploit individual-level data on stop-and-frisk encounters. Second, we have collected a weekly panel of reported crime at the precinct level. Third, we use census tract population data from the 2010 Decennial U.S. Census.

[13] In addition to the deterrence mechanism Durlauf (2006) discusses a retribution effect of racial profiling. We will not discuss that in more detail here, but refer to the original essay.

Stop-and-Frisk Encounters

As described in more detail above, the stop-and-frisk policy allows police officers in New York City to stop, question and frisk pedestrians based on a reasonable suspicion of criminal activity. The NYPD has a reporting policy for stop-and-frisks made by their police officers. In particular, police officers are required to document a stop under the following conditions: The stop involves the use of force, the person is frisked or more extensively searched, an arrest occurs or the person refuses to provide identification of him- or herself (Jones-Brown *et al.*, 2013). These stops are recorded by filling out the "Stop, Question And Frisk Report Worksheet", known as the UF-250 form (see figure 5A.1 in the appendix). While some of the stops which do not meet these requirements are still documented, not all of them are which leads to concerns about underreporting of stop-and-frisk activity. According to an estimate by Gelman *et al.* (2007) about 70% of the stops are recorded.[14]

Data on each documented stop-and-frisk encounter from 2003 until 2014 are obtained from the NYPD Stop, Question and Frisk Database. The data provide information about the exact date, time, duration and geographical location of the stop. Furthermore, the data contain detailed demographic information about the suspects such that we can identify the race, sex, age and gender. Moreover, the suspected crime as well as the reason for the stop (e.g. furtive movements, see figure 5A.1 for details) are documented. In addition, the outcome of the stop is reported, i.e. we have information on whether the person who was stopped was subsequently frisked, searched, arrested, whether force was used, whether a summons was issued and whether contraband or weapons were found.

Reported Crime

The stop-and-frisk data provide information on the outcome of the stop and in particular whether an arrest has been made or a summons has been issued. While these outcomes are good indicators for whether a crime has been committed by the suspect, there is no information on a subsequent conviction and thus there is potential error if an innocent person has been arrested or issued a summons. Therefore, we use an alternative data strategy to construct precinct-level crime rates based on reported crime data.

Every Monday, the New York City Police Department publishes reported crime statistics which document the weekly number of crimes by precinct and by offence as well as weekly, monthly and annual changes in crime. We have collected the

[14]Lately, new concerns about underreporting have been raised. See for example Goodman and Baker (2015).

data since mid-2012 and we can trace reported crime back to the year 2009. Some weeks are missing and in some weeks information is partly missing, hence we have an unbalanced weekly panel starting in 2009. In order to produce precinct-level crime rates, we match the reported crime panel to population data from the 2010 Decennial U.S. Census obtained from the American Fact Finder. We aggregate the census tract level population data to police precincts in order to match the population data to the crime panel.[15]

5.4.2 Descriptive Statistics

The estimation sample contains 75 police precincts which are located across the five boroughs of New York City. Note that we aggregate police precincts 120, 121 and 123, all placed in the borough of Staten Island, into one: Precinct 121 was established in 2013 only and is located in areas that formerly belonged to precincts 120 and 123. In order to ensure that our results are comparable over time, we thus subsume all three precincts under one artificial precinct. Figure 5.1 shows the geographical distribution of the police precincts by borough. In the following, we restrict the estimation sample to males and in addition we consider the black and white population only.

Variation across Precincts

What is the share of stop-and-frisk encounters of black compared to white individuals? Figure 5.2 shows a map of New York City's police precincts by the average percentage share of stop-and-frisks of black individuals. The stops are pooled over the years 2003-2014 and the precincts categorised into the quartiles of the distribution of the average percentage share. Two things can be observed from figure 5.2: First, the average share of stops of blacks compared to whites appears to be high, accounting to up to 62% even in the lowest quartile of the distribution of precincts. Note that the median share of blacks being stopped amounts to about 78%. Second, the map illustrates spatial concentration of high shares of blacks being stopped by the police in the Bronx, east Brooklyn and southeast Queens.

Do these stop-and-frisk concentrations of high shares of blacks being stopped correspond to the geographical concentration of different ethnic groups? Figure 5.3 shows a map of the police precincts by the average percentage share of the black compared to the white population. The pattern seems to be similar, although not identical, to the previous map. In particular, there are nominal differences in the

[15]We use geocoding data provided online by John Keefe:
http://johnkeefe.net/nyc-police-precinct-and-census-data.

average share of blacks being stopped and the average share of the black population in the precinct: While the median share of stops of blacks amounts to about 78%, the median share of the black population amounts to only 21.5%.

In order to measure the concentration or segregation of the black population, we can compute the index of dissimilarity for white and black males in New York City. Let b_p and w_b denote the black and white male population count in precinct p, and B and W the total black and white male population count in New York City. The index of dissimilarity is defined as:

$$\mathrm{DI} = \frac{1}{2}\sum_{p=1}^{75}\left|\frac{b_i}{B} - \frac{w_i}{W}\right|$$

When we compute the index of dissimilarity for the black and white male population in New York City, we find that DI = 0.625 or 62.5%. According to the standard interpretation of the index, that implies that 62.5% of the black population in New York City would have to move to another precinct in order to achieve a uniform distribution of whites and blacks across all precincts. That indeed shows that there is a high level of segregation in New York City which might be one contributing factor to the geographical pattern of the stop-and-frisk activity with respect to the black population as seen in 5.2.

Racial profiling strategies do not only take the population share into account but in fact the share of crime committed by one group compared to the other. Unfortunately, the data on reported crime that is available for this study does not provide any demographic information about the offender. Yet, the overall crime rate is still informative in order to understand geographical patterns of the stop-and-frisk activity. Figure 5.4 shows a map of the police precincts by average crime rates. The crime rate here is the weekly number of reported crime per 1,000 male population pooled over the years 2009 to 2014. Again, we categorise the precincts according to the quartiles of the average crime rate distribution. The map suggests that the precincts with the highest shares of stops of blacks are not necessarily congruent with the precincts with the highest crime rates.

Variation over Time

Not only the geographical distribution of police stops and crime rates is of interest here, but also the trends over time. Figure 5.5 shows the number of police stops of black and white individuals, respectively, averaged by week and over all precincts of New York City. The average number of stops of blacks more than doubles between the beginning of 2003 and the end of 2012 with an upward trend throughout. While the average number of stops of whites shows a slight increase over the same time period, there are substantially fewer stops throughout. The left of the two vertical lines in the figure marks the beginning of 2012 when the number of stops of blacks decreases dramatically at the same time as the number of stops of white decreases albeit more moderately. There is anecdotical evidence that this decrease in the stop-and-frisk activity is due to increased criticism of the policy. In August 2013, marked by the right of the two vertical lines, a court decision ruled that the stop-and-frisk practices as carried out by the New York Police Department was unconstitutional and asked for a written policy in order to specify authorised stops (see section (5.2) for details). As shown in figure 5.5, the decision is followed by an immediate kink in the trend of the average number of stops of blacks and whites, respectively. Note that the number of stop-and-frisk encounters is positive even after the judge ruling: The policy was not abolished, but largely scaled down after the judge ruling. In our empirical analysis, we exploit these discontinuities in the stop-and-frisk activity in order to identify the existence and the impact of racial profiling on crime.

Conditional on being stopped, what happens during a police stop? Figure 5.6 to figure 5.9 illustrate the trends in the potential outcomes of stop-and-frisk encounters. The figures show the average weekly number of black and white individuals who are frisked, searched or arrested or who are issued a summons after they have been stopped and questioned by a police officer. The trends for blacks and whites look strikingly similar to the trends of the average number of stops. Naturally, absolute levels differ as not every stop results in the respective outcome. Indeed, only a small fraction of stop-and-frisk encounters lead to an arrest or a summons being issued. Note that if a summons is issued, the individual is assigned a day and time at which he or she has to show up at court. Typically, summons are issued for misdemeanour and not commonly for felony crime, whereas arrests might follow the suspicion of a more serious crime. Arguably, the probability of a crime to have happened is higher when a summons is issued than when an arrest is made.

How do these trends in stop-and-frisk compare to trends in crime? Figure 5.10 and figure 5.11 show the trends in the raw as well as seasonally adjusted reported crime numbers for property and violent crime, respectively. The figures suggest that the crime numbers do not follow the same trends as the stop-and-frisk numbers. In particular, the number of reported crime is almost flat between 2011 and 2014 once seasonal fluctuations are taken into account. The figures suggest that the discontinuities in the stop-and-frisk activity do not translate into discontinuities in crime numbers.

To what extend does the stop-and-frisk strategy contribute to the overall number of arrests in New York City? Table 5.1 shows the total number of arrests and the share of arrested black and white persons, obtained from the NYPD Year End 2008-2014 Enforcement Reports[16], the number of arrest in our stop-and-frisk sample as well as the percentage share of these arrest in the total number of arrests. The numbers show that the annual total number of arrests has rather moderately increased between 2008 and 2014 while the share of black and white individuals being arrested appears to be fairly constant over the years with the exception of a slightly higher share of arrests of whites in 2014. Concerning the share of the number of arrests that have been made following a stop-and-frisk encounter compared to the total number of arrests, there is an increase between 2008 and 2010, a moderate decrease between 2010 and 2012 and a much more dramatic decrease after 2012. These figures are in line with the previous findings and suggest that the stop-and-frisk strategy has been drastically scaled down from 2012 onwards whereas overall crime appears to be much more constant over time.

5.5 Empirical Strategy

In the following, we describe how we test the hypotheses derived in section 5.3 in a regression framework: First, we test the existence of racial bias in the the stop-and-frisk practices by studying the probability across white and black individuals that an individual who is stopped and questioned by a police officer is guilty. Second, we evaluate the stop-and-frisk policy as a police strategy and examine the impact on crime rates in New York City. We pursue a number of empirical strategies which are described in detail in what follows.

5.5.1 Individual-Level Analysis

As outlined in the data description, we exploit two different data sources. In the following, we describe the empirical approach for the individual-level analysis based on the stop-and-frisk data. In that data, potential outcomes of a police stop include frisks, searches, arrests and summons. We do not observe whether the individual has committed crime or is about to commit crime. Hence, arrests and summons following a police stop are approximations for criminal activity that has taken place or is about to take place. Throughout this section, the subscripts i, p, y, w and d denote the individual, precinct, year, week and day of the observed stop. Let t be the calendar date summarising the year, week and day in order to ease notation.

[16] The reports are available online at: http://www.nyc.gov/html/nypd/html/analysis_and_planning/crime_and_enforcement_activity.shtml

Linear Probability Model and Logit Model

We start our analysis with a linear probability model and estimate the effect of being black on the probability that a police stop leads to the individual being frisked, searched, arrested or a summons being issued:

$$Y_{ipt} = \beta_0 + \beta_1 \cdot BLACK_i + \alpha_p + \alpha_y + \alpha_w + \epsilon_{ipt} \tag{5.8}$$

Here, $Y_{ipt} \in \{FRISK_{ipt}, SEARCH_{ipt}, ARREST_{ipt}, SUMMONS_{ipt}\}$ is a binary variable that takes the value 1 if the respective outcome occurs and 0 otherwise. Likewise, $BLACK_i$ is a binary variable that takes the value 1 if the person who is stopped is black, and the value 0 if he is white. We include precinct, calendar year and calendar week fixed effects α_p, α_y and α_w, ϵ_{ipt} is the error term. As an alternative to the linear probability model, we specify a logit model using the same set of outcomes and explanatory variables.

Both models exploit variation over time and space, but do not identify the effect of interest from any exogenous variation. Yet, there may be underlying factors that correlate both with race and the outcomes, as for example income or expected schooling level. In order to identify causal effects we therefore exploit exogenous variation as described in the following.

Regression Discontinuity Design

As discussed above, there is an obvious discontinuity in the stop-and-frisk activity after the judge ruling on August 12, 2013. Stop-and-frisk practices as they were carried out by the NYPD before that date were ruled unconstitutional, and the NYPD was asked to clarify the requirements under which police officers are authorised to stop, question and frisk individuals. We exploit the resulting discontinuity in the number of stop-and-frisk encounters in a regression discontinuity design (RDD). Here, the running variable is the (normalised) calendar date. Let D^* denote the day of the judge ruling. We restrict the estimation window to six weeks before and after the judge ruling. We exploit a sharp discontinuity in stop-and-frisks and estimate the following model:

$$Y_{ipt} = f(DATE_i) + \rho \cdot \mathrm{I}_{[DATE_i \geq D^*]} + \eta_{ipt} \tag{5.9}$$

where $DATE_i$ is the calendar date, $f(\cdot)$ is a polynomial function, I is the indicator function and η_{ipt} is the error term. Arguably the judge ruling led to a sharp discontinuity in the stop-and-frisk practices: The change might be probabilistic rather than deterministic. Therefore, we alternatively estimate a fuzzy RD model.

The fuzzy RD model has the advantage that we can interpret it as an instrumental variable model, where we instrument the treatment $BLACK_i$ with a post-judge ruling dummy $D_i = \mathrm{I}[DATE_i \geq D^*]$.

Event Study Design

A second quasi-experimental approach to retrieve causal estimates is based on a different type of exogenous variation. We exploit events related to police officers being killed in New York City. Information on police officers who were killed in New York City, including the corresponding police precinct, is retrieved from the Officers Down Memorial Page.[17] Here, an event is defined as the killing of one or more police officers. We have 10 such events during our sample period. Let $EVENT_p$ be a binary variable which takes the value 1 if there ever is an event in precinct p, and 0 otherwise. Moreover, let $POST_t$ be a binary variable that takes the value 1 for any date post the event. We restrict the event window to four weeks before and after the event where the coefficients are estimated relative to the non-event precincts five weeks before the event. The following model estimates the average effect of an event happening in a precinct p on the outcome $Y_{ipt} \in \{FRISK_{ipt}, SEARCH_{ipt}, ARREST_{ipt}, SUMMONS_{ipt}\}$:

$$Y_{ipt} = \gamma_0 + \gamma_1 \cdot (EVENT_p \text{ x } POST_t) + \delta_y + \delta_w + \nu_{ipt} \tag{5.10}$$

where δ_y and δ_w denote year and week fixed effects, and u_{ipt} is the error term. This model identifies the average effect of an event in precinct p. Yet, we are further interested in the timing of these effects. In particular, is there a change after the event occurs and is this is very short-term or a longer lasting effect? Thus, we allow the coefficient on the event dummy to vary by week to and after the event $EVENTTIME$:

$$Y_{ipt} = \gamma_0^* + \gamma_1^* \cdot (EVENT_p \text{ x } EVENTTIME_t) + \delta_y^* + \delta_w^* + \nu_{ipt}^* \tag{5.11}$$

The event study design provides additional evidence over and above the local treatment effect of the judge ruling in 2013 which we exploit in the RD design.

One drawback of all the empirical strategies described so far is that we lack data on actual crime, and rely on the approximations arrests and summons. In the following, we describe the precinct level analysis which allows us to estimate the impact of the stop-and-frisk strategy on crime in New York City.

[17] https://www.odmp.org/agency/2758-new-york-city-police-department-new-york

5.5.2 Precinct-Level Analysis

In the stop-and-frisk data, we do not observe whether the individual truly has committed a crime or is about to commit a crime. Hence, we construct a precinct-level panel of reported crime. We aggregate the number of stop-and-frisk encounters to the precinct level and match these to the crime panel. That allows us to study reported crime rates as an outcome variable. In the following, let the crime rate CR_{pt} denote the number of reported crimes per 1,000 male population. Further, let $BSTOP_{pt}$, be the percentage share of stops of blacks and $BPOP_p$ the percentage share of the black population.[18] Accordingly, $BSTOP_{pt}/BPOP_p$ is the share of stops of blacks relative to the black population share. If $BSTOP_{pt}/BPOP_p = 1$, the share of blacks who are stopped compared to whites corresponds to the population share. If $BSTOP_{pt}/BPOP_p > 1$, the share of black who are stopped is over-representative compared to the population share in the police precinct.

Linear Regression Model

We start the precinct-level analysis with a log-linear regression model (OLS) of the crime rate on the share of stops of black individuals. We include fixed effects a_p, a_y and a_w for the precinct, year and week, respectively.

$$\ln CR_{pt} = b_0 + b_1 \cdot \ln BSTOP_{pt} + a_p + a_y + a_w + u_{ipt} \tag{5.12}$$

Alternatively, we use the relative share of stops of blacks to the black population $BSTOP_{pt}/BPOP_p$ as an explanatory variable. In the OLS model, one might be concerned about reversed causalities: On the one hand, if criminals respond to the police behaviour, then the share of black stops should affect the crime rate. On the other hand, if the police responds to the criminals' behaviour, then the crime rate should affect the share of stops of blacks. That would lead to an endogeneity bias in the estimate of b_1. In order to recover causal estimates, we employ three different strategies as described in the following.

Difference-in-Differences

First, we exploit the variation in the number of stop-and-frisk encounters after the judge ruling on August 12, 2013 using a difference-in-differences strategy. Here, we restrict the sample to the time period 2012-2014. Let $POST_t$ be a binary variable that takes the value 1 for all observations after August 12, 2013 and 0 otherwise.

[18] Note that the population shares are constant and do not vary over our sample period. This is due to the fact that we obtained population numbers from the 2010 Decennial Census.

We define the treatment group as precincts in which the relative share of black stops relative to the share of the black population is above the average at the beginning of the sample period t^*:

$$TREAT_p = \mathrm{I}\left[\frac{BSTOP_{pt^*}}{BPOP_p} > \frac{1}{P}\sum_{p=1}^{P}\frac{BSTOP_{pt^*}}{BPOP_p}\right]$$

That means that the treatment group consists of precincts in which initially the black population was over-represented in the number of stop-and-frisks compared to the population share and compared to the relative shares across all precincts. If this is due to racial bias, then one would expect these precincts to be affected by the judge ruling. Accordingly, we estimate the following first stage and reduced form equations:

$$\ln BSTOP_{pt} = b_0^* + b_1^* \cdot TREAT_p + b_2^* \cdot POST_t + b_3^* \cdot (TREAT_p \text{ x } POST_t) + a_p^* + a_y^* + a_w^* + u_{ipt}^* \quad (5.13)$$

$$\ln CR_{pt} = b_0^* + b_1^* \cdot TREAT_p + b_2^* \cdot POST_t + b_3^* \cdot (TREAT_p \text{ x } POST_t) + a_p^* + a_y^* + a_w^* + u_{ipt}^* \quad (5.14)$$

Regression Discontinuity Design

Alternatively, we exploit the same discontinuity in the stop-and-frisk activity but in a regression discontinuity design. The idea is very similar to that in the individual-level analysis. Yet, using precinct-level data allows us to estimate the reduced form effect of the discontinuity on actual crime rates instead of arrests or summons. Hence, we estimate the following model:

$$\ln CR_{pt} = f(DATE_t) + b \cdot \mathrm{I}_{[DATE_t \geq D^*]} + e_{pt} \quad (5.15)$$

Similarly to the individual-level analysis, we first specify a sharp RD model and then estimate a fuzzy RD model which can be interpreted as an instrumental variable model.

Event Study Design

Last, we use an event study design which exploits variation in the stop-and-frisk activity caused by specific events. The advantage of that approach again is that it provides us with additional evidence over and above the local treatment effect of the judge ruling in 2013. While we use the deaths of police officers as events in the individual-level data analysis, we have a slightly different approach here:[19] We use data on individuals who were killed during encounters with the police and define events as the deaths of these individuals.[20] We classify the events into three different categories: A black suspect was killed when he or she attacked one or more police officers (4 events), a black suspect was killed when he or she attacked one or more police officers during a police operation (9 events), or a black suspect was killed during a stop-and-frisk related police operation (12 events).

Again, let $EVENT_p$ and $POST_t$ be binary variables which take the value 1 if there ever is an event in precinct p and for any date post the event, respectively, and 0 otherwise. Again, we restrict the event window to four weeks before and after the event where the coefficients are estimated relative to the non-event precincts five weeks before the event. We estimate the first stage and reduced form effects of the events for the outcomes $Y_{pt} \in \{BSTOP_{pt}, CR_{pt}\}$ as:

$$\ln Y_{pt} = c_0 + c_1 \cdot (EVENT_p \text{ x } POST_t) + d_y + d_w + v_{pt} \tag{5.16}$$

Same as in the individual-level analysis, we are interested not only in the average effect of the events but in particular in the timing: Is there a reaction in terms of stop-and-frisk and crime after a black suspect has been killed? The following model allows us to investigate that question, where $EVENTTIME$ denotes the week to and after the event:

$$\ln Y_{pt} = c_0^* + c_1^* \cdot (EVENT_p \text{ x } EVENTTIME_t) + d_y^* + d_w^* + v_{pt}^* \tag{5.17}$$

5.6 Results

In this section, we discuss the results from our empirical analysis. The results correspond to the estimating equations described in the previous section. First we address the results from the individual-level analysis, before we present the results from the precinct-level analysis.

[19]The sample period for the precinct-level data unfortunately is not long enough to include a sufficient number of events when events are defined as police officers being killed.

[20]Data is retrieved from Brian Burghart's website: www.fatalencounters.org. The events can be verified from news reports.

5.6.1 Individual-Level Analysis

Tables 5.2 to 5.11 show the results from the individual-level estimations. Table 5.2 displays the results for the linear probability model and corresponds to estimating equation (5.8). Conditional on being stopped, the probability of being frisked, searched or arrested positively correlates with being black, the coefficients being statistically significant at the 1% level, whereas the probability of a summons being issued is negatively related to being black. Note that summons typically are issued for minor crime but require the suspect to go to court. In that sense, a summons being issued compared to an arrest being made suggests that indeed a crime has been committed but one which might not be severe enough to arrest the individual. For the outcomes search and arrest the estimates are imprecise once we cluster the standard errors at the precinct level.

Next, we split the analysis by type of crime (property crime, violent crime, sex crime and drug crime) where the type of crime refers to the crime that is suspected when the police officer stops the individual. We find that conditional on being stopped the correlations between the probability of an arrest or a summons and being black are heterogenous across the different categories of crime (see table 5.3). In particular, we find significantly positive associations between the probability of an arrest and being black for property and sex crime, and significantly negative correlations for violent and drug crime. In terms of summons issued, we find negative correlations for all four types of crime.

Table 5.4 shows the estimation results when we specify a logit instead of a linear probability model. The results are in line with the findings above in the sense that they suggest a significantly positive correlation between being frisked and being black, conditional on being stopped, as well as a significantly negative correlation between summons issued and being black.

When we estimate the logit model separately by type of suspected crime (see Table 5.5), we find results which are again in line with the findings from the linear probability model: There is a significantly positive association between the probability of an arrest for property or sex crime and being black, and a significantly negative association between the probability of being arrested for violent or drug crime and being black. For summons issued, we find significantly negative correlations for all types of suspected crime, conditional on being stopped.

Whilst descriptive, these results suggest that conditional on being stopped there are less successful stops of blacks compared to whites for violent and drug crimes and more successful stops of blacks compared to whites for property and sex crimes in terms of arrests. In terms of summons being issued the findings imply less successful stops of blacks compared to whites for all types of crime. At this point we cannot draw causal conclusions, but the descriptive results are interesting for our analysis in two aspects: First, they suggest the hypothesis that police officers screen differently by type of suspected crime. That means that there may be racial profiling for example for violent crime but not so much for other crime types such as sex crime. Second, the fact that the correlation between the probability of a summons being issued and being black is negative conditional on being stopped implies that black individuals might either be stopped based on less conclusive evidence of a crime being committed or for more severe crime that would lead to an arrest rather than to a summons.

In order to retrieve causal effects, we then estimate an RD model as specified in estimating equation (5.9). First, we interpret the discontinuity that followed the judge ruling in August 2013 as a sharp discontinuity. The corresponding results are shown in table 5.6. Column (1) shows the first stage of the RD model, i.e. the change in the probability that a person who is stopped is black. We find only marginally significant increases, yet not robust across different bandwidth choices. Similarly, we do not find any evidence for changes in the probability of being frisked, arrested or summons being issued when we estimate the reduced form. However, we find that the probability of being searched conditional on being stopped increases after the judge ruling. We interpret that as an indication that police officers stopped individuals based on more sophisticated suspicions after the judge ruling than before.[21]

Table 5.7 shows the results when we estimate a fuzzy RD model instead. We interpret the fuzzy RD model as a two-stage least square model. The results do not yield any evidence for causal effects of being black on the probability of being frisked, searched or arrested or a summons being issued.

Last, we exploit variation from a different type of quasi-experiment in an event study design as specified in estimating equations (5.10) and (5.11). The results are shown in table 5.8. The first stage results are found in column (1) and the reduced form results in columns (2) to (7). In terms of the first stage, we find that the probability that the person who is stopped being black is higher in precincts where a police officer was killed. Further, the timing of the effect suggests that this is a causal relationship: Up to the time of the event, the probability of a black compared to a white stop is not significantly higher in the treated precincts relative to non-treated precincts, but becomes significantly higher in week two and three after the event.

[21]Indeed, anecdotical evidence suggest that police officers were uncertain about optimal stop-and-frisk behaviour after the judge ruling and reacted by restricting their stops to those which they knew would be successful and hence non-controversial.

The reduced form effects for the outcomes frisks, searches, arrests and summons yield somewhat mixed evidence. On average, the overall probability of an arrest conditional on a stop decreases in precincts where a police offer was killed. The coefficient is almost identical with the coefficient when we restrict the sample to blacks only, which suggests that the decrease in arrests is due to a decrease in arrests of blacks. There are two possible mechanisms that would yield such a result: First, stops might be less successful. That implies that there are more stop-and-frisk encounters and police officers stop individuals on lower levels of suspicion after an event. Second, crime might go down after an event leading to less arrests given a constant number of stop-and-frisks. For example, that would be the case if there were police deployment effects similar to those found in the literature on policing after terror attacks. Unfortunately, we do not have access to data that would allow us to investigate that in more detail. Yet, the fact that we do not find any impact on the probability of summons being issued favours the first over the second hypothesis.

What about the timing of the negative effect on arrests? While there seems to be a significantly negative effect on arrests in the week just after the event, the results are not yet conclusive as the probability of arrests conditional on being stopped appears to be higher in the treated compared to the non-treated precincts during some weeks before the event, too.

Tables 5.9 to 5.11 show the results for the event study design when we restrict the sample to stop-and-frisk encounters following a suspicion of property, violent and drug crime, respectively. For property crime, we find on average a significant increase in the probability of the person who is stopped being black, yet the timing of the event effect does not appear to support a causal link. Moreover, contrary to the previous analysis the results show that there is a significant increase in the probability of a summons being issued in the week after the event but no impact on the probability of arrests. For violent crime, we find a marginally significant coefficient at the first stage, but no causal impact of the events on the probabilities of being arrested or a summons being issued following a police stop. For drug crime, we find mixed evidence. While the average first stage effect suggests an increase in the probability of a person who is stopped being black, the timing of the effects does not support a causal impact. Yet, the reduced form estimations yield an increase in the probability of being frisked or searched following a police stop two weeks after the event and an increase in the probability of being arrested three weeks after the event.

5.6.2 Precinct-Level Analysis

Tables 5.12 to 5.16 show the results for the precinct-level analysis. We start the analysis with the linear regression model (5.12) relating crime rates to the share and relative share of black individuals who are stopped, frisked or searched. The results are shown in table 5.12. We find significantly positive elasticities of the crime rate with respect to the share of stops of black, both for property and violent crime. Yet, when we cluster the standard errors at the precinct level, the estimates are more noisy. When we estimate the effect of the relative share of stops of blacks, the coefficient is very similar to the coefficient on the absolute share of stops of blacks. This is not surprising given the specification of the estimating equation: The share of the black compared to the white population is constant over the sample period for the reasons explained above, and thus is an additive component in the log-linear regression model. Further, we find a significantly positive elasticity of the property crime rate with respect to the share of black individuals being frisked, and zero effect for violent crime. Elasticities with respect to the share of black individuals being searched are not significantly different from zero.

The linear regression model suffers from the endogeneity problem outlined above and thus the estimates can not be interpreted as causal effects. Our first approach in order to recover causal estimates in the precinct-level analysis is the difference-in-difference model specified in estimating equation (5.14). The results are shown in table 5.13. If the judge ruling in August 2013 stopped previously existing racial profiling strategies, then one would expect that precincts with higher shares of stops of blacks are affected by the judge ruling to a greater extent than other precincts. When we estimate the first stage, we indeed find that the share of stops of black compared to white individuals decreases significantly in the treated precincts compared to the control precincts.

Does the decrease in the share of stops of black individuals translate into changes in crime? If the stop-and-frisk strategy and in particular racial profiling is indeed an efficient police strategy, one would expect crime rates to increase following a decrease in the intensity of the policy. Columns (3) to (8) show the results of the reduced form estimations. We do not find any evidence that the changes after the judge ruling in 2013 affect overall, property or violent crime rates. That means that we do not find any evidence that crime rates increase when the share of black stops decreases, as you would expect in the case of racial profiling being in line with the Broken Windows theory. Thus, the findings from the difference-in-differences model imply that there was indeed a decrease in racial profiling after the judge ruling that did no have any effect on crime rates. One interpretation is that this supports the existence of racial bias before the discontinuity.

One threat to the identification of the difference-in-difference model is whether or not allocation to treatment is random. Thus, in order to provide additional evidence we estimate the RD model described in estimating equation (5.15). The results are shown in table 5.14 and confirm the previous estimation results: We find significantly negative effects on the share of stop-and-frisk encounters of black persons at the first stage, but again no evidence that this translates into changes in crime rates. This is true both for property and for violent crime. When we estimate a fuzzy instead of a sharp RDD model, we find imprecise estimates both for the first stage and for the reduced form (see table 5.15).

Similarly to the individual-level analysis, we further exploit variation from events other than the judge ruling in 2013. The advantage of that approach is that we are able to provide additional quasi-experimental evidence that does not rely on the variation from the judge ruling which contributes to the external validity of our results. In particular, we use an event study design as specified in equations (5.16) and (5.17) and exploit the following events: A black suspect was killed when he or she attacked one or more police officers (event type 1), a black suspect was killed when he or she attacked one or more police officers during a police operation (event type 2), or a black suspect was killed during a stop-and-frisk related police operation (event type 3). The results are shown in table 5.16 where columns (1) and (2) correspond to the first type of events, (3) and (4) to the second type and (5) and (6) to the third type.

For each event type, we estimate the first stage effect on the share of stops of blacks as well as the reduced form effect on the crime rate. For the first type of events, where a black suspect was killed when he or she attacked one or more police officers, there is on average a significant increase in the share of stops of black compared to white individuals in the treated relative to the non-treated precincts. Yet, the timing of the effect suggests that this is not necessarily a causal impact: The share of stops of blacks appears to be significantly higher in all weeks leading up to the event and following the event which suggests a selection of precincts into the treatment group rather than a causal increase in the share of stop-and-frisk encounters for blacks due to the events. For the remaining two types of events, where a black suspect was killed when he or she attacked one or more police officers during a police operation or a black suspect was killed during a stop-and-frisk related police operation, the estimations do not yield any evidence for an impact of the events neither on the share of blacks being stopped nor on the crime rate.

5.7 Conclusion

The stop-and-frisk policy entitles police officers in New York City to stop, question and frisk suspects based on reasonable suspicion of criminal activity. The legal basis for police officers in the U.S. to stop, question and frisk citizens was established in 1968 and legally enacted in New York City, in 1971. Stop-and-frisk became more widely used as a police strategy in the 1990s in order to reduce crime in the city. The policy itself and in particular racial profiling as one of its features has been controversial since then. The political and popular debate about the pros and cons has received a lot of attention from the media.

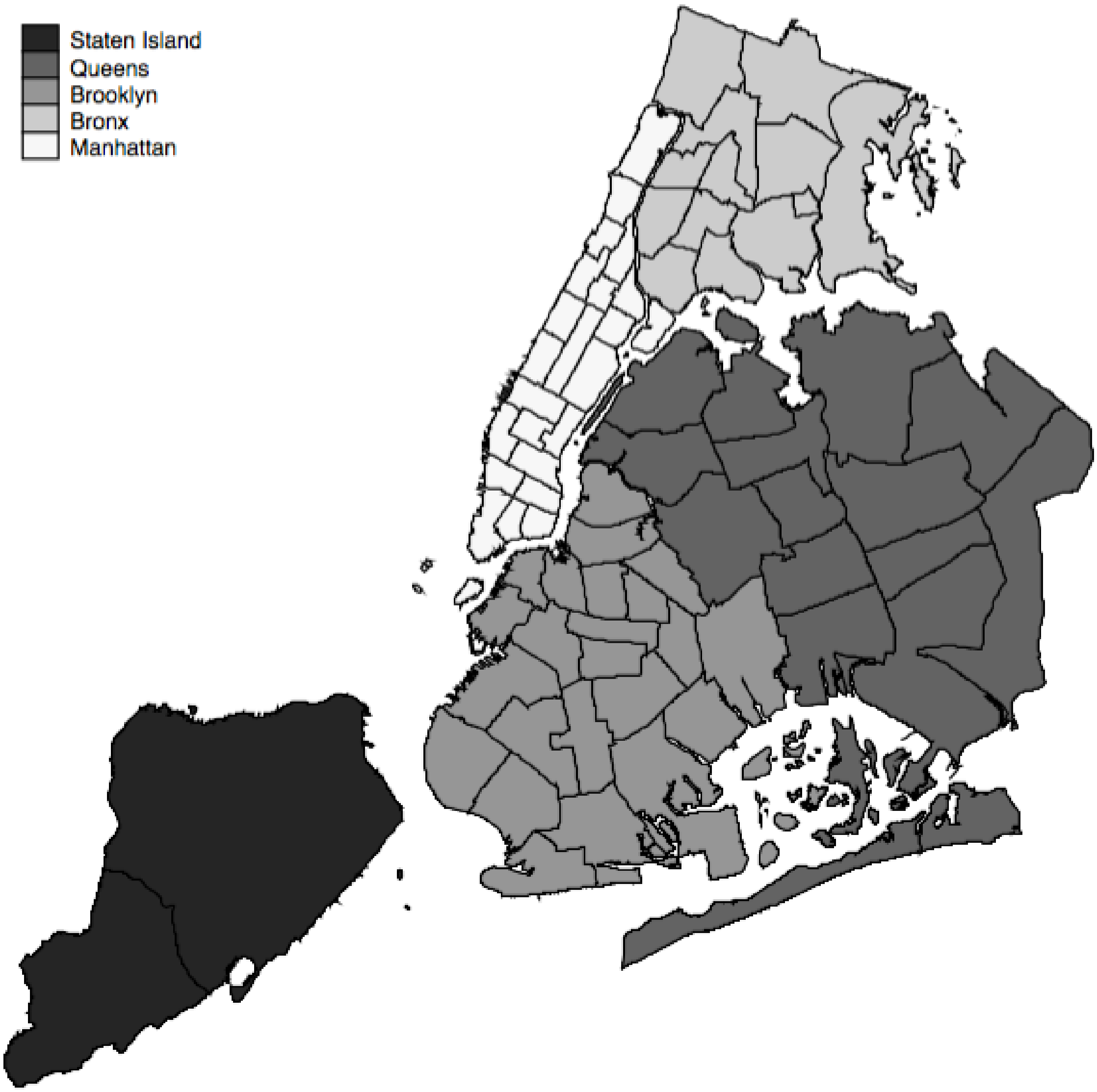
In this chapter, we examine the hypothesis of racial bias in the stop-and-frisk practices in New York City and evaluate the impact on crime. We introduce a theoretical framework of racial bias in police stops and basic welfare considerations. The empirical analysis is based on a number of different strategies in order to identify racial bias in the stop-and-frisk practices and to evaluate the overall impact on crime in New York City. In particular, we base our analysis on detailed individual-level data on stop-and-frisk encounters between 2003 and 2014 as well as precinct-level data on reported crime. For identification, we exploit discontinuities in the stop-and-frisk activities due to a judge ruling in 2013 and due to events related to police officers being killed or black suspects being killed by police officers.

Supporters of the stop-and-frisk policy and racial profiling have used the Broken Windows theory to promote the policy as a police strategy that efficiently targets minor crime and serves as a deterrent more serious crime. The results of our empirical analysis disagree with these arguments in the sense that they suggest the existence of a racial bias in the probability of stop-and-frisk encounters over and above what can be explained by statistical discrimination. Moreover, we find no evidence that these stop-and-frisk practices indeed reduce crime and hence we can not conclude that the policy is an efficient crime deterrence mechanism.

In this chapter, we evaluate a police strategy as a public policy but do not conduct a full welfare analysis. In terms of policy implications, our results imply that the stop-and-frisk policy is not an efficient police strategy. Our analysis does not yet take into account any harm caused to individuals who are innocently stopped by the police. Recent unrests and riots in other U.S. cities, which were triggered by shootings of black suspects and a perception of unjust behaviour of the police towards Afro-Americans, suggest that these considerations are indeed very important and should enter the policy debate.

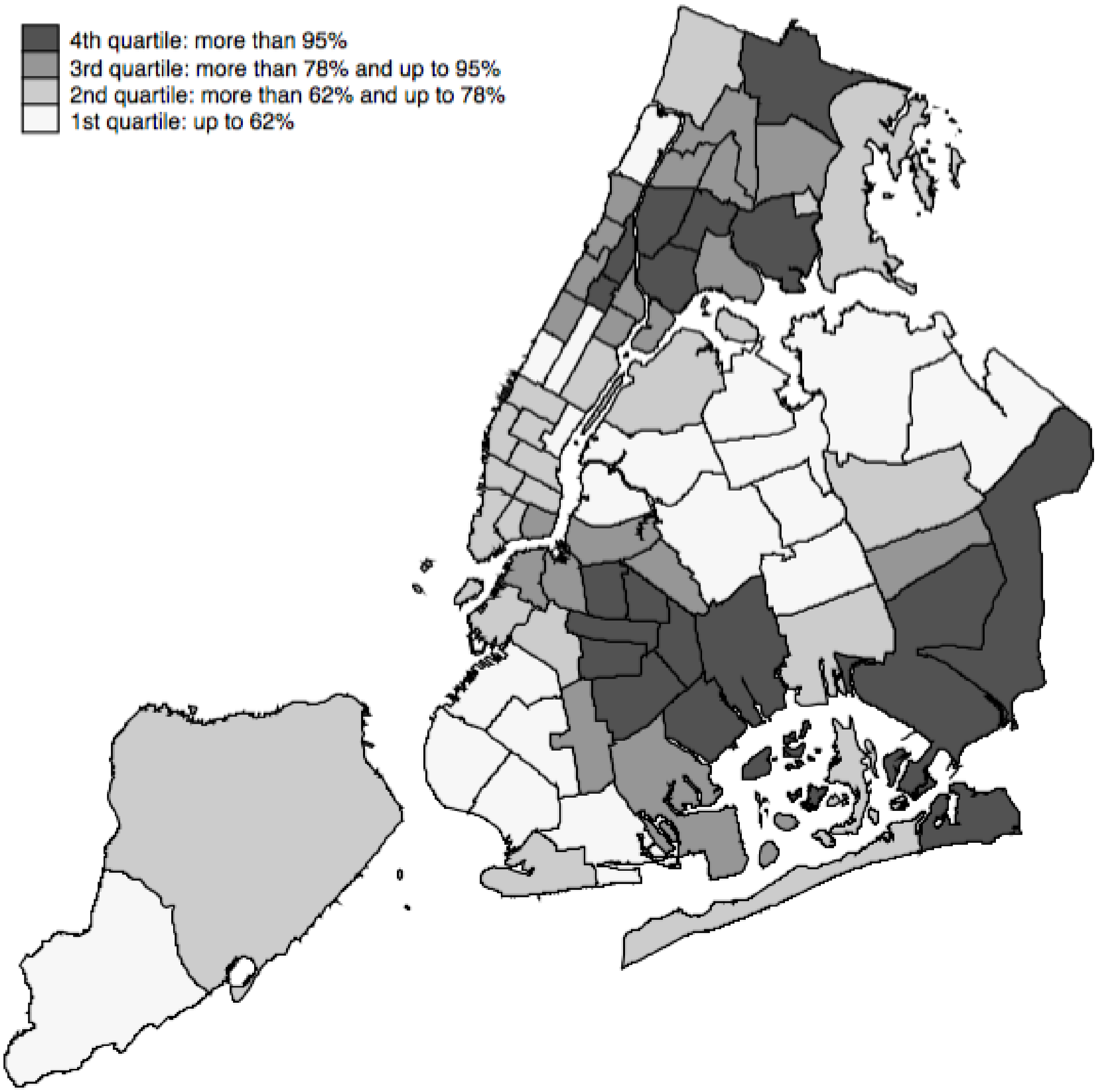
5.8 Figures

Figure 5.1: New York City Police Precincts



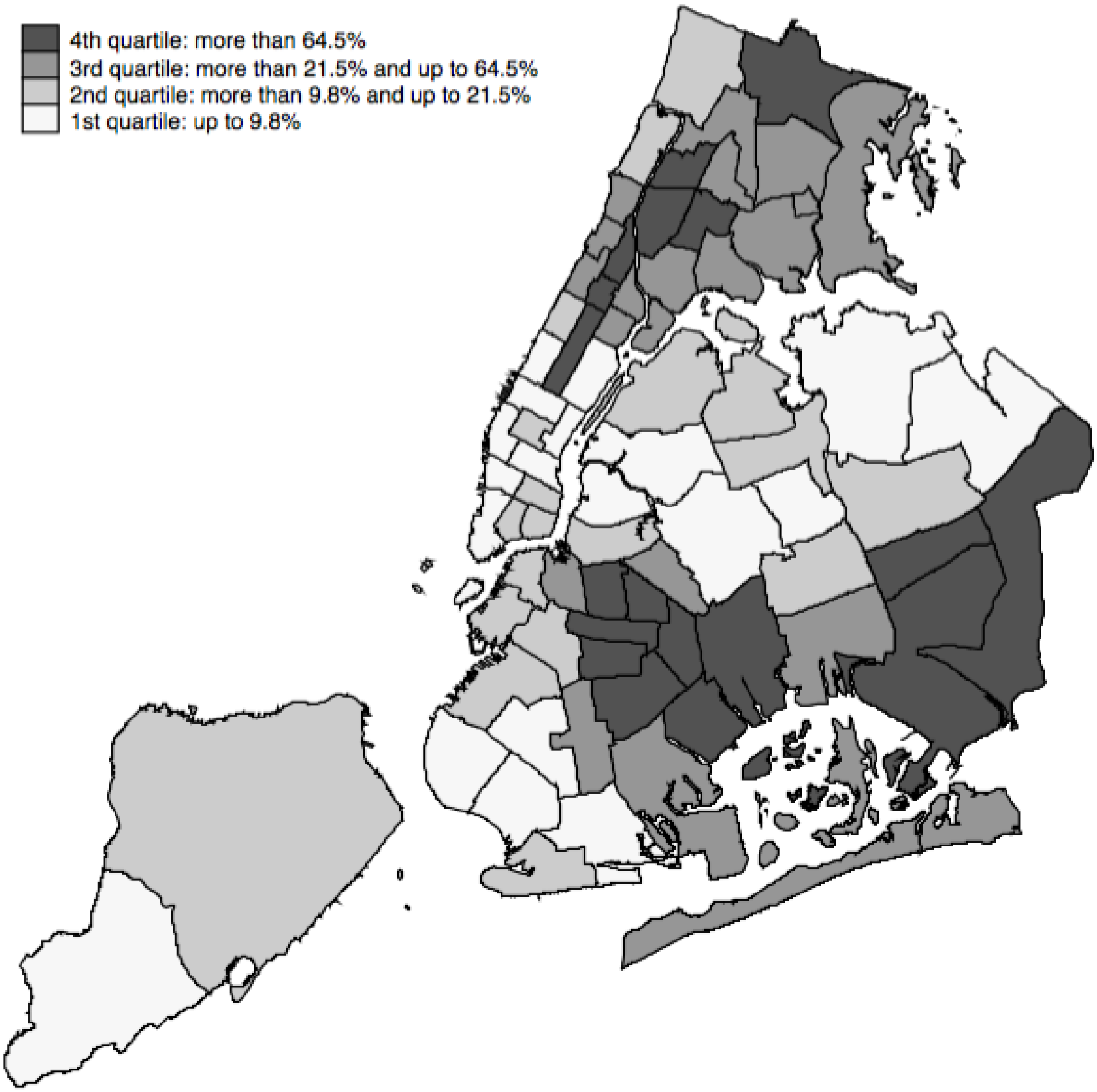
Note: The map shows the 75 police precincts of the estimation sample by borough. We subsume Staten Island's police precincts 120, 121 and 123 to one precinct, see the data description for more details. The five boroughs of New York City include Staten Island, Queens, Brooklyn, Bronx and Manhattan. *Source:* http://johnkeefe.net/nyc-police-precinct-and-census-data and own calculations.

Figure 5.2: Percentage Share of Stop-and-Frisks of Black Individuals



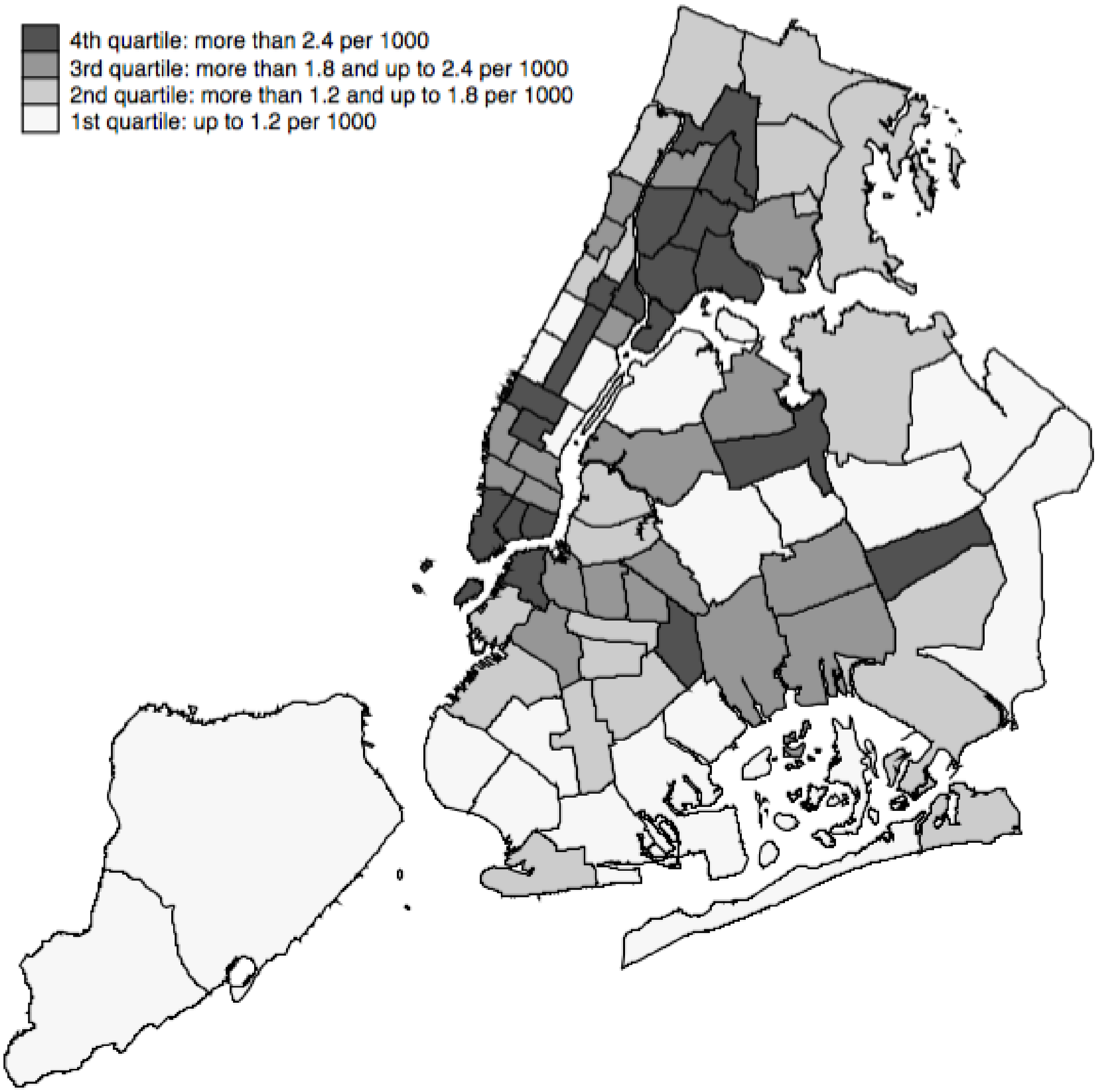
Note: The map shows New York City's police precincts categorised into quartiles of the distribution of the average percentage share of stop-and-frisk encounters involving black compared to white individuals. The average percentage share is computed for each precinct as the average weekly percentage share between 2003 and 2014. *Source:* NYPD Stop, Question and Frisk Database and own calculations.

Figure 5.3: Percentage Share of Black Population



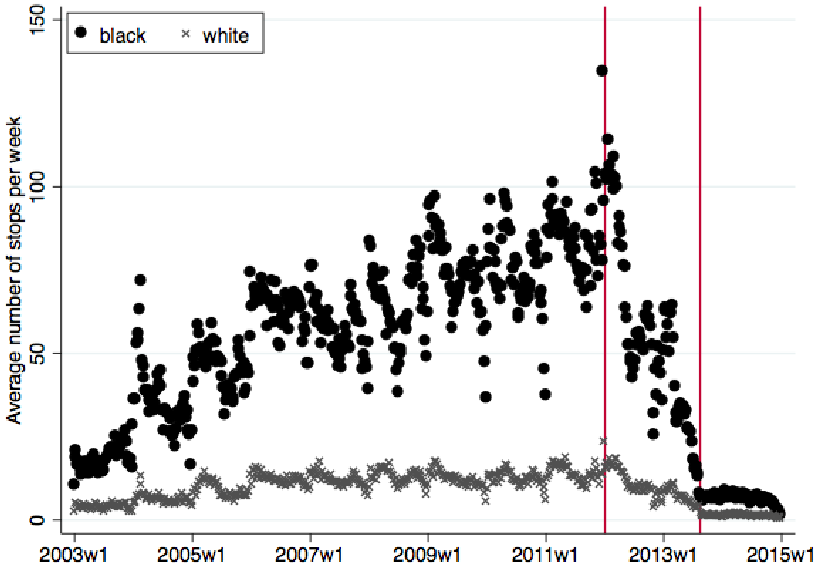
Note: The map shows New York City's police precincts categorised into quartiles of the distribution of the percentage share of black compared to white population. *Source:* 2010 U.S. Decennial Census and own calculations.

Figure 5.4: Reported Crime Rate



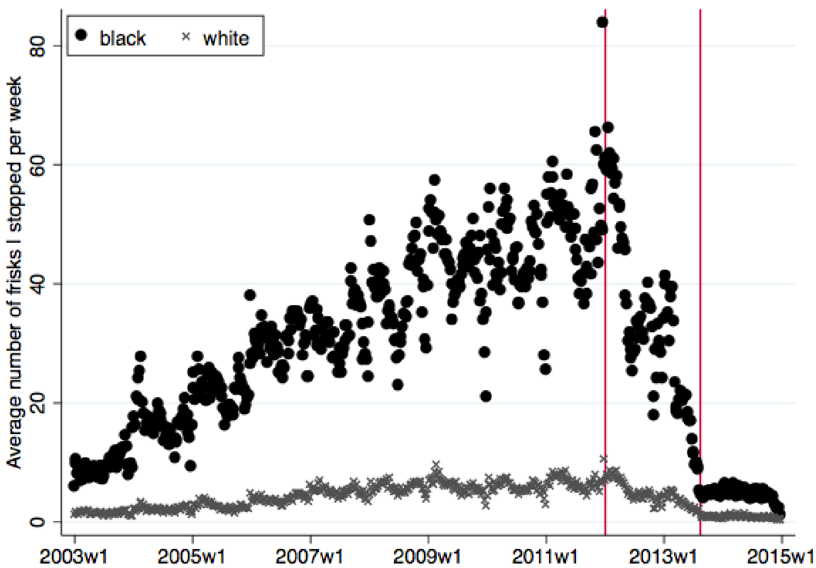
Note: The map shows New York City's police precincts categorised into quartiles of the distribution of the average crime rate. The average crime rate is computed for each precinct as the average weekly number of reported crimes per 1,000 male population between 2009 and 2014. *Source:* NYPD Crime Statistics and own calculations.

Figure 5.5: Stop-and-Frisk Encounters, Stops



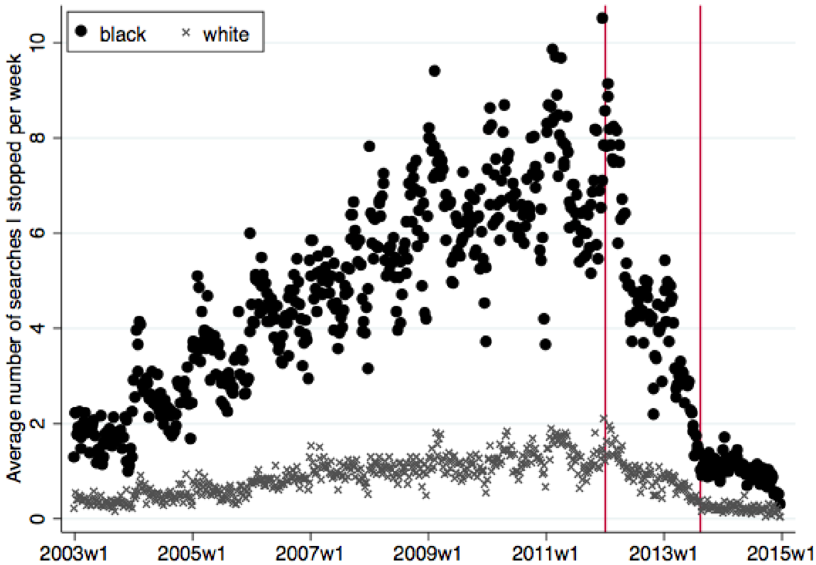
Note: The figure shows the average number of stop-and-frisk encounters between police officers, and black and white individuals, respectively. The average number of encounters is computed for New York City as the the average weekly number of encounters between 2003 and 2014. The vertical lines mark the start of the decline in the stop-and-frisk activity in 2012 and the day of the judge ruling in August 2013 (see the data description for details). *Source:* NYPD Stop, Question and Frisk Database and own calculations.

Figure 5.6: Stop-and-Frisk Encounters, Frisks



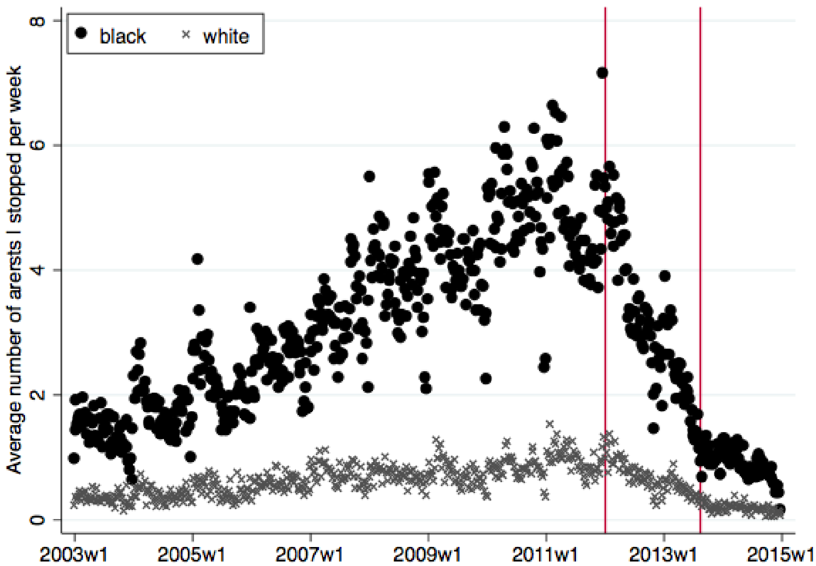
Note: The figure shows the average number of stop-and-frisk encounters during which the suspect was frisked for black and white individuals, respectively. The average number of frisks is computed for New York City as the the average weekly number of frisks between 2003 and 2014. The vertical lines mark the start of the decline in the stop-and-frisk activity in 2012 and the day of the judge ruling in August 2013 (see the data description for details). *Source:* NYPD Stop, Question and Frisk Database and own calculations.

Figure 5.7: Stop-and-Frisk Encounters, Searches



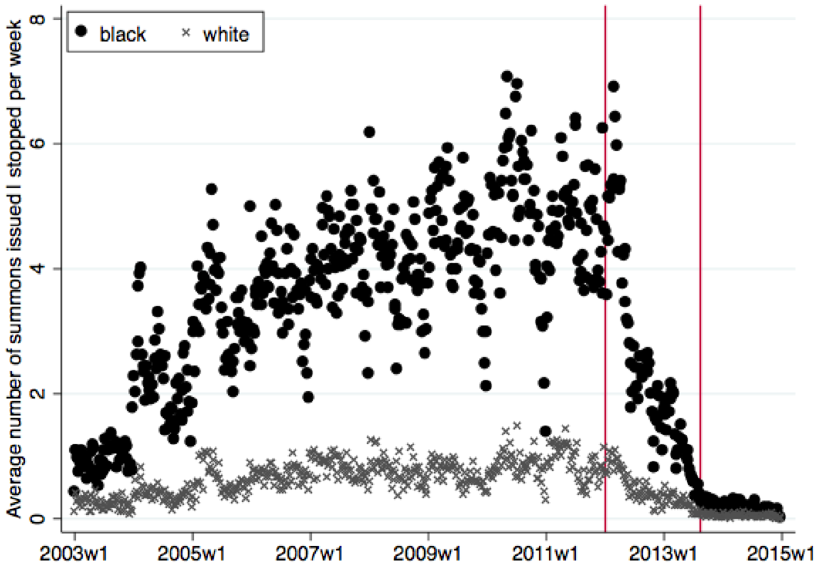
Note: The figure shows the average number of stop-and-frisk encounters during which the suspect was searched for black and white individuals, respectively. The average number of searches is computed for New York City as the the average weekly number of searches between 2003 and 2014. The vertical lines mark the start of the decline in the stop-and-frisk activity in 2012 and the day of the judge ruling in August 2013 (see the data description for details). *Source:* NYPD Stop, Question and Frisk Database and own calculations.

Figure 5.8: Stop-and-frisk Encounters, Arrests



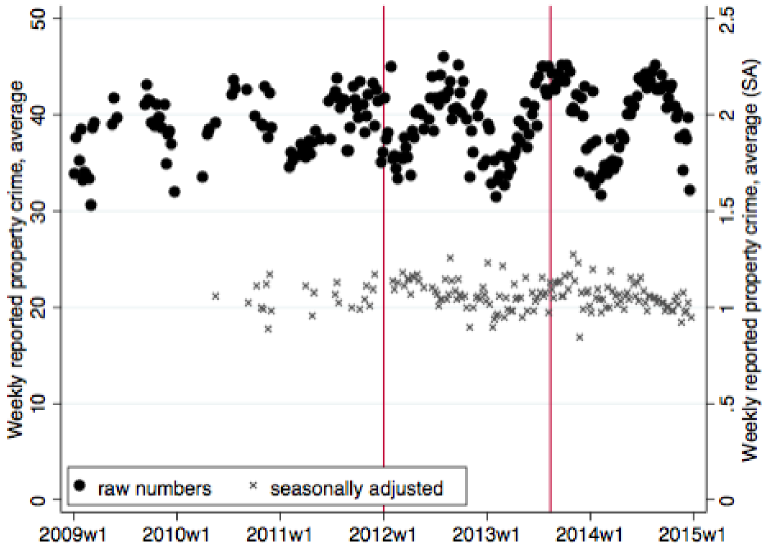
Note: The figure shows the average number of stop-and-frisk encounters during which the suspect was arrested for black and white individuals, respectively. The average number of arrests is computed for New York City as the the average weekly number of arrests between 2003 and 2014. The vertical lines mark the start of the decline in the stop-and-frisk activity in 2012 and the day of the judge ruling in August 2013 (see the data description for details). *Source:* NYPD Stop, Question and Frisk Database and own calculations.

Figure 5.9: Stop-and-Frisk Encounters, Summons



Note: The figure shows the average number of stop-and-frisk encounters during which a summons was issued for black and white individuals, respectively. The average number of summons issued is computed for New York City as the the average weekly number of summons issued between 2003 and 2014. The vertical lines mark the start of the decline in the stop-and-frisk activity in 2012 and the day of the judge ruling in August 2013 (see the data description for details). *Source:* NYPD Stop, Question and Frisk Database and own calculations.

Figure 5.10: Reported Crime, Property Crime



Note: The figure shows the average number of reported property crimes in raw numbers as well as seasonally adjusted. The average number of reported property crimes is computed for New York City as the average weekly number of reported property crimes between 2009 and 2014. Seasonal adjustment is relative to the same week in the previous year. The vertical lines mark the start of the decline in the stop-and-frisk activity in 2012 and the day of the judge ruling in August 2013 (see the data description for details). *Source:* NYPD Crime Statistics and own calculations.

Figure 5.11: Reported Crime, Violent Crime

Note: The figure shows the average number of reported violent crimes in raw numbers as well as seasonally adjusted. The average number of reported violent crimes is computed for New York City as the average weekly number of reported violent crimes between 2009 and 2014. Seasonal adjustment is relative to the same week in the previous year. The vertical lines mark the start of the decline in the stop-and-frisk activity in 2012 and the day of the judge ruling in August 2013 (see the data description for details). *Source:* NYPD Crime Statistics and own calculations.

5.9 Tables

Table 5.1: Total Arrests vs Stop-and-Frisk Arrests

	(1) Total Arrests: All	(2) Total Arrests: % Black	(3) Total Arrests: % White	(4) Stop-and-Frisk: Arrests	(5) Stop-and-Frisk: % of Total
2008	93,962	48.61	12.28	32,206	34.28
2009	93,040	48.23	12.36	34,919	37.53
2010	93,184	48.89	12.12	41,084	44.09
2011	97,104	48.13	12.89	40,084	41.28
2012	97,331	48.55	12.69	32,315	33.20
2013	104,470	47.71	12.29	15,443	14.78
2014	120,539	47.08	13.34	6,898	5.72

Note: The table shows the number of total arrests in New York City for the years 2008-2014 and the percentage shares of blacks and whites of all arrestees, as well as the number of arrests following a stop-and-frisk encounter both as the total number and as a percentage share of all arrests in New York City. *Source:* NYPD End of Year Enforcement Report 2008-2014, NYPD Stop, Question and Frisk Database and own calculations.

Table 5.2: Linear Probability Model

	(1) LPM	(2) LPM	(3) LPM	(4) LPM	(5) LPM	(6) LPM	(7) LPM	(8) LPM
Y=	FRISK	FRISK	SEARCH	SEARCH	ARREST	ARREST	SUMMONS	SUMMONS
BLACK	0.0986***	0.0986***	0.0055***	0.0055	0.0030***	0.0030	-0.0174***	-0.0174***
	(0.0010)	(0.0109)	(0.0006)	(0.0035)	(0.0005)	(0.0025)	(0.0005)	(0.0028)
Precinct FE	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	x	x	x
Week FE	x	x	x	x	x	x	x	x
VCE	robust	cluster	robust	cluster	robust	cluster	robust	cluster
Cluster-Var		precinct		precinct		precinct		precinct
Sample Size	2,852,279	2,852,279	2,852,279	2,852,279	2,852,279	2,852,279	2,852,279	2,852,279

Note: The table shows the regression results corresponding to estimating equation (5.8). The sample runs from 2003 to 2014. All regressions include fixed effects for the police precinct, the calendar year and the calendar week. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Stop, Question and Frisk Database and own calculations.

Table 5.3: Linear Probability Model, Crime Type

	(1) LPM	(2) LPM	(3) LPM	(4) LPM	(5) LPM	(6) LPM	(7) LPM	(8) LPM
Y=	ARREST property	ARREST violent	ARREST sex	ARREST drug	SUMMONS property	SUMMONS violent	SUMMONS sex	SUMMONS drug
BLACK	0.0164***	-0.0140***	0.0354**	-0.0160*	-0.0095**	-0.0254***	-0.0146*	-0.0205**
	(0.0019)	(0.0029)	(0.0122)	(0.0069)	(0.0031)	(0.0051)	(0.0058)	(0.0074)
Precinct FE	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	x	x	x
Week FE	x	x	x	x	x	x	x	x
VCE	cluster	cluster	cluster	cluster	cluster	cluster	cluster	cluster
Cluster-Var	precinct	precinct	precinct	precinct	precinct	precinct	precinct	precinct
Sample Size	637,250	1,177,518	10,906	252,947	637,250	1,177,518	10,906	252,947

Note: The table shows the regression results corresponding to estimating equation (5.8) split by crime type. The sample runs from 2003 to 2014. All regressions include fixed effects for the police precinct, the calendar year and the calendar week. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level.
Source: NYPD Stop, Question and Frisk Database and own calculations.

Table 5.4: Logit Model

	(1) Logit	(2) Logit	(3) Logit	(4) Logit
Y=	FRISK	SEARCH	ARREST	SUMMONS
BLACK	0.4135*** (0.0462)	0.068 (0.0437)	0.051 (0.0437)	-0.3530*** (0.0547)
Precinct FE	x	x	x	x
Year FE	x	x	x	x
Week FE	x	x	x	x
VCE	cluster	cluster	cluster	cluster
Cluster-Var	precinct	precinct	precinct	precinct
Sample Size	2,852,279	2,852,279	2,852,279	2,852,279

Note: The table shows the regression results corresponding to a logit model as described in section 5.5. The sample runs from 2003 to 2014. All regressions include fixed effects for the police precinct, the calendar year and the calendar week. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Stop, Question and Frisk Database and own calculations.

Table 5.5: Logit Model, Crime Type

	(1) Logit	(2) Logit	(3) Logit	(4) Logit	(5) Logit	(6) Logit	(7) Logit	(8) Logit
Y=	ARREST property	ARREST violent	ARREST sex	ARREST drug	SUMMONS property	SUMMONS violent	SUMMONS sex	SUMMONS drug
BLACK	0.3714***	-0.2488***	0.3910***	-0.1580*	-0.2348**	-0.4650***	-0.4223*	-0.3105**
	(0.0434)	(0.0452)	(0.1166)	(0.0617)	(0.0755)	(0.0868)	(0.1743)	(0.1077)
Precinct FE	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	x	x	x
Week FE	x	x	x	x	x	x	x	x
VCE	cluster	cluster	cluster	cluster	cluster	cluster	cluster	cluster
Cluster-Var	precinct	precinct	precinct	precinct	precinct	precinct	precinct	precinct
Sample Size	637,250	1,177,518	10,895	252,947	637,250	1,177,518	10,895	252,947

Note: The table shows the regression results corresponding to a logit model as described in section 5.5 split by crime type. The sample runs from 2003 to 2014. All regressions include fixed effects for the police precinct, the calendar year and the calendar week. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Stop, Question and Frisk Database and own calculations.

Table 5.6: Regression Discontinuity Design, Sharp Discontinuity

	(1) RDD First stage	(2) RDD Reduced form	(3) RDD Reduced form	(4) RDD Reduced form	(5) RDD Reduced form
Y=	BLACK	FRISK	SEARCH	ARREST	SUMMONS
POST	0.0509	-0.0433	0.0526*	0.0198	0.0173
	(0.0299)	(0.0344)	(0.0231)	(0.0227)	(0.0138)
POST	0.0261	-0.0150	0.0623***	0.0364*	0.0174
(half bw)	(0.0190)	(0.0221)	(0.0161)	(0.0155)	(0.0095)
POST	0.0424*	-0.0194	0.0504***	0.0255	0.0099
(double bw)	(0.0201)	(0.0219)	(0.0148)	(0.0145)	(0.0094)
Est.BW	2.26	2.56	2.66	2.63	2.39
Est.BW	1.13	1.28	1.33	1.32	1.20
Est.BW	4.51	5.12	5.32	5.27	4.79
Sample Size	12,974	12,974	12,974	12,974	12,974

Note: The table shows the regression results corresponding to estimating equation (5.9). The sample runs from six weeks before to six weeks after a judge ruling on August 12, 2013. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Stop, Question and Frisk Database and own calculations.

Table 5.7: Regression Discontinuity Design, Fuzzy Discontinuity

	(1) FRD	(2) FRD	(3) FRD	(4) FRD
Y=	FRISK	SEARCH	ARREST	SUMMONS
BLACK	-0.8807 (0.9336)	1.0777 (0.7994)	0.4046 (0.5147)	0.3454 (0.3394)
BLACK (half bw)	-0.5743 (0.9888)	2.3853 (1.8289)	1.3945 (1.1720)	0.6677 (0.6065)
BLACK (double bw)	-0.4732 (0.6004)	1.2723 (0.6887)	0.6398 (0.4634)	0.2337 (0.2439)
Est.BW	2.56	2.66	2.63	2.39
Est.BW	1.28	1.33	1.32	1.20
Est.BW	5.12	5.32	5.27	4.79
Sample Size	12,974	12,974	12,974	12,974

Note: The table shows the regression results corresponding to the fuzzy RD model described in section 5.5. The sample runs from six weeks before to six weeks after a judge ruling on August 12, 2013. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Stop, Question and Frisk Database and own calculations.

Table 5.8: Event Study Design

	(1) ES	(2) ES	(3) ES	(4) ES	(5) ES	(6) ES	(7) ES
Y=	BLACK	FRISK	SEARCH	ARREST all	ARREST blacks	SUMMONS all	SUMMONS blacks
Panel A: Average Effects							
EVENT x POST	0.1078**	0.0168	-0.0164	-0.0246**	-0.0248**	-0.0106	-0.0131
	(0.0347)	(0.0415)	(0.0142)	(0.0091)	(0.0086)	(0.0079)	(0.0087)
Panel B: Timing of Effects							
EVENT x (-5)	0.0625	-0.0257	-0.0309*	-0.0128	-0.0116	-0.0043	-0.0083
	(0.0405)	(0.0444)	(0.0127)	(0.0151)	(0.0164)	(0.0103)	(0.0126)
EVENT x (-4)	0.0565	-0.0365	-0.0001	-0.0227*	-0.0244*	0.0058	0.0064
	(0.0399)	(0.0584)	(0.0127)	(0.0113)	(0.0113)	(0.0111)	(0.0114)
EVENT x (-3)	0.0672	0.0010	-0.0005	-0.0242***	-0.0230**	0.0075	0.0004
	(0.0425)	(0.0429)	(0.0199)	(0.0070)	(0.0080)	(0.0106)	(0.0124)
EVENT x (-2)	0.0708	0.0106	-0.0295*	-0.0262*	-0.0241*	-0.0116	-0.0132
	(0.0497)	(0.0359)	(0.0112)	(0.0107)	(0.0103)	(0.0087)	(0.0098)
EVENT x (-1)	0.0628	0.0952*	0.0188	0.0002	-0.0048	0.0173	0.0127
	(0.0484)	(0.0464)	(0.0215)	(0.0124)	(0.0102)	(0.0164)	(0.0176)
EVENT	0.0631	0.0987	0.0337	-0.0081	-0.0055	0.0033	-0.0031
	(0.0450)	(0.0545)	(0.0173)	(0.0129)	(0.0135)	(0.0194)	(0.0207)
EVENT x (+1)	0.0776	0.0800	-0.0181	-0.0332**	-0.0298*	0.0130	0.0042
	(0.0449)	(0.0402)	(0.0144)	(0.0110)	(0.0118)	(0.0116)	(0.0138)
EVENT x (+2)	0.1098*	0.1057*	-0.0037	-0.0021	-0.0033	0.0086	0.0013
	(0.0477)	(0.0468)	(0.0232)	(0.0170)	(0.0174)	(0.0168)	(0.0171)
EVENT x (+3)	0.1097**	0.1213	0.0271	-0.0014	0.0034	0.0177	0.0122
	(0.0405)	(0.0635)	(0.0199)	(0.0187)	(0.0191)	(0.0128)	(0.0142)
EVENT x (+4)	0.0591	0.0602	0.0056	-0.0089	-0.0063	0.0125	0.0108
	(0.0371)	(0.0627)	(0.0163)	(0.0122)	(0.0127)	(0.0129)	(0.0146)
Year FE	x	x	x	x	x	x	x
Week FE	x	x	x	x	x	x	x
VCE	cluster	cluster	cluster	cluster	cluster	cluster	cluster
Cluster-Var	precinct	precinct	precinct	precinct	precinct	precinct	precinct
Sample Size	322,051	322,051	322,051	322,051	267,994	322,051	267,994

Note: The table shows the regression results corresponding to estimating equations (5.10) and (5.11). The sample runs from 2003 to 2014. All regressions include fixed effects for the calendar year and the calendar week. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Stop, Question and Frisk Database, Officers Down Memorial Page and own calculations.

Table 5.9: Event Study Design, Property Crime

	(1) ES	(2) ES	(3) ES	(4) ES	(5) ES	(6) ES	(7) ES
Y=	BLACK	FRISK	SEARCH	ARREST all	ARREST blacks	SUMMONS all	SUMMONS blacks
Panel A: Average Effects							
EVENT x POST	0.2208***	-0.0563	-0.0252*	-0.0268**	-0.0285**	0.0187	0.0173
	(0.0386)	(0.0649)	(0.0098)	(0.0079)	(0.0090)	(0.0196)	(0.0175)
Panel B: Timing of Effects							
EVENT x (-5)	0.1916***	-0.0051	-0.0460***	-0.0341**	-0.0389**	0.0298	0.0320
	(0.0480)	(0.0511)	(0.0108)	(0.0102)	(0.0121)	(0.0397)	(0.0418)
EVENT x (-4)	0.1314	-0.1525	-0.0146	-0.0205	-0.0245	-0.0017	-0.0044
	(0.0740)	(0.0885)	(0.0242)	(0.0234)	(0.0251)	(0.0299)	(0.0302)
EVENT x (-3)	0.1932**	-0.0913	-0.0340	-0.0593***	-0.0752***	0.0579*	0.0689*
	(0.0615)	(0.0835)	(0.0187)	(0.0118)	(0.0134)	(0.0286)	(0.0324)
EVENT x (-2)	0.1216	-0.1006*	-0.0668**	-0.0311	-0.0396	-0.0047	-0.0037
	(0.0689)	(0.0502)	(0.0226)	(0.0204)	(0.0225)	(0.0238)	(0.0245)
EVENT x (-1)	0.1297	-0.0370	-0.0457*	-0.0144	-0.0276	0.0745*	0.0859*
	(0.0974)	(0.0702)	(0.0186)	(0.0192)	(0.0169)	(0.0338)	(0.0392)
EVENT	0.1281	0.1497	0.0173	0.0073	-0.0019	0.0396	0.0547
	(0.0783)	(0.1085)	(0.0330)	(0.0236)	(0.0247)	(0.0427)	(0.0444)
EVENT x (+1)	0.0818	0.0361	-0.0234	-0.0041	-0.0143	0.1056**	0.0993**
	(0.0735)	(0.1028)	(0.0341)	(0.0257)	(0.0291)	(0.0317)	(0.0315)
EVENT x (+2)	0.0042	-0.0077	-0.0694**	0.2108	-0.0498	0.2651	0.3620
	(0.2356)	(0.1819)	(0.0240)	(0.1508)	(0.0287)	(0.2292)	(0.2694)
EVENT x (+3)	0.0864	0.1259	0.0109	0.0176	-0.0001	0.0367	0.0334
	(0.1005)	(0.1129)	(0.0435)	(0.0339)	(0.0389)	(0.0248)	(0.0255)
EVENT x (+4)	0.2084*	0.0307	-0.0071	-0.0131	-0.0096	0.0243	0.0378*
	(0.0930)	(0.1445)	(0.0124)	(0.0092)	(0.0124)	(0.0171)	(0.0173)
Year FE	x	x	x	x	x	x	x
Week FE	x	x	x	x	x	x	x
VCE	cluster	cluster	cluster	cluster	cluster	cluster	cluster
Cluster-Var	precinct	precinct	precinct	precinct	precinct	precinct	precinct
Sample Size	65,701	65,701	65,701	65,701	46,230	65,701	46,230

Note: The table shows the regression results corresponding to estimating equations (5.10) and (5.11) for property crime only. The sample runs from 2003 to 2014. All regressions include fixed effects for the calendar year and the calendar week. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Stop, Question and Frisk Database, Officers Down Memorial Page and own calculations.

Table 5.10: Event Study Design, Violent Crime

	(1) ES	(2) ES	(3) ES	(4) ES	(5) ES	(6) ES	(7) ES
Y=	BLACK	FRISK	SEARCH	ARREST all	ARREST blacks	SUMMONS all	SUMMONS blacks
Panel A: Average Effects							
EVENT x POST	0.0670***	-0.0106	-0.0177	-0.0163	-0.0176*	-0.0139*	-0.0148
	(0.0188)	(0.0307)	(0.0194)	(0.0095)	(0.0074)	(0.0068)	(0.0074)
Panel B: Timing of Effects							
EVENT x (-5)	0.0313	-0.0858	-0.0383***	-0.0123	-0.0079	-0.0004	-0.0048
	(0.0253)	(0.0519)	(0.0090)	(0.0118)	(0.0125)	(0.0102)	(0.0083)
EVENT x (-4)	-0.0031	-0.0807	0.0243	0.0005	-0.0012	-0.0143	-0.0161
	(0.0443)	(0.0578)	(0.0201)	(0.0155)	(0.0113)	(0.0181)	(0.0198)
EVENT x (-3)	0.0453	0.0102	-0.0177	-0.0120	-0.0073	-0.0477*	-0.0521*
	(0.0271)	(0.0433)	(0.0201)	(0.0128)	(0.0132)	(0.0215)	(0.0217)
EVENT x (-2)	0.0790**	-0.0192	-0.0109	-0.0196	-0.0157	-0.0593*	-0.0607*
	(0.0285)	(0.0441)	(0.0188)	(0.0111)	(0.0112)	(0.0236)	(0.0246)
EVENT x (-1)	0.0849*	0.0532	0.0478	0.0173	0.0200	-0.0432	-0.0448
	(0.0384)	(0.0478)	(0.0271)	(0.0224)	(0.0210)	(0.0347)	(0.0352)
EVENT x	0.0563	0.0999	0.0890*	0.0105	0.0101	-0.0615	-0.0611
	(0.0354)	(0.0578)	(0.0384)	(0.0201)	(0.0200)	(0.0578)	(0.0599)
EVENT x (+1)	0.0639*	0.0393	0.0312	-0.0121	-0.0087	-0.0939	-0.0939
	(0.0282)	(0.0380)	(0.0202)	(0.0145)	(0.0145)	(0.0539)	(0.0555)
EVENT x (+2)	0.0865	-0.1131	0.0707	-0.0238	-0.0206	0.0792	0.0413
	(0.0489)	(0.1979)	(0.0704)	(0.0235)	(0.0240)	(0.0481)	(0.0416)
EVENT x (+3)	0.0471	0.0654	0.1063***	0.0104	0.0105	-0.1128	-0.1102
	(0.0334)	(0.0633)	(0.0274)	(0.0179)	(0.0162)	(0.0667)	(0.0679)
EVENT x (+4)	0.0583*	0.0226	0.0000	-0.0158	-0.0194	0.0871	0.0889
	(0.0292)	(0.1085)	(0.0250)	(0.0130)	(0.0107)	(0.0525)	(0.0528)
Year FE	x	x	x	x	x	x	x
Week FE	x	x	x	x	x	x	x
VCE	cluster	cluster	cluster	cluster	cluster	cluster	cluster
Cluster-Var	precinct	precinct	precinct	precinct	precinct	precinct	precinct
Sample Size	118,794	118,794	118,794	118,794	108,146	118,794	108,146

Note: The table shows the regression results corresponding to estimating equations (5.10) and (5.11) for violent crime. The sample runs from 2003 to 2014. All regressions include fixed effects for the calendar year and the calendar week. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Stop, Question and Frisk Database, Officers Down Memorial Page and own calculations.

Table 5.11: Event Study Design, Drug Crime

	(1) ES	(2) ES	(3) ES	(4) ES	(5) ES	(6) ES	(7) ES
Y=	BLACK	FRISK	SEARCH	ARREST all	ARREST blacks	SUMMONS all	SUMMONS blacks
Panel A: Average Effects							
EVENT x POST	0.1095***	-0.0042	-0.0167	-0.0083	-0.0056	-0.0183	-0.0276
	(0.0303)	(0.0444)	(0.0219)	(0.0166)	(0.0174)	(0.0139)	(0.0165)
Panel B: Timing of Effects							
EVENT x (-5)	0.0348	-0.0034	0.0399	0.0787	0.0740	-0.0791***	-0.0772***
	(0.0559)	(0.0672)	(0.0680)	(0.0596)	(0.0467)	(0.0099)	(0.0099)
EVENT x (-4)	0.0609	0.0005	0.1666	0.2308*	0.2294*	0.0510	0.0563
	(0.0597)	(0.1261)	(0.0934)	(0.1111)	(0.1070)	(0.0525)	(0.0571)
EVENT x (-3)	0.0190	-0.1136	-0.0652	-0.0524	-0.0331	0.0132	0.0241
	(0.0976)	(0.0799)	(0.0440)	(0.0741)	(0.0817)	(0.0342)	(0.0354)
EVENT x (-2)	-0.0339	-0.1565	-0.0346	0.0144	0.0201	-0.0194	-0.0119
	(0.1071)	(0.1113)	(0.0431)	(0.0473)	(0.0456)	(0.0315)	(0.0317)
EVENT x (-1)	0.0819	-0.0065	-0.0449	0.0374	0.0160	0.0347	0.0346
	(0.0663)	(0.1131)	(0.0442)	(0.0469)	(0.0491)	(0.0380)	(0.0351)
EVENT	0.0523	0.1766	0.0755	0.0337	0.0236	0.0327	0.0458
	(0.0722)	(0.1606)	(0.0613)	(0.0574)	(0.0546)	(0.0465)	(0.0439)
EVENT x (+1)	-0.0333	0.0027	0.0455	0.0640	0.0270	0.0538	0.0609
	(0.0822)	(0.2074)	(0.0695)	(0.0787)	(0.0641)	(0.0519)	(0.0511)
EVENT x (+2)	0.1146	0.7773***	0.6680***	0.0367	0.0336	0.1005	0.1175
	(0.0943)	(0.1409)	(0.0643)	(0.0657)	(0.0567)	(0.0808)	(0.0800)
EVENT x (+3)	-0.0236	0.0999	0.1132	0.1572**	0.1360**	0.0604	0.0652
	(0.0931)	(0.2646)	(0.0823)	(0.0574)	(0.0488)	(0.0649)	(0.0645)
EVENT x (+4)	0.0405	0.1404	-0.0152	-0.0156	0.0010	0.0300	-0.0077
	(0.0529)	(0.1920)	(0.0328)	(0.0403)	(0.0403)	(0.0481)	(0.0373)
Year FE	x	x	x	x	x	x	x
Week FE	x	x	x	x	x	x	x
VCE	cluster	cluster	cluster	cluster	cluster	cluster	cluster
Cluster-Var	precinct	precinct	precinct	precinct	precinct	precinct	precinct
Sample Size	26,235	26,235	26,235	26,235	22,170	26,235	22,170

Note: The table shows the regression results corresponding to estimating equations (5.10) and (5.11) for drug crime. The sample runs from 2003 to 2014. All regressions include fixed effects for the calendar year and the calendar week. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Stop, Question and Frisk Database, Officers Down Memorial Page and own calculations.

Table 5.12: Linear Regression Model

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7) OLS	(8) OLS	(9) OLS
Y=	logCR	logCR	logCR	logPCR	logVCR	logPCR	logVCR	logPCR	logVCR
logBSTOP	0.0105***		0.0105	0.0130***	0.0089*				
	(0.0031)		(0.0087)	(0.0035)	(0.0041)				
logBSTOP/BPOP		0.0107***							
		(0.0031)							
logBFRISK						0.0063**	0.0020		
						(0.0022)	(0.0030)		
logBSEARCH								0.0010	0.0008
								(0.0007)	(0.0012)
Precinct FE	x	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	x	x	x	x
Week FE	x	x	x	x	x	x	x	x	x
VCE	robust	robust	cluster	robust	robust	robust	robust	robust	robust
Cluster-Var			precinct						
Sample Size	16,281	16,281	16,281	16,281	16,281	16,281	16,281	16,281	16,281

Note: The table shows the regression results corresponding to estimating equation (5.12). The sample runs from 2009 to 2014. All regressions include fixed effects for the police precinct, the calendar year and the calendar week. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Crime Statistics, NYPD Stop, Question and Frisk Database and own calculations.

Table 5.13: Difference-in-Differences Model

	(1) DiD First stage	(2) DiD First Stage	(3) DiD First Stage	(4) DiD Reduced form	(5) DiD Reduced form	(6) DiD Reduced form	(7) DiD Reduced form	(8) DiD Reduced form
Y=	logBSTOP	logBSTOP	logBSTOP	logCR	logCR	logCR	logPCR	logVCR
TREAT	-0.4864***	-2.3854***	-2.3854***	-0.0693**	0.9004***	0.9004***	1.2096***	-0.0102
	(0.0524)	(0.4079)	(0.2451)	(0.0221)	(0.0212)	(0.0078)	(0.0217)	(0.1041)
POST	-1.8136***	-0.5418***	-0.5418	-0.0034	0.0624**	0.0624***	0.0643*	0.1153**
	(0.0832)	(0.1543)	(0.3041)	(0.0198)	(0.0223)	(0.0120)	(0.0279)	(0.0366)
TREAT x POST	-1.3577***	-1.3521***	-1.3521*	0.0005	0.0042	0.0042	-0.0086	-0.0251
	(0.1751)	(0.1590)	(0.5165)	(0.0318)	(0.0152)	(0.0169)	(0.0183)	(0.0255)
Precinct FE	-	x	x	-	x	x	x	x
Year FE	-	x	x	-	x	x	x	x
Week FE	-	x	x	-	x	x	x	x
VCE	robust	robust	cluster	robust	robust	cluster	robust	robust
Cluster-Var			precinct			precinct		
Sample Size	11,260	11,260	11,260	11,260	11,260	11,260	11,260	11,260

Note: The table shows the regression results corresponding to estimating equation (5.14). The sample runs from 2009 to 2014. Regressions include fixed effects for the police precinct, the calendar year and the calendar week where indicated. The treatment group includes precincts with higher than average relative shares of stops of black individuals before a judge ruling on August 12, 2013. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Crime Statistics, NYPD Stop, Question and Frisk Database and own calculations.

Table 5.14: Regression Discontinuity Design, Sharp Discontinuity

	(1) RDD First stage	(2) RDD First stage	(3) RDD Reduced form	(4) RDD Reduced form	(5) RDD Reduced form
Y=	logBSTOP	logBFRISK	logCR	logPCR	logVCR
POST	-1.2691* (0.5275)	-1.0340 (1.0418)	0.0947 (0.1649)	0.0939 (0.1734)	0.3251 (0.2414)
POST (half bw)	-1.8849* (0.8281)	-0.8000 (0.7890)	0.0673 (0.1333)	0.0588 (0.1408)	0.2330 (0.1799)
POST (double bw)	-1.2630** (0.4817)	-1.4653* (0.7104)	0.0424 (0.1122)	0.0194 (0.1175)	0.1929 (0.1509)
Est.BW	5.33	3.27	3.76	3.80	3.99
Est.BW	2.67	1.63	1.88	1.90	1.99
Est.BW	10.67	6.53	7.52	7.61	7.98
Sample Size	975	975	975	975	975

Note: The table shows the regression results corresponding to estimating equation (5.15). The sample runs from six weeks before to six weeks after a judge ruling on August 12, 2013. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Crime Statistics, NYPD Stop, Question and Frisk Database and own calculations.

Table 5.15: Regression Discontinuity Design, Fuzzy Discontinuity

	(1) FRD	(2) FRD	(3) FRD	(4) FRD	(5) FRD	(6) FRD
Y=	logCR	logCR	logPCR	logPCR	logVCR	logVCR
logBSTOP	-0.0652 (0.1179)		-0.0649 (0.1229)		-0.2272 (0.1999)	
logBSTOP (half bw)	-0.0539 (0.1068)		-0.0471 (0.1119)		-0.1865 (0.1625)	
logBSTOP (double bw)	-0.0337 (0.0905)		-0.0154 (0.0934)		-0.1531 (0.1376)	
logBFRISK		-0.0952 (0.1990)		-0.0944 (0.2019)		-0.3274 (0.4239)
logBFRISK (half bw)		-0.0842 (0.1854)		-0.0735 (0.1877)		-0.2912 (0.3666)
logBFRISK (double bw)		-0.0276 (0.0744)		-0.0126 (0.0763)		-0.1240 (0.1154)
Est.BW	3.76	3.76	3.80	3.80	3.99	3.99
Est.BW	1.88	1.88	1.90	1.90	1.99	1.99
Est.BW	7.52	7.52	7.61	7.61	7.98	7.98
Sample Size	975	975	975	975	975	975

Note: The table shows the regression results corresponding to the fuzzy RD model described in section 5.5. The sample runs from six weeks before to six weeks after a judge ruling on August 12, 2013. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Crime Statistics, NYPD Stop, Question and Frisk Database and own calculations.

Table 5.16: Event Study Design

	(1) ES	(2) ES	(3) ES	(4) ES	(5) ES	(6) ES
	Event Type 1		Event Type 2		Event Type 3	
Y=	logBSTOP	logCR	logBSTOP	logCR	logBSTOP	logCR
Panel A: Average Effects						
EVENT x POST	2.3598**	0.1452	0.9843	0.0148	1.2371*	0.1074
	(0.6585)	(0.2154)	(0.6799)	(0.2685)	(0.5703)	(0.2261)
Panel B: Timing of Effects						
EVENT x (-5)	2.4347**	0.2270	-1.0908	0.4083	1.2036*	0.1299
	(0.7993)	(0.2181)	(2.2049)	(0.4419)	(0.5180)	(0.4028)
EVENT x (-4)	3.0331*	0.0726	-2.6121	0.1022	-0.7992	0.1790
	(1.1700)	(0.2564)	(2.8330)	(0.4965)	(1.4730)	(0.3103)
EVENT x (-3)	2.4303**	0.1402	0.3032	0.1403	-0.0974	0.2075
	(0.7986)	(0.2535)	(2.2586)	(0.4414)	(1.6012)	(0.3433)
EVENT x (-2)	2.3186**	0.1141	-0.3765	0.0510	0.1780	0.0103
	(0.7509)	(0.2406)	(1.8409)	(0.3879)	(1.3835)	(0.2895)
EVENT x (-1)	2.4021**	0.1126	1.6497	-0.0148	1.2141	0.0600
	(0.7835)	(0.2651)	(1.0570)	(0.4244)	(0.6917)	(0.3254)
EVENT	2.3925**	0.2066	0.2289	-0.0259	0.0668	0.0987
	(0.7798)	(0.2215)	(1.8403)	(0.4368)	(1.3802)	(0.3087)
EVENT x (+1)	2.3316**	0.0852	0.5801	0.0905	0.6204	0.0103
	(0.7940)	(0.2274)	(1.0151)	(0.3257)	(0.6941)	(0.3361)
EVENT x (+2)	2.3696**	0.0444	2.6333*	-0.3796	1.0409	-0.3095
	(0.7795)	(0.2925)	(1.1667)	(0.4180)	(0.6512)	(0.3681)
EVENT x (+3)	2.4173**	0.0285	1.8097	-0.1631	1.0197	0.0326
	(0.8081)	(0.2504)	(1.1097)	(0.3629)	(0.6236)	(0.2580)
EVENT x (+4)	2.1992**	0.1037	1.2385	-0.1395	0.8176	-0.0331
	(0.7362)	(0.2403)	(0.8779)	(0.3437)	(0.6464)	(0.3122)
Year FE	x	x	x	x	x	x
Week FE	-	-	x	x	x	x
VCE	cluster	cluster	cluster	cluster	cluster	cluster
Cluster-Var	precinct	precinct	precinct	precinct	precinct	precinct
Sample Size	1092	1092	1632	1632	2579	2579

Note: The table shows the regression results corresponding to estimating equation (5.16) and (5.17). The sample runs from 2009 to 2014. Regressions include fixed effects for the police precinct, the calendar year and the calendar week where indicated. Standard errors are shown in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; *** indicates significance at the 1 percent level. *Source:* NYPD Crime Statistics, NYPD Stop, Question and Frisk Database, Fatal Encounters online database and own calculations.

Appendix

Figure 5A.1: UF-250 Form

(COMPLETE ALL CAPTIONS)

STOP, QUESTION AND FRISK REPORT WORKSHEET
PD344-151A (Rev. 11-02)

Pct.Serial No.

Date | Pct. Of Occ.

Time Of Stop | Period Of Observation Prior To Stop | Radio Run/Sprint #

Address/Intersection Or Cross Streets Of Stop

☐ Inside ☐ Outside | ☐ Transit ☐ Housing | Type Of Location Describe:

Specify Which Felony/P.L. Misdemeanor Suspected | Duration Of Stop

What Were Circumstances Which Led To Stop?
(MUST CHECK AT LEAST ONE BOX)

- ☐ Carrying Objects In Plain View Used In Commission Of Crime e.g., Slim Jim/Pry Bar, etc.
- ☐ Fits Description.
- ☐ Actions Indicative Of "Casing" Victim Or Location.
- ☐ Actions Indicative of Acting As A Lookout.
- ☐ Suspicious Bulge/Object (Describe)
- ☐ Other Reasonable Suspicion Of Criminal Activity (Specify)
- ☐ Actions Indicative Of Engaging In Drug Transaction.
- ☐ Furtive Movements.
- ☐ Actions Indicative Of Engaging In Violent Crimes.
- ☐ Wearing Clothes/Disguises Commonly Used In Commission Of Crime.

Name Of Person Stopped | Nickname/ Street Name | Date Of Birth

Address | Apt. No. | Tel. No.

Identification: ☐ Verbal ☐ Photo I.D. ☐ Refused
☐ Other (Specify)

Sex: ☐ Male ☐ Female | Race: ☐ White ☐ Black ☐ White Hispanic ☐ Black Hispanic ☐ Asian/Pacific Islander ☐ American Indian/Alaskan Native

Age | Height | Weight | Hair | Eyes | Build

Other (Scars, Tattoos, Etc.)

Did Officer Explain Reason For Stop ☐ Yes ☐ No | If No, Explain:

Were Other Persons Stopped/ Questioned/Frisked? ☐ Yes ☐ No | If Yes, List Pct. Serial Nos.

If Physical Force Was Used, Indicate Type:

- ☐ Hands On Suspect
- ☐ Suspect On Ground
- ☐ Pointing Firearm At Suspect
- ☐ Handcuffing Suspect
- ☐ Suspect Against Wall/Car
- ☐ Drawing Firearm
- ☐ Baton
- ☐ Pepper Spray
- ☐ Other (Describe)

Was Suspect Arrested? ☐ Yes ☐ No | Offense | Arrest No.

Was Summons Issued? ☐ Yes ☐ No | Offense | Summons No.

Officer In Uniform? ☐ Yes ☐ No | If No, How Identified? ☐ Shield ☐ I.D. Card ☐ Verbal

Figure 5A.2: UF-250 Form (cont.)

Was Person Frisked? ☐ Yes ☐ No **IF YES, MUST CHECK AT LEAST ONE BOX**

☐ Inappropriate Attire - Possibly Concealing Weapon
☐ Verbal Threats Of Violence By Suspect
☐ Knowledge Of Suspects Prior Criminal Violent Behavior/Use Of Force/Use Of Weapon
☐ Other Reasonable Suspicion of Weapons (Specify)
☐ Furtive Movements
☐ Actions Indicative Of Engaging In Violent Crimes
☐ Refusal To Comply With Officer's Direction(s) Leading To Reasonable Fear For Safety
☐ Violent Crime Suspected
☐ Suspicious Bulge/Object (Describe)

Was Person Searched? ☐ Yes ☐ No **IF YES, MUST CHECK AT LEAST ONE BOX** ☐ Hard Object ☐ Admission Of Weapons Possession ☐ Outline Of Weapon ☐ Other Reasonable Suspicion of Weapons (Specify)

Was Weapon Found? ☐ Yes ☐ No If Yes, Describe: ☐ Pistol/Revolver ☐ Rifle/Shotgun ☐ Assault Weapon ☐ Knife/Cutting Instrument ☐ Machine Gun ☐ Other (Describe)

Was Other Contraband Found? ☐ Yes ☐ No If Yes, Describe Contraband And Location ______
Demeanor Of Person After Being Stopped ______
Remarks Made By Person Stopped ______

Additional Circumstances/Factors: (Check All That Apply)

☐ Report From Victim/Witness
☐ Area Has High Incidence Of Reported Offense Of Type Under Investigation
☐ Time Of Day, Day Of Week, Season Corresponding To Reports Of Criminal Activity
☐ Suspect Is Associating With Persons Known For Their Criminal Activity
☐ Proximity To Crime Location
☐ Other (Describe)
☐ Evasive, False Or Inconsistent Response To Officer's Questions
☐ Changing Direction At Sight Of Officer/Flight
☐ Ongoing Investigations, e.g., Robbery Pattern
☐ Sights And Sounds Of Criminal Activity, e.g., Bloodstains, Ringing Alarms

Pct. Serial No. ______ | Additional Reports Prepared: Complaint Rpt.No. ______ Juvenile Rpt. No. ______ Aided Rpt. No. ______ Other Rpt. (Specify) ______

REPORTED BY: Rank, Name (Last, First, M.I.)	REVIEWED BY: Rank, Name (Last, First, M.I.)
Print ______ Tax# ______	Print ______ Tax# ______
Signature ______ Command ______	Signature ______ Command ______

Chapter 6. Concluding Remarks

In this dissertation, I study aspects of the relationship between labour markets, public policies and crime. In particular, I apply a number of economic concepts and statistical methods to the context of illegal activity and different factors that potentially trigger or deter such criminal behaviour.

In chapter 2 of this dissertation, I review and discuss the existing theoretical and empirical literature on labour markets and crime that addresses questions about how and why individuals choose to participate in illegal activity. Labour economists have in particular been interested in understanding who the individuals at risk of criminal behaviour are, and how they are affected by the state of the labour market in general and their individual labour market performance in particular. In that chapter I first discuss the seminal economic models of crime as well as subsequent models that take a more dynamic view of criminal choices. Second, I discuss examples of empirical work on different aspects of labour markets and crime.

The literature yields evidence across countries that supports the hypothesis that labour markets indeed matter for crime outcomes. Still, there are open research questions with respect to the underlying mechanisms as well as with respect to the magnitudes of the effects, which are both crucial to establish for broader policy implications. The chapter identifies a number of these open research questions some of which are addressed in subsequent chapters of this dissertation.

In particular, there appears to be the need for more research in order to understand the exact impact of unemployment on crime: While there is evidence that suggests that such a link indeed exists, we do not yet know as much about the mechanisms. Chapter 3 of this dissertation contributes to solving that gap in the literature by addressing the question of how criminal careers are initiated, and in particular whether labour market conditions at the time when youth leave school play a role in forming criminal choices. Existing research has demonstrated scarring effects of criminal behaviour on labour market opportunities later in life, but we do not know whether there is a reverse scarring effect. Yet, these effects plausibly exist as recessions typically lead to an increase in youth unemployment: If these tight labour markets at labour market entry make it more difficult for youths to find jobs, that might trigger initial involvement in crime and prompt criminal careers.

In that chapter, we empirically analyse whether such long-term effects exist. We use a variety of U.S. and UK data sources at the individual and birth cohort level. The estimations yield robust evidence that young people who graduate from school during recessions are significantly more likely to become involved in crime than those who leave school while labour markets are more buoyant. The majority of criminal offenders has a criminal record that dates back to relatively young ages, first-time offenders at older ages are observed less often. In combination with our results this leads to the conclusion that recessions do play a role in the making of career criminals by triggering initial involvement in crime. Indeed, the hypothesis is supported by additional results in the chapter which show that crime scars from higher entry level unemployment rates are both long lasting and substantial.

In our analysis, we show that there is a more long-term impact of unemployment on crime than has been previously shown in the literature, which has predominantly studied the contemporaneous link between labour markets and illegal activity. One strength of our empirical approach is that we are able to use a variety of data sources and find robust results across all specifications. A potential limitation exists in terms of the type of data that is available: We track birth cohorts over time and observe crime rates in these cohorts. Whilst the results very strongly support the hypothesis that labour market conditions at the time of graduation initiate criminal careers, we are not able to ultimately prove that those individuals in a cohort who commit crime at older ages are the same who have committed crime at younger ages when triggered by the labour market conditions. Yet, as stated above, first-time offenders at older ages are the exception which implies that it is unlikely for this not to be the case. Thus, we believe that the data restriction is not a major limitation of our analysis and does not affect the overall conclusion.

Chapter 4 focuses on a more contemporaneous relationship between unemployment and crime: I study the impact of recent and unprecedented structural changes in the U.S. labour market on crime. During the Great Recession unemployment rates in the U.S. were very high, to a very similar extent as during previous recessions. However, during the Great Recession contrary to previous recessions, there has been a substantial increase in the average duration of unemployment spells. In particular, there has been an unprecedented occurrence of long-term unemployment which stands in clear contrast to European labour markets with a history of long-term unemployment. Labour economists have associated these increases in unemployment durations with temporary unemployment benefit extensions which were implemented by policy makers in order to delay the time of benefit exhaustion in times of financial hardship. Here, I use quasi-experimental methods to estimate the impact of these labour market and policy changes on crime.

In line with previous findings in the literature, I find that higher unemployment is linked to higher crime rates and more surprisingly that this is driven by the unemployment benefit extensions. Existing economic models which suggest that the probability of criminal behaviour increases with the duration of unemployment, for example due to human capital effects or behavioural responses, lead to the hypothesis that the unemployment-crime relationship is duration dependent. Given the increase in unemployment durations associated with the benefit extensions, that offers an explanation for the findings above. Indeed, I find empirical evidence that the relationship between unemployment and crime varies with the duration of unemployment.

There are two major contributions of that chapter to the literature on unemployment on crime. First, I evaluate the impact of the recent labour market and policy changes on crime in the U.S. Second, I provide evidence for a contemporaneous link between unemployment and crime which underlies dynamics that have not yet been captured in the empirical literature. Again, the study is limited by the kind of data which is available. Unfortunately, I do not have access to individual-level data which would allow me to study the unemployment duration dependence of crime in greater detail and to entirely abstract from general equilibrium effects which potentially bias the results, such as compositional changes in the unemployed population.

While the literature on unemployment on crime focuses on the determinants of criminal behaviour, economists have also shown great interest in evaluating mechanisms of deterrence from crime. On the one hand, there is the need to understand the relationship between unemployment and crime in order to evaluate the implications of labour market policy design on criminality and the broader prevention of initial criminal behaviour. On the other hand, understanding crime deterrence mechanisms and their effectiveness is crucial for policy decision with respect to the efficient allocation of resources. A particular interest has hence been shown in establishing the causal impact of policing on crime.

In chapter 5, we analyse a prominent example of such a police strategy that targets crime deterrence: The stop-and-frisk policy in New York City. This policy allows police officers to stop, question and frisk pedestrians in New York City based on a reasonable suspicion. The policy has been legally established in New York City in the 1970s and has been widely expanded in the 1990s as one feature of the zero-tolerance strategy. Since then, the policy has been controversial: Whilst supporters have argued that the stop-and-frisk practices have strongly contributed to the decline in criminality in the city over the last two decades and have defended racial profiling strategies under the policy as effective policing being based on statistical discrimination, opponents have argued that the policy has been racially discriminating and that there has been no causal impact on crime.

In that chapter, we use a variety of data sources and empirical strategies in order to examine the hypothesis of racial bias and the impact on crime in New York City. Our analysis employs a number of quasi-experimental estimation techniques in order to identify racial bias in the stop-and-frisk practices and to estimate the overall impact of these practices on crime. We use police precinct level data on stop-and-frisk encounters and reported crime on the one hand, and individual level data on stop-and-frisk encounters and subsequent arrests on the other hand. Quasi-experimental variation stems from a court decision in 2013 that led to a discontinuity in the stop-and-frisk practices, as well as from police officer killings and killings of black individuals by police officers. Applying a range of estimation techniques we find that our results are qualitatively robust across the specifications: While we find evidence that supports the hypothesis that Afro-Americans face a disproportional probability of a stop-and-frisk encounter, our estimations suggest that there is no knock-on effect on crime.

One strength of our analysis lies in the fact that we are able to employ a number of estimation techniques exploiting different sources of quasi-experimental variation which leads to the same qualitative results and hence supports a wider external validity of the analysis. Yet, the major limitation of the study is restricted data availability. In particular, we are not able to track individuals over time in order to link stop-and-frisk events to convictions. Moreover, we do not have access to information on police deployment following the killings of police officers which could potentially bias our results.

Overall, this dissertation contributes to the empirical literature on labour markets, public policies and crime. The findings add to the findings in the literature and contribute to the knowledge that is not only of interest for researchers, but also crucial for policy makers with regard to policy design and policy evaluation in the context of crime prevention and efficient resource allocation.

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