Essays on the Economics of Crime and Criminal Sentencing

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Declaration

I, Brendon McConnell, 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.

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Abstract

This thesis is comprised of two papers, both related to the criminal justice system. In the first paper, I examine racial and ethnic sentencing differentials in US federal courts. The aim is to better understand not just the magnitude of these differentials, but rather their source. The second paper evaluates a cannabis depenalization policy in a single London borough, and assesses how such a policy can impact both drugs, and non-drugs crime, considering the responses of drug users and the police. As such, these papers contribute to a large body of literature concerning the economics of crime. The first, by better understanding sentencing outcomes, and the second, by considering how drug users and police respond to change in illicit drugs policy.

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Joint work

Chapter 3 is based on joint work conducted with my supervisors Jérôme Adda and Imran Rasul. The remaining work within the thesis is my own work, for which I am responsible for any errors.

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Contents

Chapter 1

Introduction

This thesis is comprised of two papers, both related to the criminal justice system. In the first paper, I examine racial and ethnic sentencing differentials in US federal courts. The aim is to better understand not just the magnitude of these differentials, but rather their source. The second paper evaluates a cannabis depenalization policy in a single London borough, and assesses how such a policy can impact both drugs, and non-drugs crime, considering the responses of drug users and the police.

As such, these papers contribute to a large body of literature concerning the economics of crime. The first, by better understanding sentencing outcomes, and the second, by considering how drug users and police respond to change in illicit drugs policy. In a seminal paper in the field, Becker [1968] modeled participation in criminal activities as a rational consideration of the relevant costs and benefits. A stylized version of this decision, where individuals commit crime if the expected returns exceed that of legal work (the opportunity cost of crime) can be written as:

$$(1-p)U(W_c) + pU(f) > U(W), \tag{1.1}$$

where p denotes the probability of apprehension, W_c the gains from the crime, f the monetarized sanctions imposed if caught, and W the legal wage. This simple equation implies that crime rates should respond to several different factors. Ceteris paribus, crime should fall as the apprehension rate rises, the penalties imposed on crime increase, or the legal wage rises.

This model forms the basis of a growing empirical literature on the economics of crime, with papers generally finding support for the key elements. This includes the impact of legal wages and employment oppurtunities [Grogger, 1998; Gould et al., 2002], and the role of policing on crime [Levitt, 1997; Draca et al., 2011; Buonanno and Mastrobuoni, 2012]. Other papers have focussed on the different roles played by criminial sanctions, namely the detterence effect of incarceration (where longer prison sentences result in lower crime rates, as the model above predicts) [Levitt, 1998; Drago et al., 2009] and the incapacitation effect (which is not a component of the Becker model) [Barbarino and Mastrobuoni, 2014]. The Becker model has also been applied to a variety of other areas, including income tax evasion [Allingham and Sandmo, 1972], the study of corruption of government officials [Rose-Ackerman, 1975], exam cheating [Kerkvliet, 1994] and environmental regulation [Heyes, 2000].

The two sections below outline the component parts of this thesis.

1.1 Sentencing Differentials in US Federal Courts

The first paper considers racial and ethnic sentencing differentials in the US criminal justice system (specifically in federal court sentencing), a system where such differences are observed at many stages. For instance, black individuals are over twice as likely to be arrested as their white counterparts [United States Department of Justice, 2012]. Hispanic defendants in state courts are significantly more likely to be denied bail, and are required to pay higher amounts if bail is approved [Demuth, 2003]. In 2010, black individuals were 5.8 times more likely to be incarcerated than whites. The corresponding figure for Hispanics was 2.5 [Prison Policy Institute, 2012]. Blacks and Hispanics are also disproportionately represented on death row [Criminal Justice Project of the NAACP Legal Defense and Educational Fund, Inc., 2013].

Although these statistics are noteworthy in their own right, it is also very important to understand what drives such disparities. Do these ethnic differentials reflect different levels of criminal activity of individuals committing crimes, or do they provide evidence of disparate treatment of ethnic minority individuals by the criminal justice system? The public policy implications will differ considerably depending on the answer to this question.

The first paper contributes to our understanding in this area by focusing on racial and ethnic differentials in a single stage of the criminal justice system, namely sentence length decisions in US federal courts. It aims to distinguish between the two key explanations for these differences: unobserved offense heterogeneity and discrimination. Mustard [2001, page 301] notes of the competing explanations for racial and ethnic sentencing differentials that these “two interpretations are difficult to distinguish empirically, because they provide similar testable implications”, whilst Spohn [2000, page 429] states that “the findings of more than 40 years of research examining the effect of race on sentencing have not resolved this debate.”

In order to separate between these two explanations, I consider the terrorist attacks in the US on September 11, 2001 (hereafter 9/11) as an exogenous shift to racial and ethnic preferences in US society, and examine the impact of this shift on the change in ethnic sentencing differentials in US federal courts. In order not to conflate this shift with any changes in the types of offenses committed, I restrict my attention to the set of individuals who committed their last offense before the attacks, and who are sentenced within a 180-day window around the attacks, comparing sentencing outcomes for those sentenced before 9/11 with those sentenced afterwards. I use a particularly rich dataset that allows me to condition upon a wealth of factors that influence sentencing.

Past papers that have looked at the labor market impact of 9/11 for certain ethnic groups [Dávila and Mora, 2005; Kaushal et al., 2007; Åslund and Rooth, 2005] consider the simultaneous interaction between employers and employees to estimate the effect of 9/11 on labor market outcomes. An issue with these papers is that any effect picked up will be a net effect; one that could be driven by either supply- or demand-side changes in post-9/11 labor market behavior. An advantage of this paper is that, due to the timing structure of the criminal justice procedure, I can hold fixed the key actions and behavior of the individuals being sentenced (the “supply-side”), and consider only the post-9/11 response of the courts (what could be thought of as the demand-side in this criminal sentencing setting). This enables me to (at least broadly) isolate the source of any resulting changes in ethnic sentencing differentials after 9/11.

The key result of the paper is that post-9/11, Hispanics experienced a 3.5% conditional sentencing penalty. There was no change in sentencing outcomes for any other ethnic or racial groups over this period. Departures from the sentencing guidelines play a key role in this shift. Although it is not possible to ascertain whether this post-9/11 sentencing penalty was due to sentencing judges or district attorneys (both of whom play critical roles in departure decisions), Hispanic defendants with private defense counsels did not experience any differential sentencing outcomes after the attacks.

A battery of robustness checks are performed on the data in order to assess the validity of the results. The results appear not to be driven by differential ethnic seasonal trends around the time of the attacks, nor by the types of *individuals* sentenced before and after September 11. Although there are some changes in the types of *offenses* for which Hispanics are sentenced post-9/11, using duration models I find little evidence to suggest that this is being driven by strategic re-ordering of the timing of the sentencing of individuals after 9/11. The decomposition analysis highlights the role that shifts in unobservables played in the post-9/11 Hispanic sentencing changes. As discussed in greater detail below, a key explanation for such shifts in unobservables is a change in discrimination.

Using such a "natural experiment" framework, the aim is to identify the causal impact of a shift of racial preferences (which we may be able to think of as tastes for discrimination à la [Becker, 1957]) on ethnic sentencing differentials. A key insight of Becker's work on the economics of discrimination, is that the observed racial wage gap in the labor market will reflect not average levels of discrimination of employers', but rather the level of discrimination of the *marginal* employer. In the short-run version of Becker's employer taste-based discrimination model, discriminating employers effectively view (equally productive) racial minorities as having a higher marginal cost ($w_b + d$, where w_b is the wage rate, and d a term that reflects employers distaste for hiring ethnic minorities) than their non minority counterparts (whose marginal cost is just w_a). In response to this, minority employees will sort towards the least discriminating (i.e. lower d) employers. If the relative supply of minority employees is sufficiently low compared to the number of discriminating employers, equilibrium wages will reveal no discrimination in the market. With a larger share of minority labor supply, the minority wage is set by the most discriminating employer with whom minority workers match. In either case, the equilibrium wage gap reflects the tastes of the marginal employer, not the average level of discrimination in the labor market as a whole.

It is interesting to contrast this with the case of criminal sentencing. Here minority defendants cannot sort over sentencing judges, and judges must make sentencing decisions on all defendants who come before them. In this sense, the estimates presented in the first paper will reflect the average preferences of those involved in sentencing decisions, thus revealing a more complete picture of ethnic and racial disparities than one may find from a similar labor market study.

This paper ties in with a small body of literature that has measured on the impact of 9/11 on ethnic minorities. In these studies, the focus is on the labor market. Dávila and Mora [2005], using decomposition techniques, find significant earnings negative effects for Middle Eastern Arab Men between 2000 and 2002. In a later study Kaushal et al. [2007] employ difference-in-difference techniques to study the impact of 9/11 on a variety of labor market outcomes. The authors document considerable earnings and wage declines for Arab and Muslim men, but no effects on employment for these groups. This study also provides suggestive evidence that these negative effects decline over time, and that the estimated effects are stronger in less tolerant states (using hate crimes as proxies for tolerance). Orrenius and Zavodny [2009] shift attention to the labor market outcomes of another set of individuals; recent Latin American immigrants, a group who were particularly affected by the post-9/11 changes made to legislation regarding undocumented workers and enhanced border security. Using a variety of comparison groups,the study finds evidence of declining labor market outcomes for recent Latin American immigrants, in terms of wages and employment, with the impact most pronounced for those with shorter US residence. Related papers that link preferences shocks to negative labor market outcomes (using difference-in-differences techniques) include Moser [2012] who provides evidence of how World War1 negatively shifted preferences against Germans in the US, and then links this shift to data on membership applications to the New York Stock Exchange, where this discrimination led to German applicants being twice as likely to be rejected. Miaari et al. [2008] consider the impact on job separations for Arab individuals in Israel after the second Intifada (a period of Israeli-Palestinian conflict), documenting rises in separation rates for this group post-second Intifada.

There is also a small, but growing body of economic literature that considers racial and ethnic sentencing differentials. Mustard [2001] provides descriptive evidence of disparate outcomes for both black and Hispanic defendants in U.S. Federal courts, along a variety of different margins. A more recent paper by Rehavi and Starr [2012] further explores the black-white sentencing differential, with data tracking individuals from arrest to offense. The authors find that black defendants receive substantially longer sentences for the same crimes. The study highlights the importance of the role of prosecutors in driving these differentials through the mechanism of the initial charge that individuals receive. Conditional on criminal behavior variables including arrest offense and criminal history, black individuals are twice as likely to be initially charged with a mandatory minimum offense as whites. Using decomposition techniques, the authors find that this difference in initial charging explains at least half of the black-white sentencing differential.

A further set of papers employ a variety of identification strategies to further explore this topic. Alesina and Ferrara [2011] propose a test of racial bias in capital crime cases (where all cases are automatically appealed), based on the notion that if courts are unbiased (in terms of defendant-victim race combinations), there should be no systematic differences in the rates at which higher courts reverse capital punishment decisions (errors in their framework) based on defendants race. The authors find that cases involving ethnic minority defendants have significantly higher error rates when the victim was white, compared to a non-white victim. These error differentials are found to be driven by courts in southern states. Abrams et al. [2012] use the random assignment of defendants to judges to document the variation in black-white differentials across judges, documenting significant inter-judge differences in incarceration rates, but not sentencing length. The authors also note that black judges tend to have smaller black-white sentencing differentials. Finally, Shayo and Zussman [2011] use data on small claims courts in Israel, and rely again on random assignment of cases to judges, who are either Jewish or Arab. The study finds evidence of what the authors terms "ingroup bias", where both Jewish and Arab judges are more likely to settle in favor of the claimant if they are of the same ethnicity. Observable characteristics of the judges don't appear to drive this bias. Interestingly, the authors then utilize information on the timing and location of fatalities within Israel related to the Israeli-Palestinian conflict, based on the idea conflict fatalities that are both recent and local to the various courts may enhance the salience of the divide between these two ethnic groups within those courts. The evidence presented is in line with this idea; the ingroup bias is much lower in times of no conflict, and increases as conflict escalates. As the authors note, "there is rather little ethnic ingroup bias in the Israeli courts except during periods in which political violence intensifies ethnic identification."

1.2 Crime and the Depenalization of Cannabis Possession

In spite of the penalties involved, use of illicit drugs is widespread across much of the world. It is estimated that between 167 and 315 million people aged 15-64 used an illicit substance in the previous 12 months in 2011. In percentage terms this amounts to between 3.6 and 6.9 per cent of the world's adult population [UNODC, 2013]. Given the age patterns of use, these figures are much higher for young people. Combining data on the US and a set European countries, Pudney [2010] documents rates of self-reported cannabis use that exceed 25% in eight of the twenty countries considered. Amongst these are both the US and the UK. The size of the UK drugs market was estimated to be Â£5.3 billion in 2004; roughly 40% of the market size of alcohol at that time. At this time in England and Wales, there were an estimated 5.5 million users of Cannabis, the most commonly used illicit drug.

There is large variation in terms of the drugs policy set by different countries. On one side is the punitive approach taken by the US, under the moniker of the "war on drugs", where in 2007 there were 1.8 million arrest for drug-related offenses [Donohue et al., 2011]. Those incarcerated for drug offenses comprised 16.8% and and 48% of state and federal prisons in 2011 respectively [Carson and Sabol, 2012]. On the other side is a more liberal, public health focus towards drugs use, such as that taken by Portugal in 2001, which decriminalized possession of all drugs. The Netherlands is another example of more liberal drugs policy, where the possession of small amount of cannabis is decriminalized. Neither of these polar approaches are without critics. Punitive policies are costly (the cost of drug use in the US in 2002 was estimated as 1.7% of GDP [ONDCP, 2004]) and criminalizing individuals for the possession of small quantities of drugs can have large individual costs. Critics of liberal policies note the potential harm of drugs on users [Arseneault et al., 2004; van Ours and Williams, 2009, 2012], the scope for "gateway effects' ' (where the consumption of soft drugs leads to harder drug use [van Ours, 2003; Deza, 2013]) and the links between drug use and crime.

Of all drugs, it is cannabis that several recent policy changes across the world have focused upon. Certain states of Australia have depenalized possession of small quantities of cannabis, and in 2012, Colorado and Washington voted in favor of legalizing the possession and sale of cannabis for recreational use. The focus of the second paper is on a similar policy change, the Lambeth Cannabis Warning Scheme (LCWS) of 2001-2. The LCWS mirrors some of the key cannabis policies seen elsewhere. The possession of small quantities of cannabis was no longer an arrestable offense, whilst the penalties for supply of the drug were kept unchanged. Key motivations for this policy were to free up police resources to focus on more serious crimes, and to reduce frictions between the police and the community.

The nature of the LCWS is of interest for several reasons. It was initially announced to last 6 months, and was later extended to run a total of 13 months. Thus, both short- and long-run impacts of the policy can be investigated. Second, the policy was introduced unilaterally in a single London borough, whilst cannabis possession in the other 31 London boroughs was still subject to arrest. As noted in the second paper, this has ramifications for the size of aggregate drugs markets in both Lambeth and the rest of London, and allows the study of spatial spillovers from such a unilateral policy.

In order to evaluate the effect of the policy data was obtained from the London Metropolitan Police at the borough-month level from April 1998 until January 2006. The dimensions and detail of the data are worth noting, as they allow a variety of key components of the policy to be identified. First, data series on three margins are utilized in the paper; the number of offenses, the number of arrests and the number of crimes cleared-up. The latter two series allow the study of how police effectiveness responded to the policy. The data is also very detailed. Drugs data for each borough and month is available at both the type of drug, as well as whether the offense was related to possession or supply. Data is also available for the other key crime types; violence against the person, sexual offenses, robbery, burglary, theft and handling, fraud and forgery, and criminal damage.

With this data in hand, the first part of the paper studies how drugs crime in Lambeth changed due to the policy. Cannabis possession in Lambeth rose substantially, both during and after the policy. At the same time, police effectiveness against cannabis crime fell. Given that one of the key reasons for the policy to be enacted was to re-allocate police resources, this is the focus of the second part of the paper. Here, evidence is provided of significant declines in five out of the seven classes of non-drugs crimes. In total, non-drugs crime fell by 9.4% compared to the rest of London. At the same time, there is evidence of a rise in police effectiveness against these crimes.

The third part of the paper considers the total welfare impact of the LCWS. This is interesting as the policy led to large increases in the size of the local cannabis market, but also to large falls in the majority of non-drugs crime categories. The policy likely affected other non-crime outcomes within the borough. To measure the welfare effects of the policy, the paper follows the work of Rosen [1974], by considering the impact of the LCWS on house prices in Lambeth. The analysis here suggests that total welfare fell due to the policy, finding large declines in house prices within the borough, particularly in high-drugs crime areas.

The final part of the paper develops and estimates a structural model, which encapsulates both drugs users' and police behavior, in order to answer the question of what would have happened had the policy run throughout the entire city of London, rather than just in Lambeth. The results of the model suggest that by removing a key mechanism, whereby cannabis users move to Lambeth to purchase cannabis, key policy benefits can be retained, whilst some of the costs can be mitigated. This is of interest not least because of recent cannabis policy changes across the world, where certain jurisdictions liberalize their policy, whilst neighboring areas do not.

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Chapter 2

Ethnicity, Sentencing and 9/11

Abstract

This paper aims to explain the source of ethnic sentencing differentials in US federal courts by distinguishing between two key explanations for these disparities, namely discrimination and unobserved offence heterogeneity. In order to do so, I consider the terrorist attacks in the US on September 11, 2001 as an exogenous shift to racial and ethnic preferences in US society, and examine the impact of this shift on the change in ethnic sentencing differentials in US federal courts. In order not to conflate this shift with any changes in the types of offences committed, I restrict my attention to the set of individuals who committed their last offence *before* the attacks, and who are sentenced within a 180-day window around the attacks, comparing sentencing outcomes for those sentenced before 9/11 with those sentenced afterwards.

The key result of the paper is that post-9/11, Hispanics experienced a 3.5% conditional sentencing penalty. There was no change in sentencing outcomes for any other ethnic or racial groups over this period. Departures from the sentencing guidelines played a key role in this shift. Although it is not possible to ascertain whether this post-9/11 sentencing penalty was due to sentencing judges or district attorneys, Hispanic defendants with private defence counsels did not experience any differential sentencing outcomes after the attacks. A battery of robustness checks are performed on the data in order to assess the validity of the results. Decomposition analysis highlights the role that shifts in unobservables played in the post-9/11 Hispanic sentencing changes. As discussed in greater detail below, a key explanation for such shifts in unobservables is a change in discrimination.

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Ethnicity, Sentencing and 9/11

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2.1 Introduction

In US federal courts in 2001, 78% of convicted white offenders received a prison sentence. The comparable figures for blacks and Hispanics were 90% and 94% respectively. Of those sentenced, black males received 54% longer sentences than their white counterparts, which translated into an extra 34 months in prison. These figures reflect only the average outcomes for the different racial and ethnic groups, and do not take into account the type and severity of crime committed, individuals' criminal histories and other factors relevant in sentencing. Controlling for such factors, racial differences are mitigated, but still persist.

A large body of legal and criminological literature focuses on such differentials[1], particularly in the US, although no universal pattern has emerged from this body of research. The large degree of heterogeneity in these studies in terms of the sample sizes utilised, the quality and richness of the data, the types of courts (state or federal) considered, whether or not sentencing was subject to determinate rules or based solely on judicial discretion and the statistical tools used to estimate sentencing differentials means that such an inconclusive result is unsurprising[2].

A key issue faced by the literature is how to interpret racial and ethnic sentencing differentials when found. Do these differences highlight discrimination by the courts (or other parts of the criminal justice system), or merely reflect unobservable individual or offence heterogeneity across different ethnic groups? Mustard [2001, page 301] notes of the competing explanations for racial and ethnic differentials that these "two interpretations are difficult to distinguish empirically, because they provide similar testable implications", whilst Spohn [2000, page 429] states that "the findings of more than 40 years of research examining the effect of race on sentencing have not resolved this debate." The inability to determine the *source* of these disparities is a key limitation of the existing literature[3]. Distinguishing between these two explanations in order to better understand the source

[1] Recent contributions include Schlesinger [2005], Johnson [2003] and Demuth and Steffensmeier [2004] for US state courts and Schanzenbach [2005] and Steffensmeier and Demuth [2000] for US federal courts. Although economics articles on this topic are less common, a recent paper by Mustard [2001] investigates racial, ethnic and gender disparities in sentencing outcomes in federal courts.

[2] See Spohn [2000] for an extensive review of the earlier literature.

[3] A recent exception is a paper by Abrams et al. [2012] who approach the topic from a different angle, using the random assignment of state court cases to judges in a US county in order to assess whether there exist significant differences in the black-white sentencing differential across judges. Using Monte Carlo technique to simulate a no-racial-bias counterfactual, the authors find large and significant inter-judge differences in the incarceration rate, yet not in length of sentence.

of ethnic and racial disparities in sentencing is the primary aim of this study.

To do so, I consider the terrorist attacks in the US on September 11, 2001 (hereafter 9/11) as an exogenous shift to racial and ethnic preferences in US society, and examine the impact of this shift on the change in ethnic sentencing differentials in US federal courts. In order not to conflate this shift with any changes in the types of offences committed, I restrict my attention to the set of individuals who committed their last offence *before* the attacks, and who are sentenced within a 180-day window around the attacks, comparing sentencing outcomes for those sentenced before 9/11 with those sentenced afterwards.

I use a particularly rich dataset that allows me to condition upon a wealth of factors that influence sentencing. Using such a "natural experiment" framework, I aim to identify the causal impact of a shift of racial preferences (which we may be able to think of as tastes for discrimination à la Becker [1957]) on ethnic sentencing differentials. To further investigate the source of these disparities I also utilise decomposition techniques, which have been used to investigate gender and racial gaps in earnings [Blau and Kahn, 1997; Juhn et al., 1991]. In this analysis, such techniques enable one to separate between several factors that drive ethnic differentials over time, including changes in the penalties to, and quantities of, both observable and unobservable characteristics. This allows a better understanding of the causes of the observed post-9/11 changes in ethnic differentials.

The key result of the paper is that post-9/11, Hispanics experienced a 3.5% conditional sentencing penalty. There was no change in sentencing outcomes for any other ethnic or racial groups over this period. Departures from the sentencing guidelines (detailed below in section 2.2.1) played a key role in this shift. Although it is not possible to ascertain whether this post-9/11 sentencing penalty was due to sentencing judges or district attorneys (both of whom play critical roles in departure decisions), Hispanic defendants with private defence counsels did not experience any differential sentencing outcomes after the attacks.

A battery of robustness checks are performed on the data in order to assess the validity of the results. The results appear not to be driven by differential ethnic seasonal trends around the time of the attacks, nor by the types of *individuals* sentenced before and after September 11. Although there are some changes in the types of *offences* for which Hispanics are sentenced post-9/11, using duration models I find little evidence to suggest that this is being driven by strategic re-ordering of the sentencing of individuals after 9/11. The decomposition analysis highlights the role that shifts in unobservables played in the post-9/11 Hispanic sentencing changes. As discussed in greater detail below, a key explanation for such shifts in unobservables is a change in discrimination.

Past papers that have looked at the labour market impact of 9/11 for certain ethnic groups [Dávila and Mora, 2005; Kaushal et al., 2007; Åslund and Rooth, 2005] consider the simultaneous interaction between employers and employees to estimate the effect of 9/11 on labour market outcomes. An issue with these papers is that any effect picked up will be a net effect; one that could be driven by either supply- or demand-side changes in post-9/11 labour market behaviour. An advantage of this paper is that, due to the timing structure of the criminal justice procedure, I can hold fixed the key actions and behaviour of the individuals being sentenced (the "supply-side"), and consider only the post-9/11 response of the courts (what could be thought of as the demand-side in this criminal sentencing setting). This enables me to (at least broadly) isolate the source of any resulting changes in ethnic sentencing differentials after 9/11.

Ethnic sentencing differentials have important social and economic implications. Such disparities in both the extensive (receiving a prison sentence) and intensive (sentence length) margin may exacerbate existing economic inequalities through a variety of mechanisms including shocks to household income due to incarceration, the negative impact of parental incarceration on dependants and the scarring effects that arrests and criminal history have on labour market outcomes (see Grogger [1995] and Finlay [2009]). As with labour market discrimination, discrimination in sentencing may lead to self-fulfilling equilibria where those minorities discriminated against change their criminal behaviour as a response to perceived discrimination (see Arrow [1973] and Coate and Loury [1993a,b]).

The paper is organised as follows. Section 2.2 describes the USSC sentencing guidelines and the data. Section 2.3 discusses the impact that 9/11 had on racial and ethnic preferences, and the implications that these have for sentencing outcomes. Section 2.4 presents descriptive statistics, as well as outlining my empirical method. The results are presented in section 2.5 and section 2.6 concludes.

2.2 The USSC sentencing guidelines and data

2.2.1 The Sentencing Reform Act 1984 and the USSC sentencing guidelines

The data used in this analysis relates to the sentencing of convicted offenders in US federal courts, and was obtained from the United States Sentencing Commission (USSC). Those sentenced within federal courts receive sentences based on a set of guidelines mandated by the Sentencing Reform Act (hereafter SRA) 1984. The SRA proposed a system of sentencing guidelines that would "further the basic purposes of criminal punishment: deterrence, incapacitation, just punishment, and rehabilitation"[4] through enhancing the "ability of the criminal justice system to combat crime through an effective, fair sentencing system"[5]. Underlying the reform were the key aims of reducing "the wide disparity in sentences imposed for similar criminal offences committed by similar offenders"[6] as well as achieving "proportionality in sentencing through a system that imposes appropriately different sentences for criminal conduct of differing severity"[7]. Once passed by Congress, the guidelines took effect from November 1, 1987.

As a result of these guidelines, two key factors determine sentencing in federal courts: i.) the offence level of the crime and ii.) the criminal history of the offender. Table 2.1 displays the USSC sentencing table which specifies a sentence range, in months, based on the intersection of the offence level and criminal history. So, for instance, a federal judge sentencing an individual with an offence level of 23 and a criminal history category IV must specify a sentence of between 73 and 87 months.

The offence level is composed of several different factors. Each offence type is given a base level. Offence-specific characteristics are then added, and further adjustments made to yield a final offence level. For example, an individual who commits a robbery is allocated a base level of 20. If a gun was involved, 5 further points are added. If the individual was a minimal participant in the robbery 4 points are subtracted. Lastly, if the individual was found to be in obstruction of justice, 2 further points would be added, yielding a final score of 23 points. The criminal history level is constructed to reflect past sentences of imprisonment, offences committed while under the criminal justice system (e.g.parole) as well as other factors such as escape attempts from prison. The level is then collapsed into one of six criminal history categories.

The guidelines do, however, allow for departures to be made from the specified range if the court finds "that there exists an aggravating or mitigating circumstance of a kind, or to a degree, not adequately taken into consideration by the Sentencing Commission in

[4]United States Sentencing Commission, Guidelines Manual, ï¿œ3E1.1 (Nov. 2001) (hereafter USSC Manual), Chapter 1.A.2.

[5]USSC Manual, Chapter 1.A.3.

[6]ibid.

[7]ibid.

formulating the guidelines that should result in a sentence different from that described"[8]. As shown later, downwards departures occur frequently, whereas upwards departures are rare. Downwards departures can occur for substantial assistance to authorities in the investigation of other individuals or crimes ("Substantial Assistance" in the analysis below) or for other reasons, such as guilt plea negotiations or voluntary disclosure of an offence (denoted by "other" below). It is the District Attorney who files a motion for departure, which is then considered by the sentencing judge. As noted by Albonetti [1997, and see references therein], there is some concern in the legal literature that by allowing departures, the guidelines have merely shifted the pre-guideline discretionary powers that judges once had to prosecuting attorneys. Judges still retain some power, as it is they who decide whether or not to accept the motion for a departure.

2.2.2 Data

For the main analysis detailed below, I consider the universe of individuals sentenced in US federal courts within a 180 day window around September 11, 2001, subject to certain selection criteria. Firstly, in order to focus solely on ethnic and racial sentencing differentials, I abstract from considering gender disparities[9] in sentencing and omit all females from the full dataset[10].

Next, I only consider males for whom there exists non-missing sentence length, criminal history, offence level, offence type, district and age data. In order to minimise the potential sample selection issues that omission of observations with missing entries entails, I keep observations with missing entries for any other control variables, and include a missing category when constructing dummies of the categorical variables used in this analysis[11].

Lastly I omit any individuals within the 6 month window who commit an offence after 9/11. Given that the crux of my identification strategy (which I detail explicitly below in Section 2.4.2) involves a comparison of those sentenced prior to 9/11 with those sentenced afterwards, it is imperative that any date-specific selection criteria are balanced across

[8]USSG §5K2.0

[9]Mustard [2001] documents both racial and gender differentials in federal courts, finding as large, and at times larger, gender differences (in favour of females) than racial and ethnic differences (in favour of whites) in sentencing.

[10]For the fiscal year of 2001, females comprised 14.5% of the 59 855 individuals sentenced in federal courts

[11]The only variables with a large number of missing entries is the variable defence counsel. For this variable, 48.2% of my main sample have missing entries.

the two groups. For this reason, for the subset of males sentenced before 9/11, I restrict the final offence date to be 180 days before September 11, 2001.

2.3 9/11 and Preference Shifts

There is evidence that suggests the attacks of September 11th, 2001 shifted preferences along a multitude of dimensions. One such dimension was society's security/civil liberty trade-off. The USA PATRIOT Act, signed on October 26th, 2001 increased the power of law enforcement agencies to perform searches of private records (including e-mail communication and phone histories) of US citizens, as well as extending the powers of enforcement agencies in detaining and deporting immigrants suspected of terrorism-related incidents. This focus on immigration resulted in two further, related Acts being passed in mid- to late-2002 (i.e. both outside of the main time period of focus in this analysis), the Enhanced Border Security Act[12] (14 May, 2002) and the Homeland Security Act (25 November, 2002).

There was also a notable spike in the reports of discrimination violence against Arab and Muslim individuals (as well as those perceived to be either of these groups, for instance South Asians or Sikhs). As Human Rights Watch [2002] noted "Arab and Muslim groups report more than two thousand September 11-related backlash incidents. The Federal Bureau of Investigation reported a seventeen-fold increase in anti-Muslim crimes nationwide during 2001. In Los Angeles County and Chicago, officials reported fifteen times the number of anti-Arab and anti-Muslim crimes in 2001 compared to the preceding year". Further reports of discrimination and hate crime victimisation are reported in American-Arab Anti-Discrimination Committee [2003]. The report also notes the rise of discriminatory immigration policies, particularly focused on young Arab men.

Several recent studies have considered the impact that this preference shift has had on labour market outcomes for such individuals in the US [Dávila and Mora, 2005; Kaushal et al., 2007] as well as other countries such as Sweden [Åslund and Rooth, 2005]. These papers are similar in spirit to this analysis, in that they utilise the attacks of 9/11 to

[12] According to Jenks [2002] the most important provisions of this Act included: i.) "A requirement that the Immigration and Naturalization Service (INS) make inter operable all its internal databases, so that all information about a particular alien may be accessed with a single search", ii.) "A requirement that federal law enforcement and intelligence agencies share data on aliens with the INS and the State Department" and iii.) "A requirement that all travel and entry documents, including visas, issued to aliens by the United States be machine-readable and tamper-resistant and include a standard biometric identifier.".

consider the impact on ethnic differentials caused by the exogenous shift of racial and ethnic preferences [13]. Studies focusing on the US have found that 9/11 had a negative effect on the labour market outcomes of Arab and Muslim men [Kaushal et al., 2007] and Middle Eastern Arab males [Dávila and Mora, 2005].

Although it would be interesting to consider the impact of 9/11 on sentencing differentials for Arab and Muslim individuals, these groups are not sufficiently identified in the data, and even if they were, the sample size (presumably a subset of the small group of "other" ethnicities in the data) would be too small to say anything reliable regarding such individuals. At this stage, I consider other ethnic and racial minorities in the US. Is there any evidence for a "spillover" effect from the increased discrimination and victimisation of Arab and Muslim individuals noted above? Such effects could result from an increase in the *salience* of race and ethnicity in post-9/11 US society, or a greater awareness of those perceived in any way to be "outsiders".

2.3.1 Post-9/11 Survey

Table 2.2 presents a selection of results from a 2002 survey of 1000 ethnic Californians, who were asked variety of questions related to the ways in which 9/11 had changed certain areas of their lives, as well as their perceptions. Unsurprisingly, Middle Eastern individuals reported some of the largest changes, for instance 58% reporting an increase in discrimination post-9/11 compared to just 3% of African-Americans. Middle Easterns are also the most likely to have reported feeling depressed post-9/11. Albeit "soft" evidence, a point to note here is that of the two largest ethnic minority groups in the US (Hispanics and blacks), the responses of Hispanics were much more aligned with those of Middle Eastern individuals than were African-Americans'. 58% of Hispanics thought the US had too much world influence, compared to 27% of African Americans. When asked if they preferred to be referred to ethnically as "American", the affirmative response for African-Americans was double that of Hispanics.

2.3.2 Assault Victimisation

Figure 2.2 displays the 2000-20001 change in the number of monthly assault victimisations for whites, blacks and Hispanics (normalised by the 2000 count - to reflect seasonal

[13] Other studies related in design include Moser [2012] who considers the impact that World War I had on entry to the New York Stock Exchange for German traders, and Miaari et al. [2008] who consider the impact of the second Intifada on the job separation of Arab workers in Israel.

differences as well differences in scale - so essentially a growth rate). Given the physically violent backlash against Arabs and Muslims noted above, it is possible that any preference shifts against either blacks or Hispanics could also show up in victimisation results. Victimisation growth rates for the three groups follow broadly similar patterns over the first part of the year. Where the results differ is that just after 9/11 the growth rate of assaults for Hispanics spiked substantially. That of blacks continued to decline, picking up in December, and white rates increased as well, although less pronounced than the changes for Hispanics.

On their own, these pieces of evidence are not particularly strong, but together, they suggest a form of spillover effect from the direct impact of anti-Arab and Muslim sentiment post-9/11 towards Hispanics. Even if such a shift in ethnic preferences did occur, what would be the implications for the sentencing of Hispanics? First, note the sentencing guidelines rule out any discrimination based on “Race, Sex, National Origin, Creed, Religion, and Socio-Economic Status” stating that such factors “are not relevant in the determination of a sentence”[14]. Based on this, it is certain that I am testing the null hypothesis of no discrimination. What is the role of the ethnicity of sentencing judges? One of the findings of Abrams et al. [2012], who used the random allocation of cases to judges to consider how racial sentencing gaps differ across judges, found that racial sentencing differentials were smaller among African-American judges compared to their white counterparts. Of those judges serving during the sample period 84% were white, 9% black and 5% Hispanic.

2.4 Descriptives and Empirical Method

2.4.1 Descriptives

Table 2.3 provides an overview of the key legal variables for this analysis. The average sentence length in the sample is 61.6 months, so just over 5 years. Blacks receive much longer sentences than any other ethnic group, with an average of 90 months compared 55 and 48 for whites and Hispanics respectively. Statistics are also presented for the “other”[15] ethnic group, yet given both the small sample size of this group and the ethnic heterogeneity within this group, I do not focus on these individuals.

[14] USSG §3H1.10, p.s.

[15] Comprised of groups including American Indian or Alaskan Native, Asian or Pacific Islander and Multi-Racial.

Black defendants have higher offence severity as well criminal history scores than any other ethnic group. Whites and Hispanics have similar averages for these two variables. Higher criminal history scores reflect more extensive prior criminal behaviour, but will also reflect the outcome of any past discrimination in the criminal justice system. Such issues, although relevant to this study, are not expanded upon any further. I control for the impact that these variables have on sentencing outcomes in all regressions below, in order to consider *current* disparities, and specifically, conditional disparities.

A similar proportion of defendants are sentenced according to their original guideline range, with 35-40% receiving downwards departures. There is some difference between ethnicities in the type of departure; Hispanics are less likely to receive downwards departures for substantial assistance to authorities, yet more likely to receive departures for other reasons. There is considerable difference too in the pre-sentence status of those from different ethnicities, with whites the least likely to be in custody (57%), Hispanics the most (85%).

Table 2.4 presents further descriptives based on non-legal individual characteristics, highlighting several differences across the groups. Just over a half of black defendants are single (54%), compared to a third of Hispanics (33%) and whites (34%). Whites are the least likely to cohabit and the most likely to be divorced. Whites have less children and are over five years older than blacks and Hispanics on average. The modal education category is "less than high school" for Hispanics and blacks, and "high school" for whites. 10% of whites are college graduates compared to 3% of blacks and 2% of Hispanics.

2.4.2 Empirical Method

My empirical approach is to consider the terrorist attacks of 9/11 as a natural experiment that exogenously shifted tastes for discrimination, and to then examine the effect of this shift on changes in ethnic and racial sentencing differentials. The key element of this identification strategy is to solely consider the sentencing of a set of individuals who committed their last offence *before* 9/11, and compare the outcomes of those sentenced prior to 9/11 with those sentenced afterwards[16]. This rules out any changes in both the

[16] For this analysis I consider those sentenced within a 180-day window around 9/11. I investigated a number of other windows-lengths around September 11, with results robust to local changes in window length. There are two countervailing issues related to specifying the length of window around 9/11. Windows too small do not yield a large enough sample size, whereas longer windows reduce the variety of offences, as only those cases with a long distance from last offence to sentence date are considered, which may introduce sample selection issues.

supply and the type of offences committed as a response to the post-9/11 environment. Figure 2.1 presents a schematic overview of my empirical strategy, detailing restrictions on both the sentencing date, and the date of last offence.

In order to consider the impact of the 9/11 terrorist attacks on ethnic sentencing differentials, I use the following specification:

$$\begin{aligned} s_{iet} &= \alpha + \sum_e \delta_e Ethnic_e + \rho Post_t + \sum_e \phi_e (Ethnic_e \times Post_t) \\ &+ X_i'\beta + \sum_g \gamma_g G_{ig} + \sum_f \omega_f OFF_{if} + \sum_d \lambda_d D_{id} + \epsilon_{iet} \end{aligned} \tag{2.1}$$

where s_{iet} denotes the log of the sentence length for individual i of ethnicity e sentenced on date t. The richness of this data is exploited by controlling for criminal history and offence severity in a very flexible way, by explicitly accounting for the USSC guideline table displayed in Table 2.1. A set of 257 dummies G_{ig} (for all bar one of the cells of Table 2.1) are included, each corresponding to a different combination of offence severity and criminal history group. Thus the specification in equation 2.1 essentially considers the within-guideline cell variation in sentencing length. A set of offence dummies OFF_{if} control for differences in sentencing penalties for different types of crimes. X_i is a vector of individual and individual-case characteristics including marital status, dummies for the age decile of the individual, number of dependants, highest education level, the type of defence counsel and pre-sentence status. District dummies D_{id} capture time-invariant differences across districts, which will include district attorney and average judge effects.

$Ethnic_e$ dummies control for sentencing differences across ethnic groups (where white is the reference category) and a dummy $Post_t$ that equals 1 for sentencing dates after September 11, 2001 allows for differences in sentencing after the terror attacks. Furthermore, the set of interactions $Ethnic_e \times Post_t$ allows for heterogeneous sentencing effects of 9/11 across ethnic groups. Of particular interest in this study are the set of ϕ_e parameters related to these interaction terms, which reflect any difference in the sentencing of ethnic minorities post-9/11.

Ideally these parameters would solely reflect changes in the discriminatory nature of the judicial system post-9/11. However these will also pick up any other ethnic group specific changes. For instance, were defence lawyers to respond to the terrorist attacks by implementing differential levels of effort for clients of different ethnicities, this too would be captured by the ϕ_e parameters. Although this would now cloud the interpretation of the key parameters, it could be that this defence counsel response is a relevant facet of discrimination in the criminal justice system. The main point here is that it is necessary to carefully consider all other factors that may cause the ϕ_e coefficients to be non-zero in order that such changes are not spuriously linked to discriminatory shifts. Lastly, the error term ϵ_{iet} is clustered at the ethnicity-district level, allowing for shocks to specific ethnic groups to be correlated at the district level.

Equation 2.1 takes the form of a repeated cross-section, regression-adjusted difference-in-differences (DD) model. It is thus important to consider the identifying assumptions of such an approach. To do so, it is useful to decompose the error term ϵ_{iet} in equation 2.1 into three terms:

$$\epsilon_{iet} \quad = \quad (\phi_i + \psi_t + u_{iet}) \tag{2.2}$$

an unobservable individual fixed effect ϕ_i, a common macro shock ψ_t and an idiosyncratic transitory shock u_{iet}.

The first assumption underlying the DD approach is that selection into treatment is not based on the individual transitory term u_{iet}. This assumption is called in to question in certain program evaluation studies where treatment selection is determined by the outcome variable, and transitory shocks in this variable alter the likelihood of treatment[17].

There is no direct analogue to this issue in my analysis, although this assumption may not be satisfied due to other reasons. For instance, one concern may be that if the courts were perceived as being harsher on ethnic-minority individuals post-9/11, that these individuals may change their attitude towards the criminal justice system, which in turn could drive differential behaviour during the sentencing procedure. To the extent that such attitudes (unobservable to the econometrician) are observable to the sentencing judge, this would lead to changes in sentencing and thereby conflate the 9/11 effect of the courts with an additional defendant response. With poorer quality data, this could be an issue. However, the sentencing guidelines documented in section 2.2.1 explicitly account for (at least some of the main) factors related to defendant attitude during

[17] A classic example of this is "Ashenfelter's Dip" [Ashenfelter, 1978], where those who experience transitory "dips" in earnings prior to a training scheme commencing are more likely to participate in the scheme as a consequence of the dip. This group would likely experience a subsequent rise in earnings from the dip, even in absence of the programme, therefore leading to an over-estimate of the effect of the treatment.

sentencing, and incorporate offence level reductions for the acceptance of responsibility[18] and increases for obstructing or impeding the administration of justice[19] during several stages of the criminal justice procedure, including sentencing.

The next assumption, and key to identification of the treatment effect, is that the treatment and control groups (ethnic minority groups and white respectively in my analysis) share a common macro shock ψ_t. Were macro shocks to differ across groups, it would not be possible to disentangle these shocks from the impact of treatment. Although not directly testable, it is possible to perform certain robustness checks to ascertain whether this is likely to be upheld in the data. I detail some of these in section 2.5.4 below. Finally, as I am using repeat cross-sectional data, it is essential that the composition of the groups does not change systematically over time, that is:

$$E\left[\phi_i|Ethnic_e = E, Post_t = 0\right] \quad = \quad E\left[\phi_i|Ethnic_e = E, Post_t = 1\right] \qquad \text{for all} \quad E \tag{2.3}$$

If equation 2.3 is not satisfied, it would lead me to spuriously attribute shifts in the composition of the groups to treatment effects. Again, several robustness checks performed in section 2.5.4 relate to testing the empirical validity of this assumption. If the assumptions discussed above are satisfied, then by using variation in access to treatment across both time and groups, the DD approach identifies the average treatment effect of the treated (ATT), where "treatment" corresponds to ethnic minority status post-9/11.

2.5 Results

2.5.1 Baseline Results

Table 2.5 presents the first set of results. The first column reports estimated parameters from what is essentially an unconditional DD regression for log sentencing. Without any control variables, there is no significant effect of the 9/11 terrorist attacks for sentencing outcomes of any of the ethnic groups, including white defendants, who are the ethnic base category here. Moving along the columns, sets of extra regressors are sequentially included in the DD regressions. The final column, where all regressors outlined in equation 2.1 are included is my baseline equation for the proceeding analysis. There

[18] USSG §3E1.1

[19] USSG §3C1.1

are several points to note.

Firstly, although 9/11 did not significantly change conditional sentencing outcomes for whites (the coefficient on $Post_t$, $\hat{\rho}$, is not significantly different from zero) or blacks, it did for Hispanics. As the dependant variable is log sentencing, the coefficient of 0.035 can be interpreted as a 3.5% increase in the conditional Hispanic sentencing differential (with respect to whites) compared to before 9/11. Such a result is striking, as prior to 9/11, the estimate $\hat{\delta}_{Hispanic}$ reflected a complete absence of a conditional sentencing penalty for Hispanics. Secondly, the results reflect the importance of controlling sufficiently for variables relevant in sentencing. For instance, unconditionally, black defendants receive 53% longer sentences than their white counterpart pre-9/11, yet once all regressors are included, this sentencing penalty declines to 5.5%. Although discrimination could play a role in generating this penalty, it is not possible to identify the extent to which it does so. This is problem generally faced by the existing literature. What the results do show for black defendants is that there was no change in the sentencing penalty post-9/11. This is very much in line with the evidence discussed in section 2.3.

To better understand this result, Table 2.6 presents evidence on the role that guideline departures play in generating the post-9/11 Hispanic sentencing penalty. The first column is a replication of the last column of Table 2.5 as a reference point. The next column, estimates from a Linear Probability Model regression of the likelihood of not departing from the initial allocation of guideline cell, shows that Hispanics again differ from the other ethnic groups. Post-9/11, non-Hispanics are 2.2% more likely to receive a departure, which, given that only 1% of the sample receive upwards departures, means moving to a cell with a lower guideline sentencing ranges. Hispanics on the other hand, are more likely to stay within their initial cell. The third and fourth columns show the implications that these differential departures have on sentencing. The pattern that emerges here is not entirely straightforward. Pre-9/11, Hispanics receiving departures were sentenced relatively harshly in their new cells, generating the conditional sentencing penalty of 3.5% seen in column 4. Hispanics not receiving departures were sentenced relatively leniently, which is reflected by the -0.029 point estimate in column 3. These two countervailing differentials cancel out on average, to produce the estimate of the pre-9/11 conditional sentencing differential found in column 1. What happened after 9/11 is that sentencing outcomes for Hispanics receiving departures did not change significantly ($\hat{\phi}_{Hispanic}$=0.02 with a standard error of 0.024), but the outcomes for those not receiving departures *did*, almost perfectly cancelling out the initial leniency experienced pre-9/11 with a penalty of 0.03. It appears that this change is what is driving the main result of an increased Hispanic sentencing penalty.

2.5.2 Quantile regression

Extending the focus of the analysis, I consider estimates of the post-9/11 changes in sentencing differentials for ethnic minorities for a large range of conditional quantiles of the sentencing distribution. For the τth conditional quantile, I use the model:

$$\begin{aligned} Quant_\tau\left(s_{iet}|.\right) &= \alpha_\tau + \sum_e \delta_{e,\tau} Ethnic_e + \rho_\tau Post_t + \sum_e \phi_{e,\tau}(Ethnic_e \times Post_t) + Z_i'\pi_\tau \\ \text{where } Z_i' &= \left[X_i' \;\; G_{ig}' \;\; OFF_{if}' \;\; D_{id}'\right]' \end{aligned} \tag{2.4}$$

and all individual terms are as described in section 2.4.2. The additional τ subscripts denote the different parameter values for each conditional quantile τ of s_{iet}. Figure 2.3 displays the results of point estimates on the $Ethnic_{Hispanic} \times Post_t$ term from 91 separate quantile regressions based on the 10th to the 90th conditional sentencing quantile. The figure illustrates a larger (although not statistically significantly different) post-11 effect for lower conditional quantiles, which then declines (non-monotonically) across the conditional sentencing distribution. How to interpret this? This pattern may be reflecting the increase in sentencing differentials for those Hispanics not receiving guideline departures. Recall from Table 2.6 that this group received lower conditional sentences than their white counterparts prior to 9/11.

2.5.3 The Role of Defence Counsel

Another dimension to consider here is the role played by the defence counsel. Table 2.7 presents the DD estimates separately based on type of defence counsel[20]. Hispanics represented by private defence counsels are insulated from any post-9/11 sentencing increases, and in fact receive 6% lower sentences than their white counterparts. The post-9/11 sentencing differentials are much more pronounced for those represented by either Court Appointed (CA) or Federal Public Defenders (FPD), with differentials of

[20] It should be noted that this sub-group analysis is limited by the poor recording of defence counsel type in this data, thus the sample sizes are small. Furthermore, if non-recording of defence counsel information (this is done by the USSC, not the defendant) is correlated with unobservables driving sentencing, then the samples will also yield biased point estimates.

6.2% and 4.5% respectively, both significantly different from zero. The outcomes differ when considering solely those who do not receive a departure. For these individuals, those represented by FPDs still experience an increased post-9/11 differential,whereas those with CA counsels do not[21]. At this point, it is not clear if the different sentencing penalties experienced by Hispanics represented by the different counsel reflect selection effects due to defendants[22] or whether different defence counsels responded differentially to 9/11. Data that I am currently collecting should hopefully shed light on this issue considering several previous stages of the criminal justice system in conjunction with sentencing, in order to separate out defence counsel responses with judicial responses.

2.5.4 Robustness Checks

In this section I present a set of analyses that checks the validity and robustness of the results discussed above. In the first of these, Table 2.8, results are shown for a set of regressions investigating the stability of the sample composition (based on observables) between the pre- and post-9/11 periods. The purpose of this check is related to the third assumption discussed in section 2.4.2. Although the fixed effect is unobservable, considering the stability of many observable characteristics may help to ascertain whether such an assumption is likely to be valid. Each variable of interest was regressed upon all other variables conditioned upon in the main analysis. So for instance a dummy for highest education being High School was regressed upon all other individuals characteristics, guideline cell dummies, offence type dummies and district dummies.

There were no significant Hispanic (or any other ethnic group) changes post-9/11 in any of the individual characteristics of defendants. These include age, educational attainments, number of dependants, pre-sentence status and criminal history score. Thus I am comparing very similar individuals before and after the attacks. The only differences between the pre- and post-9/11 time periods are in the types of offences for which Hispanics are sentenced. This can be seen in the last 3 columns. After 9/11 Hispanics were less likely to be sentenced for drug trafficking offences, and more likely to be tried for immigration offences. The fact that offence severity also declines is due to the fact that drug trafficking carries a higher baseline severity score than does an immigration

[21] Iyengar [2007] provides a very thorough analysis of the different characteristics of, and incentive structures faced by, these different defence counsels.

[22] Although defendants should be randomly allocated a defence lawyer, Iyengar [2007] finds that this is not always the case, so although there should not be, there may be selection effects here

offence (mean offence severity scores are 24 and 16 respectively). This change in offence type and severity is controlled for in all regressions, so the remaining concern is that other unobservables, related to offence characteristics, shifted too.

I further consider this issue by investigating whether or not the duration between the date of last of offence and sentencing date changed for Hispanics after 9/11 compared to before. The purpose of this section of analysis is to consider whether the changes in offence types observed in Table 2.8 could be due to a re-ordering of individuals post-9/11. One concern is that individuals who committed certain offences particularly salient after 9/11, such as immigration or terrorism-related offences, may have been processed particularly rapidly after the attacks as a signal of the courts' resolve to punish such offences. A related issue is that the worst, or most chronic, offenders may have been rushed through the system in order to be made examples of post-9/11. If either of these concerns meant that Hispanics were over-sampled in the courts post-9/11 response, then the main results from above could be due to Hispanics being incidentally sentenced more harshly due to the types of crimes they commit, rather than explicit discrimination per se. Such responses of the courts would shift the composition of the sample after 9/11, and could also invalidate the first assumption listed in section 2.4.2 that stated that treatment may not depend on the individual transitory term u_{iet}.

The duration analysis presented in Table 2.9 relates to both of these concerns, considering the full sample, and then specific offences individually. Two different duration models are utilised; a Weibull Proportional Hazard Model with Gamma distributed unobserved heterogeneity and a Cox semi-parametric Proportional Hazard model. In this duration analysis, all control variables used in the main analysis above are conditioned upon here too. It should be noted that the regression coefficients presented relate to the hazard rate, thus a negative coefficient indicates a lower hazard rate, which in turn means a longer duration. The converse also applies.

The first two columns of Table 2.9 present results based on the full sample, and show that there was no significant post-9/11 change in the duration from date of last offence to sentencing date for any group. This is true for both models. Given that there were shifts in the likelihood that Hispanics were sentenced for both immigration and drugs trafficking offences, the next four columns consider the last offence to sentencing durations for these two offence types separately. Columns 3 and 4 present results based on the Weibull model. The first point to be noted is that there is no significant change for any group in duration for immigration offences, one of the most pertinent offences after 9/11. There was, however, a significant (at the 5% level) increase in the duration for Hispanics charged with drug trafficking offences. This could indicate that cases involving Hispanic drug traffickers were better prepared (and thus took longer to bring to trial) after 9/11, but it is not at all clear why this would be the case. The results from the Cox model are presented in columns 5 and 6 and differ from the Weibull model results, in that there are no significant shifts in Hispanic durations for either drugs trafficking or immigrations offences after the attacks. Thus there is at best rather weak evidence of any strategic changes in the re-ordering of defendants after 9/11.

Another key issue to consider is whether the key results discussed in section 2.5.1 are really due to a 9/11 response of the federal courts, rather than just picking up differential ethnic time trends in sentencing, which could reflect differential ethnic trends in committing offences. An example of such would be seasonal trends in the types of crimes committed that differ across ethnic groups, with Hispanics being sentenced more harshly in the winter months. This would invalidate the common time trend assumption discussed above, and would lead to a spurious attribution of differential time trends to a post-9/11 shift in ethnic discrimination in sentencing. To consider this issue, I utilise several years of sentencing data prior to the attacks, and run a set of placebo regressions. That is, instead of using September 11, 2001 as the reference date, I use a series of relevant dates at six month intervals from March 11, 1999 to March 11, 2001. If the results were driven solely by an annual cycle, then I should observe the same patterns for September 11, 1999 and September 11, 2000, and the opposite patterns for the March 11 placebo regressions.

Table 2.10 presents placebo analogues to my baseline regression (column 5 of Table 2.5). Outcomes are considered in a six month window around the placebo date, with restrictions on the last offence date as detailed above and represented in figure 2.1, and all key covariates are controlled for as before. The main point to note here is that there is no consistent, recurrent pattern in ethnic sentencing outcomes in the years prior to 9/11, thus at a minimum, the results above were not driven by a seasonal ethnic sentencing cycle. For both prior September 11ths, significant shifts in whites' sentencing outcomes in the post period were essentially offset for both blacks and Hispanics. For instance, when mean white sentencing outcomes declined by 5.4% after September 11, 1999, black and Hispanic sentencing rose by 4.9% and 4.7% respectively, indicating stability in the ethnic minority sentencing differentials before and after this placebo date.

2.5.5 Decomposition Analysis

In order to better understand the causes of the changes in sentencing differentials post-9/11, I follow Juhn et al. [1993] in decomposing the sentencing differentials into components based on ethnic differences in the penalties associated to, and quantities of, observable and unobservable characteristics. Such techniques are commonly used in labour economics to better understand the driving forces behind wage differentials, for example gender wage differentials [Blau and Kahn, 1997] and the Black-White wage gap [Juhn et al., 1991]. The primary focus in this section is the Hispanic-white sentencing differential, but all analysis is run on the black-white differential as well. First consider a separate sentencing equation for white individual i in year t:

$$s_{it} = X'_{it}\beta^W_t + \sigma^W_t \theta_{it} \tag{2.5}$$

where s_{it} is the log of sentence length, X_{it} a vector of all covariates, β^W_t the coefficients from the white sentencing equation, σ^W_t the standard deviation of sentencing for whites in period t and θ_{it} is the standardised residual.

The sentencing differential between Hispanics and whites for period t can be written as:

$$\Delta s_t = s^H_t - s^W_t = \Delta X_t \beta^W_t + \sigma^W_t \Delta\theta_t \tag{2.6}$$

where Δ denotes the average Hispanic-white difference in the variable directly proceeding it. Equation 2.6 thus splits the Hispanic-white sentencing into two components: the first based on differences in average observable characteristics weighted by the *white* sentencing penalties[23] to these characteristics,$\Delta X_t \beta^W_t$, and a second term based on the differences between the two groups' average position in the *white* residual sentencing distribution, $\sigma^W_t \Delta\theta_t$.

The final step is to consider how the ethnic sentencing gap changed from before 9/11 (t=0) to after (t=1). To do so subtract equation 2.6 for t=0 from the corresponding equation for t=1, add and subtract 2 further terms, i.) $\left(\Delta X_0 \beta^W_1 - \Delta X_0 \beta^W_1\right)$ and ii.)

[23] The terms in equation 2.6 are based on the white sentencing equation, thus simulating a "non-discriminatory" environment, where all individuals are sentenced as if white. As white individuals are the reference category in the DD analysis above, this is the natural way to write the equation, although it could be written in terms of the Hispanic sentencing equation, or a pooled equation.

$\left(\sigma_1^W \Delta\theta_0 - \sigma_1^W \Delta\theta_0\right)$, and then re-arrange. This yields :

$$\begin{aligned} \Delta s_1 - \Delta s_0 &= (\Delta X_1 - \Delta X_0)\beta_1^W + \Delta X_0(\beta_1^W - \beta_0^W) \\ &+ (\Delta\theta_1 - \Delta\theta_0)\sigma_1^W + \Delta\theta_0(\sigma_1^W - \sigma_0^W) \end{aligned} \tag{2.7}$$

where the change in the Hispanic-white sentencing differential $(\Delta s_1 - \Delta s_0)$ is decomposed into four terms. The first term, the X-effect, measures the contribution to the change in the sentencing gap due to changes in observable characteristics between the two groups, such as offence severity or type of defence counsel. The second, the β-effect reflects the role of changes in the sentencing penalties attributed to these characteristics. For instance, given that Hispanics are more likely to commit immigration offences, if 9/11 led to judges imposing harsher penalties on these offences, then the immigration offence difference would be weighted more heavily, leading to a larger sentencing gap. The third element, θ-effect, documents the effect of changes in the sentencing differential once all explanatory variables have been conditioned upon, i.e. the change in Hispanics position within the white residual sentencing distribution. For example, shifts in discrimination against Hispanics post-9/11 would lead to an increase in Hispanic's average position in the white residual distribution, thus leading to an increase in the Hispanic sentencing penalty. The fourth term, the σ-effect, reflects the role that changes in the spread of the white residual sentencing distribution has, holding fixed the pre-9/11 ethnic residual gap.

The third and fourth terms, based on the residual sentencing gap, will reflect both discrimination and unobservable offence and defendant characteristics. Given both the richness of the data, and the identification strategy employed in this study, where only individuals who committed their last crime prior to 9/11 are considered, the θ- and σ-effect terms should predominantly reflect shifts in discrimination.

Table 2.11 displays the results from the JMP decompositions, for changes in both the black-white and Hispanic-white sentencing differential. The first 2 rows of both columns reflect the raw differential in sentencing also shown in column 1, Table 2.5. Focusing on Hispanics, the change in differential of -4.2% shows that after 9/11, without conditioning on any relevant variables, average sentencing outcomes were improved with respect to whites. The next 2 lines expand on this, highlighting a more complex story. Based on all observable characteristics, this relative sentencing improvement should have been double of what it was, with a change of -8.3%. The reason that this did not occur is due to a countervailing increase of 4.1% in the gap due to unobservables. This increase is primarily due to changes in the unobservable quantities - the θ-effect. As noted above, a key candidate for such a change is a shift in discrimination, particularly in this case given the richness of the observables. The pattern for the black-white differential is very different. This differential rose by 1.65% post-9/11, yet the full extent of this change could be attributed to changes in observables.

Part B of the table documents the roles of specific observables in the sentencing differential changes. In terms of observable characteristics, the main variable leading to a sentencing improvement for Hispanics was the guideline cell in which individuals were sentenced. As seen in Table 2.8, this was due to a decrease in the average offence severity for which Hispanics were tried. The minimal contribution of the overall β-effect hides several changes that offset each other in aggregate. The penalties related to individual characteristics and the court district dummies led to a combined improvement of average Hispanic sentencing outcomes post-9/11 of 5.1%. However, the penalties associated with the types of crimes committed led to 4.7% increase in the Hispanic penalty. This was due predominantly to an increase in the penalty for immigration offences (3.7%).

2.6 Conclusion

By considering 9/11 as an exogenous shift to tastes for discrimination in the US, and linking this change in discrimination to changes in ethnic sentencing differentials, this paper aims to address the crucial issue of the source of racial and ethnic disparities in criminal sentencing outcomes. It is necessary to understand the underlying causes of such disparities in order to best assess what can be done to address the issue; each of the explanations implies a very different policy response. The main result is that post-9/11, Hispanics experienced a 3.5% conditional sentencing penalty relative to their white counterparts. There was no change in sentencing outcomes for any other ethnic or racial groups over this period. Decomposition analysis highlights the role that changes in unobservables played in driving the observed change in conditional sentencing outcomes for Hispanics. The results are subjected to a set of robustness checks in order to ascertain whether other factors, such as differential ethnic seasonal trends in sentencing or strategic re-ordering of *when* individuals are sentenced post-9/11, could be driving this results. It appears that this is not the case.

An issue with the results presented in the paper concerns how to interpret the coefficients on the post-9/11 terms. What I claim to identify in this paper is the impact on sentencing differentials of a *change* in the taste for discrimination. Interpretation is not straightforward, as this change does not have any obviously quantifiable units, thus I don't know how *much* tastes changed (Note that the evidence in section 2.3 suggests that firstly such a shift did occur, and secondly that this shift was particularly focussed on Hispanics.). It is thus helpful to benchmark the findings, both internally (by comparing the impact of 9/11 on sentencing outcomes to the impact of other covariates on the same outcomes) and externally (by considering how the results here compare to the wider set of natural experiment papers that consider the labour market impact of preference shifts due to certain events such as 9/11, World War 1, the second Intifada etc.).

There is still much to do in this research area. At this point, I am unable to discern whether the main results are driven by the district attorney or the sentencing judge. Data that I have collected on previous stages of the criminal justice procedure, in which sentencing judges and district attorneys play different roles, may help to identify the key player(s) driving the increased sentencing penalties for Hispanics after 9/11. In order to assess the external validity of my findings, I am currently performing further analysis on data from state courts in the US. These courts consider a different set of crimes to those seen in federal courts, and thus a different set of defendants. I am also in the process of obtaining data on sentencing outcomes in Crown Courts in England around both 9/11, as well as the time of the London bombings (July 7, 2005).

Figure 2.1: Identification Strategy Overview

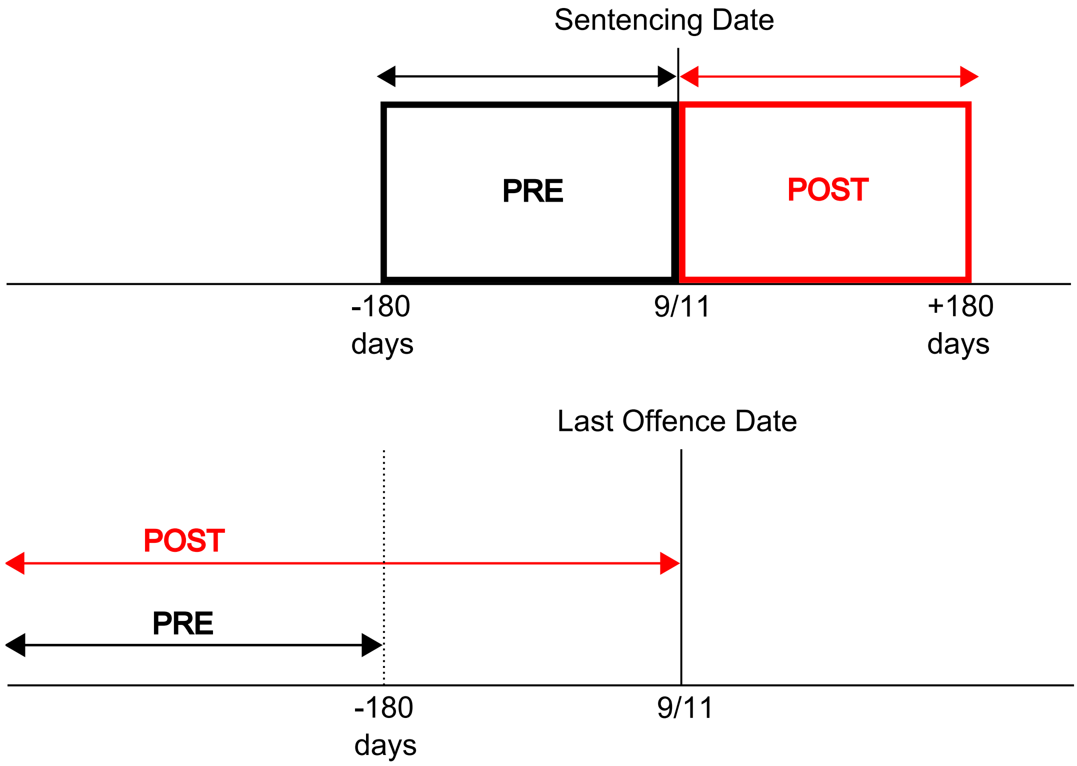
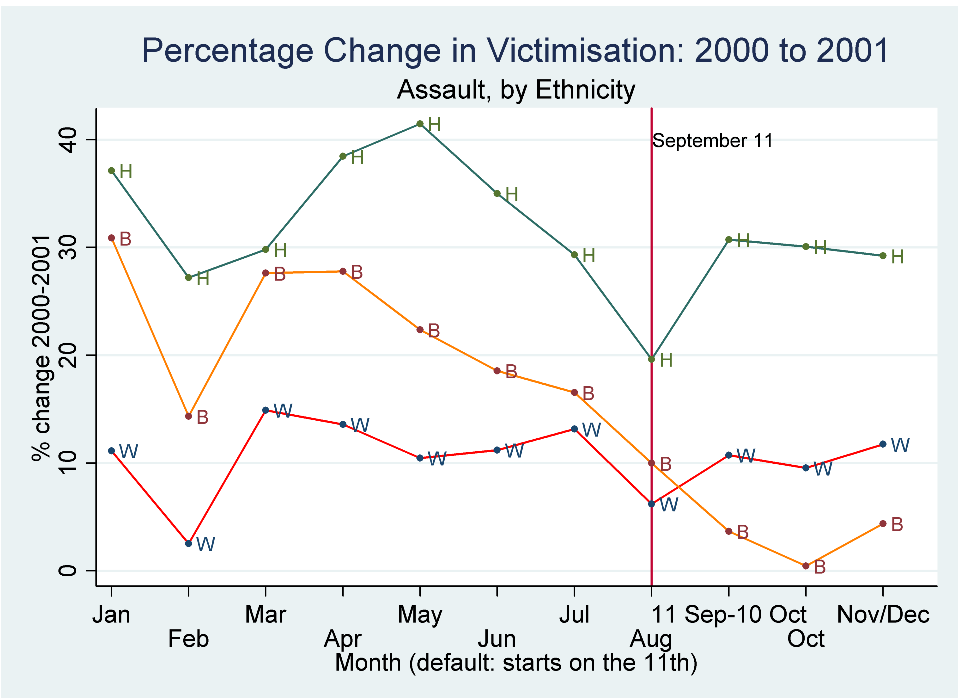


Figure 2.2: Change In Assault Victimisation Rates 2000-2001, by Race

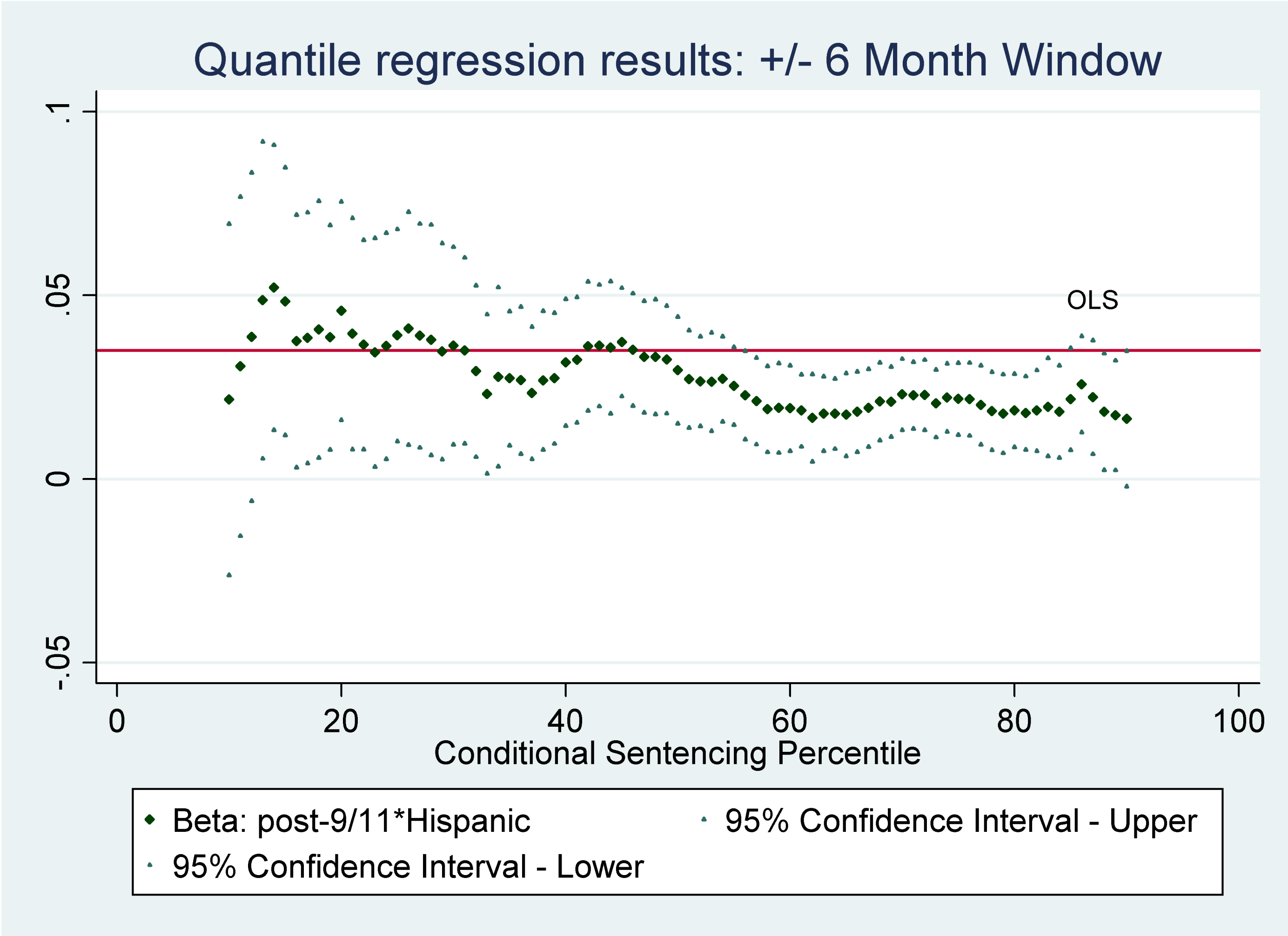


Source: National Incident-Based Reporting System, 2000 and 2001: Extract Files

Dependant Variable: $\frac{Victimisation_{2001}^{Month,Race} - Victimisation_{2000}^{Month,Race}}{Victimisation_{2000}^{Month,Race}}$

Notes: H denotes Hispanic, W denotes white and B denotes black individuals.

Figure 2.3: Quantile Regression Estimates



Notes: Each main point represents the coefficient on post-9/11*Hispanic from a separate quantile regression of the type in equation 2.4. 91 Quantile regressions were run for the 10th to the 90th conditional percentile. The regression sample was the same sample of 39597 males used in all other analysis. Controls included the set of ethnicity and post-9/11 dummies, Individual Characteristics, Guideline cell, Offence Type and District variables (detailed in Table 2.5 Notes).

Table 2.1: United Sates Sentencing Commission Sentencing Table, 2001

		Criminal History Category (Criminal History Points)					
		I	II	III	IV	V	VI
Offence Level		(0 or 1)	(2 or 3)	(4, 5, 6)	(7, 8, 9)	(10, 11, 12)	(13+)
	1	***0-6***	***0-6***	***0-6***	***0-6***	***0-6***	***0-6***
	2	***0-6***	***0-6***	***0-6***	***0-6***	***0-6***	1-7
	3	***0-6***	***0-6***	***0-6***	***0-6***	2-8	3-9
	4	***0-6***	***0-6***	***0-6***	2-8	4-10	6-12
Zone A	5	***0-6***	***0-6***	1-7	4-10	6-12	***9-15***
	6	***0-6***	1-7	2-8	6-12	***9-15***	12-18
	7	***0-6***	2-8	4-10	***8-14***	12-18	15-21
	8	***0-6***	4-10	6-12	***10-16***	15-21	18-24
Zone B	9	4-10	6-12	***8-14***	12-18	18-24	21-27
	10	6-12	***8-14***	***10-16***	15-21	21-27	24-30
Zone C	11	***8-14***	***10-16***	12-18	18-24	24-30	27-33
	12	***10-16***	12-18	15-21	21-27	27-33	30-37
	13	12-18	15-21	18-24	24-30	30-37	33-41
	14	15-21	18-24	21-27	27-33	33-41	37-46
	15	18-24	21-27	24-30	30-37	37-46	41-51
	16	21-27	24-30	27-33	33-41	41-51	46-57
	17	24-30	27-33	30-37	37-46	46-57	51-63
	18	27-33	30-37	33-41	41-51	51-63	57-71
	19	30-37	33-41	37-46	46-57	57-71	63-78
	20	33-41	37-46	41-51	51-63	63-78	70-87
	21	37-46	41-51	46-57	57-71	70-87	77-96
	22	41-51	46-57	51-63	63-78	77-96	84-105
	23	46-57	51-63	57-71	70-87	84-105	92-115
	24	51-63	57-71	63-78	77-96	92-115	100-125
	25	57-71	63-78	70-87	84-105	100-125	110-137
	26	63-78	70-87	78-97	92-115	110-137	120-150
	27	70-87	78-97	87-108	100-125	120-150	130-162
Zone D	28	78-97	87-108	97-121	110-137	130-162	140-175
	29	87-108	97-121	108-135	121-151	140-175	151-188
	30	97-121	108-135	121-151	135-168	151-188	168-210
	31	108-135	121-151	135-168	151-188	168-210	188-235
	32	121-151	135-168	151-188	168-210	188-235	210-262
	33	135-168	151-188	168-210	188-235	210-262	235-293
	34	151-188	168-210	188-235	210-262	235-293	262-327
	35	168-210	188-235	210-262	235-293	262-327	292-365
	36	188-235	210-262	235-293	262-327	292-365	324-405
	37	210-262	235-293	262-327	292-365	324-405	360-life
	38	235-293	262-327	292-365	324-405	360-life	360-life
	39	262-327	292-365	324-405	360-life	360-life	360-life
	40	292-365	324-405	360-life	360-life	360-life	360-life
	41	324-405	360-life	360-life	360-life	360-life	360-life
	42	360-life	360-life	360-life	360-life	360-life	360-life
	43	life	life	life	life	life	life

Source: United States Sentencing Commission, Guidelines Manual, §3E1.1 (Nov. 2001)
Notes: Each cell represents the guideline range for the sentence length (in months) based on offence level and Criminal History category.

Table 2.2: Post-9/11 Survey, selected responses

	Ethnicity			
Affirmative response to:	Middle Eastern	Hispanic	Asian	African-Americ
US World Influence: Too much	57%	58%	56%	27%
Ethnicity: Prefer to be called “American”	19%	11%	11%	38%
Money Making post-9/11: Less Money	29%	37%	36%	18%
Less Money reason: September 11th	75%	72%	75%	54%
Victim of Discrimination: More often	58%	13%	16%	3%
More depressed Post-9/11: Agree	56%	50%	45%	26%
Sample Size	300	200	300	200

Source: “Post 9/11 Survey,” USC Annenberg Institute for Justice and Journalism, July/August 2002
Sample: 1000 “Ethnic” Californians

Table 2.3: Descriptive Statistics - Legal Variables

Mean, standard deviation in parentheses

	Ethnicity:				
Variable	White	Black	Hispanic	Other	Total
Sentence Length	55.15	89.76	48.47	50.56	61.60
	(66.67)	(91.93)	(56.59)	(68.02)	(72.94)
Offence Severity	20.28	23.43	19.88	19.21	20.93
	(7.73)	(8.49)	(7.76)	(8.34)	(8.12)
Criminal History Score	2.45	3.23	2.43	2.02	2.64
	(1.75)	(1.83)	(1.68)	(1.50)	(1.77)
Departure Status:					
None	0.62	0.66	0.59	0.63	0.62
	(0.49)	(0.47)	(0.49)	(0.48)	(0.49)
Upwards	0.01	0.01	0.00	0.03	0.01
	(0.10)	(0.08)	(0.06)	(0.16)	(0.09)
Downwards	0.12	0.07	0.26	0.13	0.16
(Other)	(0.32)	(0.26)	(0.44)	(0.33)	(0.37)
Downwards	0.20	0.20	0.10	0.14	0.16
(Substantial Assistance)	(0.40)	(0.40)	(0.30)	(0.34)	(0.36)
Pre-sentence Status:					
In Custody	0.57	0.73	0.85	0.66	0.73
	(0.50)	(0.45)	(0.36)	(0.47)	(0.44)
Out on bail/bond	0.31	0.21	0.11	0.21	0.19
	(0.46)	(0.40)	(0.31)	(0.41)	(0.40)
Own recognizance	0.10	0.05	0.01	0.10	0.05
	(0.30)	(0.22)	(0.11)	(0.30)	(0.22)
Other	0.01	0.01	0.00	0.02	0.01
	(0.12)	(0.10)	(0.06)	(0.14)	(0.09)
Defence Counsel:					
Privately Retained	0.14	0.08	0.08	0.14	0.10
	(0.35)	(0.27)	(0.27)	(0.35)	(0.30)
Court Appointed	0.18	0.16	0.28	0.25	0.22
	(0.39)	(0.36)	(0.45)	(0.43)	(0.41)
Federal Public Defender	0.14	0.15	0.26	0.19	0.19
	(0.34)	(0.36)	(0.44)	(0.39)	(0.40)
Self Represented	0.00	0.00	0.00	0.00	0.00
	(0.04)	(0.04)	(0.02)	(0.03)	(0.03)
Waived Rights	0.00	0.00	0.00	0.00	0.00
	(0.03)	(0.04)	(0.03)	(0.05)	(0.03)
Other	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.02)	(0.00)	(0.00)	(0.01)
Sample Size	10962	10758	16626	1251	39597

Notes: Figures based on full regression sample of those individuals sentenced within a six month window of 9/11, and i.) if sentenced after September 11, 2001, committed their final offence prior to September 11, 2001 ii.) if sentenced before September 11, 2001, committed their final offence six months prior to September 11, 2001.

Table 2.4: Descriptive Statistics: Non-Legal Variables

Mean, standard deviation in parentheses

	Ethnicity:				
Variable	White	Black	Hispanic	Other	Total
Marital Status:					
Married	0.33	0.20	0.35	0.31	0.30
	(0.47)	(0.40)	(0.48)	(0.46)	(0.46)
Single	0.34	0.54	0.33	0.42	0.39
	(0.48)	(0.50)	(0.47)	(0.49)	(0.49)
Cohabiting	0.08	0.13	0.16	0.11	0.13
	(0.28)	(0.34)	(0.37)	(0.31)	(0.34)
Divorced	0.17	0.06	0.05	0.09	0.09
	(0.38)	(0.24)	(0.23)	(0.29)	(0.28)
Widow	0.00	0.00	0.00	0.00	0.00
	(0.06)	(0.05)	(0.04)	(0.05)	(0.05)
Separated	0.05	0.05	0.05	0.04	0.05
	(0.22)	(0.21)	(0.22)	(0.20)	(0.22)
Number Dependants	1.08	1.71	1.87	1.43	1.59
	(1.42)	(1.83)	(1.80)	(1.73)	(1.74)
Highest Education:					
Less than High School	0.29	0.42	0.65	0.37	0.48
	(0.45)	(0.49)	(0.48)	(0.48)	(0.50)
High School	0.39	0.37	0.18	0.35	0.30
	(0.49)	(0.48)	(0.39)	(0.48)	(0.46)
Some College	0.21	0.17	0.07	0.18	0.14
	(0.41)	(0.38)	(0.26)	(0.38)	(0.35)
College Graduate	0.10	0.03	0.02	0.10	0.05
	(0.30)	(0.17)	(0.14)	(0.29)	(0.21)
Age	37.76	31.52	32.16	33.90	33.59
	(11.63)	(8.96)	(9.09)	(10.95)	(10.23)
Sample Size	10962	10758	16626	1251	39597

Notes: Figures based on full regression sample of those individuals sentenced within a six month window of 9/11, and i.) if sentenced after September 11, 2001, committed their final offence prior to September 11, 2001 ii.) if sentenced before September 11, 2001, committed their final offence six months prior to September 11, 2001.

Table 2.5: Baseline Regression with Varying Regressors

Dependant Variable:	**log(sentence length in months)**				
	OLS				
Black	0.534***	0.387***	0.319***	0.258***	0.055***
	(0.043)	(0.042)	(0.044)	(0.024)	(0.011)
Hispanic	-0.055	-0.159**	-0.176*	-0.078***	0.010
	(0.092)	(0.076)	(0.094)	(0.028)	(0.014)
Other	-0.193**	-0.191**	-0.128*	-0.090	0.025
	(0.090)	(0.093)	(0.069)	(0.063)	(0.023)
Post 9/11	-0.006	-0.006	-0.014	-0.012	-0.004
	(0.022)	(0.020)	(0.022)	(0.020)	(0.009)
Black*Post 9/11	0.016	0.007	0.016	0.009	0.006
	(0.033)	(0.029)	(0.028)	(0.026)	(0.014)
Hispanic*Post 9/11	-0.042	-0.051*	0.002	0.000	0.035**
	(0.030)	(0.028)	(0.026)	(0.023)	(0.014)
Other*Post 9/11	0.018	-0.003	-0.053	-0.033	-0.032
	(0.079)	(0.084)	(0.058)	(0.058)	(0.033)
Observations	39597	39597	39597	39597	39597
R^2	0.06	0.18	0.34	0.39	0.83
Individual Characteristics	No	Yes	Yes	Yes	Yes
Offence Type	No	No	Yes	Yes	Yes
District	No	No	No	Yes	Yes
Guideline Cell	No	No	No	No	Yes

Notes: $***$ denotes significance at 1%, $**$ at 5%, and $*$ at 10%. Standard errors clustered at race-district level. Figures based on full regression sample of those individuals sentenced within a six month window of 9/11, and i.) if sentenced after September 11, 2001, committed their final offence prior to September 11, 2001 ii.) if sentenced before September 11, 2001, committed their final offence six months prior to September 11, 2001.
White defendants are the ethnic base category in the table above, so estimated ethnic effects reflect differences in ethnic-minority outcomes relative to the corresponding white outcomes.
Individual Characteristics include dummies for education level (high School, part-college, college, missing), marital status (single, cohabiting,divorced, widowed,separated, missing), age decile(23-25,26-27,28-29,30-32,33-35,36-38,39-43,44-49,50-87), pre-sentence status(bail,own recognizance, other,missing), number of dependants(1,2-3,4+,missing) and defence counsel(private, federal,self,waived,other,missing). Guideline cell dummies comprise of dummies for all but one combinations of sentence severity and criminal history, totalling 257 dummies. Offence Type includes a dummy for all but one of the 41 Offence Types. District contains 93 dummies for all but one Sentencing District.

Table 2.6: Sentencing and Departures

Dependant Variable:	**log(sentence)**	**1(No Departure=1)**	**log(sentence)**	
Sample:	Full		No Departure	Downwards Departure
	OLS	**LPM**	**OLS**	
Black	0.055***	0.050***	0.005	0.054***
	(0.011)	(0.013)	(0.009)	(0.019)
Hispanic	0.010	0.037***	-0.029***	0.035*
	(0.014)	(0.013)	(0.010)	(0.021)
Other	0.025	0.013	0.034*	-0.014
	(0.023)	(0.029)	(0.020)	(0.055)
Post 9/11	-0.004	-0.022**	0.000	0.007
	(0.009)	(0.011)	(0.009)	(0.020)
Black*Post 9/11	0.006	0.006	0.015	-0.014
	(0.014)	(0.016)	(0.012)	(0.027)
Hispanic*Post 9/11	0.035**	0.030**	0.030***	0.020
	(0.014)	(0.015)	(0.011)	(0.024)
Other*Post 9/11	-0.032	0.007	-0.026	0.013
	(0.033)	(0.032)	(0.021)	(0.083)
Observations	39597	39597	24492	12657
R^2	0.83	0.18	0.92	0.8

Notes: $***$ denotes significance at 1%, $**$ at 5%, and $*$ at 10%. Standard errors clustered at race-district level. Figures based on full regression sample of those individuals sentenced within a six month window of 9/11, and i.) if sentenced after September 11, 2001, committed their final offence prior to September 11, 2001 ii.) if sentenced before September 11, 2001, committed their final offence six months prior to September 11, 2001.
White defendants are the ethnic base category in the table above, so estimated ethnic effects reflect differences in ethnic-minority outcomes relative to the corresponding white outcomes.
1(.=1) denotes an indicator function (equalling 1 if the statement insides the parentheses is true, and 0 otherwise), and represents binary regressors. Marginal effects and the corresponding standard errors from the Linear Probability Models (LPM) were similar in sign and magnitude to the corresponding Probit estimates. LPM estimation was used in order to minimise the assumptions required in this analysis.
In addition to the set of ethnicity and post-9/11 dummies, Individual Characteristics, Guideline cell, Offence Type and District variables (detailed in Table 2.5 Notes) are controlled for in all regressions.

Table 2.7: Sentencing and Departures, by Defence Counsel

Dependant Variable:	**log(sentence)**					
Defence Counsel:	Private		Court Appointed		Federal PD	
Sample:	All	No Departure	All	No Departure	All	No Departure
			OLS			
Black	0.106***	-0.013	0.045*	-0.013	0.028	0.010
	(0.036)	(0.030)	(0.027)	(0.016)	(0.024)	(0.020)
Hispanic	0.034	-0.061***	0.001	-0.025	-0.026	-0.047**
	(0.033)	(0.023)	(0.024)	(0.018)	(0.020)	(0.021)
Other	-0.056	-0.062	-0.023	0.063	0.000	0.005
	(0.055)	(0.070)	(0.048)	(0.042)	(0.063)	(0.046)
Post 9/11	0.006	0.006	0.001	0.014	-0.015	-0.043*
	(0.027)	(0.021)	(0.022)	(0.025)	(0.022)	(0.023)
Black*Post 9/11	-0.003	0.026	-0.003	0.013	0.013	0.058**
	(0.043)	(0.038)	(0.035)	(0.032)	(0.030)	(0.029)
Hispanic*Post 9/11	-0.027	0.016	0.062**	0.034	0.045*	0.082***
	(0.042)	(0.033)	(0.029)	(0.029)	(0.024)	(0.025)
Other*Post 9/11	0.086	0.058	0.068	-0.090*	0.084	0.086*
	(0.085)	(0.080)	(0.068)	(0.047)	(0.072)	(0.046)
Observations	3911	2099	8694	4370	7699	5170
R^2	0.79	0.94	0.84	0.94	0.84	0.92

Notes: $***$ denotes significance at 1%, $**$ at 5%, and $*$ at 10%. Standard errors clustered at race-district level. Figures based on those individuals sentenced within a six month window of 9/11, and i.) if sentenced after September 11, 2001, committed their final offence prior to September 11, 2001 ii.) if sentenced before September 11, 2001, committed their final offence six months prior to September 11, 2001.
White defendants are the ethnic base category in the table above, so estimated ethnic effects reflect differences in ethnic-minority outcomes relative to the corresponding white outcomes.
In addition to the set of ethnicity and post-9/11 dummies, Individual Characteristics (excluding defence counsel dummies), Guideline cell, Offence Type and District variables (detailed in Table 2.5 Notes) are controlled for in all regressions.

Table 2.8: Sample Composition

Dependant Variable:	**Age**	**1(High School=1)**	**Number Dependants**	**1(In Custody Pre-Sentence=1)**	**Criminal History**	**Offence Severity**	**1(Drug Trafficking=1)**	**1(Immigration=1)**
	OLS	**LPM**	**OLS**	**LPM**	**OLS**		**LPM**	
Black	-3.605***	-0.051***	0.761***	0.056***	0.428***	1.175***	0.043***	-0.001
	(0.270)	(0.011)	(0.044)	(0.013)	(0.039)	(0.173)	(0.013)	(0.008)
Hispanic	-3.389***	-0.142***	0.511***	0.168***	-0.523***	0.191	0.100***	0.136***
	(0.269)	(0.013)	(0.037)	(0.011)	(0.041)	(0.215)	(0.017)	(0.013)
Other	-2.065***	-0.034	0.368***	0.052**	-0.454***	0.009	-0.149***	0.011
	(0.434)	(0.022)	(0.075)	(0.021)	(0.091)	(0.441)	(0.030)	(0.021)
Post 9/11	0.221	-0.006	0.006	-0.004	-0.026	-0.026	-0.009	0.005
	(0.216)	(0.009)	(0.026)	(0.009)	(0.030)	(0.128)	(0.010)	(0.004)
Black*Post 9/11	0.337	0.014	0.003	-0.003	0.021	0.045	-0.011	-0.003
	(0.279)	(0.014)	(0.046)	(0.014)	(0.044)	(0.176)	(0.012)	(0.005)
Hispanic*Post 9/11	0.241	0.016	0.025	0.001	0.049	-0.367*	-0.037***	0.051***
	(0.250)	(0.012)	(0.040)	(0.010)	(0.039)	(0.195)	(0.013)	(0.008)
Other*Post 9/11	-0.624	0.011	0.018	0.020	0.003	-0.162	0.042*	-0.006
	(0.643)	(0.022)	(0.078)	(0.023)	(0.086)	(0.357)	(0.023)	(0.016)
Observations	39597	39597	38447	39597	39597	39597	39597	39597
R^2	0.31	0.09	0.27	0.28	0.33	0.44	0.47	0.56

Notes: $***$ denotes significance at 1%, $**$ at 5%, and $*$ at 10%. Standard errors clustered at race-district level.
Figures based on full regression sample of those individuals sentenced within a six month window of 9/11, and i.) if sentenced after September 11, 2001, committed their final offence prior to September 11, 2001 ii.) if sentenced before September 11, 2001, committed their final offence six months prior to September 11, 2001.
White defendants are the ethnic base category in the table above, so estimated ethnic effects reflect differences in ethnic-minority outcomes relative to the corresponding white outcomes.
1(.=1) denotes an indicator function (equalling 1 if the statement insides the parentheses is true, and 0 otherwise), and represents binary regressors. Marginal effects and the corresponding standard errors from the Linear Probability Models (LPM) were similar in sign and magnitude to the corresponding Probit estimates. LPM estimation was used in order to minimise the assumptions required in this analysis.
Here one of the regular regressors is the dependant variable.Individual Characteristics, Guideline cell, Offence Type and District variables (detailed in Table 2.5 Notes) controlled for in all regressions, apart from those relevant to the dependant variable itself. For instance, if 1(Offence=drug trafficking) is the dependant variable, then Individual Characteristics, Guideline cell and District variables also controlled for in this regression.

Table 2.9: Duration Analysis

Dependant Variable:	**(sentence date-last offence date)**					
Sample:	Full		Drugs Trafficking	Immigration	Drugs Trafficking	Immigration
	Weibull PH, Gamma Frailty	**Cox semi-parametric PH**	**Weibull PH, Gamma Frailty**		**Cox semi-parametric PH**	
Black	0.093	0.025	-0.176	0.645	-0.021	0.105
	(0.071)	(0.028)	(0.118)	(0.425)	(0.039)	(0.193)
Hispanic	0.421	0.150	0.405	0.246	0.191	0.158
	(0.099)**	(0.036)**	(0.110)**	(0.221)	(0.038)**	(0.078)*
Other	-0.238	-0.123	-0.565	0.364	-0.067	-0.107
	(0.203)	(0.066)	(0.477)	(0.839)	(0.114)	(0.256)
Post 9/11	-0.045	-0.028	0.055	0.062	-0.023	0.118
	(0.061)	(0.018)	(0.111)	(0.363)	(0.041)	(0.097)
Black*Post 9/11	-0.059	-0.022	-0.074	-0.458	-0.013	-0.141
	(0.078)	(0.027)	(0.138)	(0.618)	(0.049)	(0.191)
Hispanic*Post 9/11	-0.145	-0.038	-0.284	-0.365	-0.090	-0.152
	(0.084)	(0.030)	(0.130)*	(0.396)	(0.050)	(0.104)
Other*Post 9/11	0.012	-0.021	0.217	-0.852	-0.034	-0.102
	(0.159)	(0.055)	(0.498)	(0.637)	(0.129)	(0.185)
Observations	45128	45128	19911	7031	19911	7031

Notes: $**$ denotes significance at 1%, $*$ at 5%. Standard errors clustered at race-district level.
Figures based on full regression sample of those individuals sentenced within a six month window of 9/11, and i.) if sentenced after September 11, 2001, committed their final offence prior to September 11, 2001 ii.) if sentenced before September 11, 2001, committed their final offence six months prior to September 11, 2001.
White defendants are the ethnic base category in the table above, so estimated ethnic effects reflect differences in ethnic-minority outcomes relative to the corresponding white outcomes.
In addition to the set of ethnicity and post-9/11 dummies, Individual Characteristics, Guideline cell, Offence Type and District variables (detailed in Table 2.5 Notes) are controlled for in the first 2 columns, Individual Characteristics, Guideline cell and District variables in the last 4 columns.
Regression coefficients reported. A lower coefficient means a lower hazard rate, which in turn means a longer duration.
Weibull PH, Gamma Frailty denotes a Weibull Proportional Hazard model, with Gamma-distributed unobserved heterogeneity or “frailty”. Cox semi-parametric PH denotes a Cox semi-parametric Proportional Hazard model.

Table 2.10: Placebo Regressions

Dependant Variable:	**log(sentence length in months)**				
Placebo Date:	11/03/99	11/09/99	11/03/00	11/09/00	11/03/01
			OLS		
Black	0.045***	0.032**	0.068***	0.093***	0.045***
	(0.012)	(0.013)	(0.014)	(0.016)	(0.013)
Hispanic	0.051***	0.000	0.037**	0.057***	0.032**
	(0.015)	(0.016)	(0.015)	(0.019)	(0.013)
Other	0.036	0.012	0.062*	0.042	0.041
	(0.030)	(0.029)	(0.035)	(0.044)	(0.029)
Post-Placebo	0.023**	-0.054***	-0.013	0.069***	0.009
	(0.011)	(0.014)	(0.014)	(0.017)	(0.009)
Black*Post-Placebo	0.001	0.049***	0.017	-0.053***	0.006
	(0.015)	(0.017)	(0.018)	(0.018)	(0.013)
Hispanic*Post-Placebo	-0.035**	0.047***	0.004	-0.053**	-0.026*
	(0.016)	(0.016)	(0.018)	(0.024)	(0.014)
Other*Post-Placebo	-0.012	0.045	-0.007	0.000	-0.013
	(0.034)	(0.033)	(0.038)	(0.038)	(0.026)
Observations	32521	36426	37894	38626	36024
R^2	0.83	0.79	0.76	0.78	0.82

Notes: $***$ denotes significance at 1%, $**$ at 5%, and $*$ at 10%. Standard errors clustered at race-district level. Figures based on full regression sample of those individuals sentenced within a six month window of placebo date, and i.) if sentenced after placebo date, committed their final offence prior to placebo date ii.) if sentenced before placebo date, committed their final offence six months prior to placebo date.
White defendants are the ethnic base category in the table above, so estimated ethnic effects reflect differences in ethnic-minority outcomes relative to the corresponding white outcomes.
In addition to the set of ethnicity and post-placebo date dummies, Individual Characteristics, Guideline cell, Offence Type and District variables (detailed in Table 2.5 Notes) are controlled for in all regressions.

Table 2.11: JMP Decomposition of pre- to post-9/11 changes in ethnic sentencing differentials

Dependant Variable:	**log(sentence length in months)**	
Sentencing differential:	Black-White	Hispanic-White
Part A: Main results		
Pre-9/11 (raw) differential	0.5341	-0.0550
Post-9/11 (raw) differential	0.5506	-0.0969
Change in differential	0.0165	-0.0419
due to observables	0.0165	-0.0828
due to unobservables	0.0000	0.0409
Observable quantity: X-effect	0.0098	-0.0855
Observable penalties: β-effect	0.0090	-0.0030
Observable interaction	-0.0023	0.0057
Unobservable quantities: θ-effect	-0.0057	0.0378
Unobservable penalties: σ-effect	-0.0045	-0.0005
Unobservable interaction	0.0102	0.0036
Part B: Further decomposition of observables		
Observable quantity:		
All Xs	0.0098	-0.0855
- Individual Characteristics	0.0017	0.0041
- District Variables	0.0015	0.0029
- Guideline Group Cells	0.0062	-0.0887
- Offence Variables	0.0005	-0.0038
- Offence=Drug Trafficking	0.0008	0.0027
- Offence=Immigration	-0.0004	-0.0060
Observable penalties:		
All βs	0.0090	-0.0030
- Individual Characteristics	-0.0036	-0.0251
- District Variables	0.0118	-0.0257
- Guideline Group Cells	-0.0039	0.0011
- Offence Variables	0.0048	0.0467
- Offence=Drug Trafficking	0.0072	0.0063
- Offence=Immigration	-0.0010	0.0368

Notes: The differentials above are noted to be "raw" or unconditional sentencing differentials: group average differentials that do not account for any legal or non-legal controls. These correspond most closely to the first column of results in 2.5.
Figures based on full regression sample of those individuals sentenced within a six month window of 9/11, and i.) if sentenced after September 11, 2001, committed their final offence prior to September 11, 2001 ii.) if sentenced before September 11, 2001, committed their final offence six months prior to September 11, 2001.
Individual Characteristics, Guideline cell, Offence Type and District variables (detailed in Table 2.5 Notes) are controlled for in both decompositions.

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Chapter 3

Crime and the Depenalization of Cannabis

Crime and the Depenalization of Cannabis Possession: Evidence from a Policing Experiment[1]

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Abstract

We evaluate the impact on crime of a localized policing experiment that depenalized the possession of small quantities of cannabis in the London borough of Lambeth. We find that depenalization policy caused the police to reallocate effort towards non-drug crime. Despite the overall fall in crime attributable to the policy, we find the total welfare of local residents likely fell, as measured by house prices. We shed light on what would be the impacts on crime of a citywide depenalization policy, by developing and calibrating a structural model of the market for cannabis and crime.

Keywords: cannabis, crime, depenalization, police behavior.

JEL Classification: H75, J18, K42.

3.1 Introduction

In nearly every country the market for illicit drugs remains pervasive, despite long running attempts to restrict such activities. Around the globe various policy approaches have been tried, ranging from punitive approaches as manifested in the US 'war on drugs', to more liberal law enforcement strategies, such as those in Holland or Portugal, that lead to the decriminalization or depenalization of the possession of some forms of illicit drug, most notably cannabis.[2]

Both approaches have been criticized on theoretical and empirical grounds [Glaeser and Shleifer 2001, Becker *et al.* 2006]: the historically tough US policy stance is estimated to cost tens of billions of dollars annually, and there remain an estimated 3.7 million individuals regularly using illicit drugs, the majority of whom consume cannabis [DHHS 2008]. At the same time, concerns over more liberal policy strategies relate to the inherent characteristics of the illicit drugs market: consumption might damage user's health [Arseneault *et al.* 2004, van Ours and Williams 2009]; the use of some drugs might provide a gateway to more addictive drugs [van Ours 2003]; and there are potentially large spillover effects on crime and other forms of anti-social behavior.

We contribute to this policy debate by evaluating an increasingly common policy intervention in the illicit drug market: the depenalization of cannabis possession, so that the possession of small quantities of cannabis is no longer a criminally prosecutable offence. We present evidence from a localized UK policing experiment that introduced such a policy and focus attention on measuring its impact on crime, considered to be a major social cost of illicit drug markets.

Criminal activity and drug markets might be linked because: (i) the substance itself leads to more violent or criminal behavior by users; (ii) users commit property crimes to obtain money to buy drugs; (iii) violence occurs between drug suppliers to control selling areas. We present evidence over a broad range of crime types to assess the impact of depenalization both on the size of illicit drugs markets for cannabis and harder drugs, as

[2] Donohue *et al.* [2011] categorize illicit drug policies into three types: (i) legalization – a system in which possession and sale are lawful but subject to regulation and taxation; (ii) criminalization – a system of proscriptions on possession and sale backed by criminal punishment, potentially including incarceration; (iii) depenalization – a hybrid system, in which sale and possession are proscribed, but the prohibition on possession is backed only by such sanctions as fines or mandatory substance abuse treatment, not incarceration. Following Donohue *et al.* [2011] we prefer the use of depenalization over decriminalization as best describing the policy experiment we evaluate, and closely mapping into the definition of depenalization used by criminologists.

well as the policy impact on non-drug crime such as property and violent crime.[3]

The depenalization policy we evaluate was unilaterally introduced by the local police force in one London borough, Lambeth, in July 2001, a policy known as the Lambeth Cannabis Warning Scheme (LCWS). We describe the motivation behind the policy and its implementation in more detail later. It is however worth noting that many aspects of the policy reflect how other depenalization policies have been implemented around the world: (i) the possession of small quantities of cannabis for personal consumption was still a recordable offence, but would no longer lead to the individual being arrested; (ii) the primary motivation was to free up police time and other resources to focus on crimes related to other drugs or other non-drug related crimes; (iii) the policy did not alter penalties for cannabis supply.

The LCWS was first announced as a temporary policing experiment to run for six months from July 2001. At the end of this trial period the policy was adjudged to have been a success with the support of local residents. The policy was then announced to have been extended for a further six months. Following this announcement, media reports of the deleterious effects of the policy on crime, drug tourism, and drug use by children began to steadily increase. As local support for the LCWS waned, the policy came to an end by July 2002, having run for 13 months. We use these various policy switches to assess the short and long run effects of the depenalization policy on the levels and composition of drug crime and non-drug crime.

When evaluating localized policy interventions in illicit drug markets, it is important to recognize interlinkages between drug markets: the equilibrium market size for cannabis in a given location is partly a function of the endogenous choices of police and cannabis users in *other* locations. More precisely, a localized depenalization policy in Lambeth will likely: (i) impact the size of the market for cannabis in Lambeth as well as the rest of London as drug users move there to purchase cannabis; (ii) enable the Lambeth police to reallocate effort towards other types of crime, consequently impacting the number of

[3]The size of drug markets has previously been linked to crime rates [Grogger and Willis 2000, Pacula and Kilmer 2003], especially for property crime [Corman and Mocan 2000]. On users, Fergusson and Horwood [1997] report evidence of a link between the early onset of cannabis use and subsequent crime using longitudinal data for a birth cohort of New Zealand children. Early onset users had significantly higher rates of later substance use, juvenile offending, mental health problems, unemployment and school dropout. On cannabis and violence, there is no clear evidence between the two as cannabis is usually thought to inhibit aggressive behavior [Resignato 2000]. On crimes by drug suppliers, Kuziemko and Levitt [2004] find that incarcerating drug offenders is almost as effective in reducing violent and property crime as locking up other types of offenders. Levitt and Venkatesh [2000] show that workers in the illicit drugs market are not particularly well remunerated and so pursuing property crime might provide additional income and the flexibility to continue working in the drugs trade.

drug and non-drug related crime in all locations.[4]

We investigate whether such changing patterns of crime and police behavior are observed during and after the depenalization policy is introduced in Lambeth. To do so, we use administrative records obtained from the London Metropolitan Police Service (MPS) to construct a panel data set on crime for all 32 London boroughs, for each month from April 1998 until January 2006. This contains information on the number of recorded drug offences at two fine levels of detail: (i) the number of criminal offences related to any given *drug type*, e.g. cannabis, heroin, cocaine etc.; (ii) for each drug type, the *specific offence* committed: possession, trafficking, intent to supply etc. Such detailed measurement of drug crime allows us to assess the impact of the policy on the size of cannabis market (as proxied by the total number of cannabis offences), and whether the change in market size is predominantly driven by changes in demand-related offences such as cannabis possession, or by supply-related offences such as cannabis trafficking etc.

A depenalization policy can free up police resources to tackle non-cannabis drug crime. The disaggregated drug crime data we exploit allows us to specifically measure such effects on other illicit drug markets, not just the direct effects on the market for cannabis, as well as for seven types of non-drug crime: violence against the person, sexual offences, robbery, burglary, theft and handling, fraud and forgery, and criminal damage. Finally, we note that the administrative records also contain information on two measures more closely correlated to police behavior for each disaggregated crime type: the number of individuals arrested, and the number of crimes cleared-up. These margins help provide evidence on how *police effectiveness* across crime types changes in response to the depenalization policy.

We present four classes of results. First, the depenalization of cannabis in Lambeth leads to a significant increase in cannabis related crime: offence rates for cannabis related crime rise by 29.3% more in Lambeth relative to the rest of London between the pre-policy and policy period; comparing the pre-policy and post-policy periods, they are 61.0% higher in Lambeth vis-à-vis the rest of London. This longer term effect persists well after the policy experiment ends. At the same time, we document significant falls in police effectiveness against cannabis related crime, that also persist well after the policy

[4]This potential reallocation of police effort across crime types has been hinted at in previous studies. For example Single [1989] notes that following depenalization in California, there is some evidence that the police targeted non-cannabis crime to a greater extent.

officially ends.

Second, we find some evidence the policy causes the police to reallocate their effort towards crimes relating to the supply of hard drugs, such as heroin, crack and cocaine (that are known as 'Class-A' drugs in the UK drug classification system). However, the primary benefit of the policy is that it allows the Lambeth police to reallocate their effort towards *non-drug* crime: we observe significant reductions in five out of seven other crime types in the long run, and significant improvements in police effectiveness against such crimes, as measured by arrest and clear-up rates.[5] Overall, these channels cause total non-drug crime to fall by 9.4% in the long term in Lambeth relative to the rest of London. This reduction occurs against a backdrop of unchanging offence rates for non-drug crime in the post-policy period for the rest of London.

Our third class of results document the *welfare* impacts of the depenalization on local residents. The welfare effects of the policy are *a priori* ambiguous: although it caused total crime to fall, it also led to a dramatic change in the composition of crime. There was an increase in cannabis related offences, but the rates of many other types of crime fell in the longer term. To estimate the overall impact of the policy through these changing crime patterns, as well as through other non-crime channels, we estimate policy impacts on house prices in Lambeth relative to other London boroughs. Intuitively, the total social cost of depenalization (not just those costs arising from crime) should be reflected in house prices [Rosen 1974, Thaler 1978].

We find that despite the overall fall in crime attributable to the policy, the total welfare of local residents likely fell, as measured by house prices. These welfare losses are concentrated in Lambeth zip codes where the illicit drug market was most active. We provide a lower bound estimate of the loss in property values in Lambeth (that has around 280,000 residents and 119,000 property units) due to the policy to be around £200mn.

Our final set of results use the lessons from the localized policing experiment to shed light on the likely impacts on crime if the same policy were to be applied *citywide*. To do so we develop and calibrate a structural model of the market demand for cannabis and non-

[5] Section 2.1 describes in far more detail the definitions of each monthly crime series data related to offences, arrests and clear-ups. Here we note that we define the offence rate, for a given crime, as the number of offences per 1000 of the adult population (aged 16 and above). As individuals are not necessarily immediately arrested for offences committed, we define the arrest rate as the number of arrests in period t divided by the number of offences committed between month t and the previous quarter within the borough. The clear-up rate is analogously defined.

drug crime, accounting for the behavior of police and cannabis users. The model makes precise interlinkages across cannabis markets, where the number of individuals purchasing cannabis from a given location depends on the policing strategies in all locations. With citywide depenalization, an important mechanism driving the impacts of the localized policing experiment: the movement of cannabis users towards Lambeth to purchase cannabis, is shut down. Due to this, the counterfactual policy simulation highlights that many of the gains of the policy can be retained, and some of the deleterious consequences ameliorated, if all jurisdictions simultaneously depenalize cannabis possession.

Our study builds on the evidence on the effects of depenalization or decriminalization policies on crime. MacCoun and Reuter [2001] review these studies and find positive but modest impacts. One reason for the difference with our findings stems from our research design exploiting within *and* across borough variation in crime, rather than being based on nationwide policy changes. US studies have exploited the fact that in the 1970s some states depenalized cannabis and found weak impacts on crime [NRC 2001]. However, Pacula *et al.* [2004] have questioned such studies because, "[so called] decriminalized states are not uniquely identifiable based on statutory law as has been presumed by researchers over the past twenty years".

We contribute to this literature by exploiting a localized policy change and using detailed administrative records on crime and police behavior. Our evidence provides a nuanced picture of the impacts of an increasingly observed policy, the depenalization of cannabis: (i) across crimes related to cannabis, Class-A drugs, and seven non-drug crime types; (ii) on measures of police behavior, by assessing its impact on arrest and clear-up rates; (iii) across time, by assessing the short and long run impacts of the LCWS; (iv) on welfare, as measured by house prices, and how this varies *within* Lambeth depending on the prevalence of the illicit drug market across different zip code sectors in Lambeth. Taken together with our structural model estimates, these results provide new evidence relevant to the policy debate on interventions in illicit drug markets.[6]

The paper is organized as follows. Section 2 describes the motivation behind the LCWS, and reasons for its ending. Section 3 describes our administrative data and

[6]We also contribute to the literature examining the impact of drug policies on drug usage. The earlier evidence is mixed: some studies find little evidence of increased drug usage either in the UK [Warburton *et al.* 2005, May *et al.* 2007a, Pudney 2010] or other countries [Single 1989, DiNardo and Lemieux 2001, MacCoun and Reuter 2005, Hughes and Stevens 2010], and others finding slight increases [Williams 2004, Damrongplasit *et al.* 2010]. Our reduced form results suggests there might have been a considerable increase in the equilibrium market size for cannabis in Lambeth. The structural model sheds light on how total usage might vary with citywide depenalization.

empirical method. Section 4 presents the results on the impact of depenalization on cannabis crime. Section 5 investigates how the policy impacts other drug crime, and non-drug crime. Section 6 uses house price information to provide a hedonic evaluation of the depenalization policy. This sheds light on how Lambeth residents value the total social effects of depenalization in the long run, not just those operating through changes in crime. In Section 7 we shed light on what would be the impacts on crime if the same policy were to be applied *citywide*, by developing and calibrating an equilibrium model of crime and the demand for cannabis. Section 8 concludes. The Appendix contains further information related to the crime and housing data, and further robustness checks.

3.2 The Lambeth Cannabis Warning Scheme (LCWS)

3.2.1 Background

To understand why the LCWS policing experiment was introduced in Lambeth in July 2001, we need to go back to the earlier UK policy debate stimulated by the publication of the Runciman Report in 2000. This was a high profile inquiry commissioned by the Police Foundation, whose remit was to review and suggest amendments to the primary piece of UK legislation governing the policing of illicit drugs: the Misuse of Drugs Act 1971. This laid out the three-tiered drug classification system used in the UK, with assignment from Class-C to Class-A intended to indicate increasing potential harm to users: Class-A drugs are cocaine, crack, crystal-meth, Heroin, LSD, MDMA and methadone; Class-B drugs are amphetamines and cannabis; Class-C drugs are anabolic steroids, GHB, and ketamine. The Runciman Report called for the classification system to more closely follow the scientific evidence of relative harms, and consequently that cannabis be reclassified from a Class-B to a Class-C drug. The report emphasized three benefits of doing so: (i) reduced numbers of individuals being criminalized; (ii) removing a source of friction between the police and local communities; (iii) freeing up police time.

Subsequent to the Runciman Report, the Metropolitan Police Service (MPS) produced their own report on drugs policing, 'Clearing the Decks.' This suggested the idea of a workable depenalization policy in May 2000. This report again emphasized that such a policy might enable the police to divert resources towards areas of high priority if they were willing to explore alternatives to arrest for a number of minor crimes, including possession of cannabis. The notion that such a depenalization policy might actually be implemented within London began to take hold a year later in early 2001, when the police commander for the London borough of Lambeth, Brian Paddick, conducted a staff consultation exercise on drugs policing strategy. During the consultation, officers complained they spent a considerable amount of time dealing with arrests for cannabis possession and this detracted from their ability to deal with high priority crime such as street crime, to tackle Class-A drugs, and to respond to emergency calls.[7]

With the sanctioning of the Metropolitan Police Commissioner, Sir John Stevens, the LCWS was introduced in Lambeth on July 4th 2001 as a pilot project that was intended to run for six months. Under the scheme, those found in possession of small quantities of cannabis for their personal use: (i) had the drugs confiscated; (ii) an offence was still recorded, although individuals were given a warning rather than an arrest being recorded – prior to the policy such individuals would have been arrested [Dark and Fuller 2002]. To be clear, the policy was designed to lead to no change in how the police should record offences related to cannabis possession, all else equal. Rather, it would reduce the penalties to offending individuals such that they would not be arrested. As such, the LCWS had all the hallmarks of many policies trialed around the world that have sought to depenalize rather than decriminalize the possession of small quantities of cannabis [Donohue *et al.* 2011].

There are various mechanisms through which such a depenalization policy can impact drug crime, depending on whether and how such policies alter the behavior of the police, cannabis users, and local residents. As emphasized throughout, it is likely the policy induced changes in police behavior: under the policy the police can effectively reallocate resources from cannabis related crime to other crimes. This has the obvious benefit that it allows the police to better deal with non-drug related crime, and should be evident in falling offence rates for other crimes and rising police effectiveness against such non-drug crime.[8]

Second, such changes in police behavior will induce endogenous changes in behavior among cannabis users who perceive reduced penalties for being caught in possession of

[7] Police officers also reported concerns, following a recent disciplinary case, that they might face formal sanctions if they continued to follow a long-standing unofficial practice of dealing with people found in possession of cannabis by informally warning them and destroying the drugs on the streets. Pre-policy, such actions did not have official sanction [May *et al.* 2002, Warburton *et al.* 2005, May *et al.* 2007a].

[8] Of course the behavior of illicit drug suppliers could also alter with depenalization. However, given the lack of information on the supply side, and no reliable time series on drug prices by London borough, for the bulk of our analysis we do not focus on this channel. We return to this issue in the conclusion.

cannabis in Lambeth. As emphasized in the structural model developed later, such users might originate from Lambeth or other parts of London. If users assume there to be lower penalties of being caught in possession of almost *any* quantity of cannabis, then offence rates for cannabis possession should rise with the LCWS because the possession of such larger quantities of cannabis would still be recorded as an offence and still lead to an arrest.[9] Alternatively, the lower penalties might induce some individuals to *start* using cannabis. If such new users then choose to possess sufficiently large quantities, this would again cause recorded cannabis offences to increase with the policy, all else equal. Hence changes in police behavior can explain both a simultaneous increase in cannabis related crime and a reduction in other types of non-drug crime.

An alternative scenario is if any changes in police behavior induce no change in the behavior of cannabis users, neither in terms of whether to purchase cannabis, nor where to purchase it from. The LCWS should then lead to no change in recorded offences in cannabis possession and *mechanically reduce* arrest and clear-up rates for cannabis possession: behaviors that previously would have been recorded as offences would continue to be classified as such, but the LCWS policy would lead to the number of arrests and clear-ups for cannabis possession falling in this scenario.

Absent any changes in behavior among cannabis users, changes in offence rates for cannabis possession might also occur through what criminologists refer to as a 'net-widening effect' that operates through changes in police *reporting* behavior [Christie and Ali 2000, Warburton *et al.* 2005, May 2007a]. This states that depenalization policies allow the police to start formally dealing with cannabis offences where previously they might have issued informal warnings and no offence recorded. Indeed, given the documented heterogeneity in behavior of individual police officers in relation to drugs policing [May 2007a], we would certainly expect some element of net-widening to occur under the LCWS. In consequence, the LCWS would cause recorded offence rates for cannabis possession to increase. This channel alone does *not* suggest any impact on arrest and clear-up rates for cannabis possession, nor does it imply any change in police effectiveness against non-drug crime.

Finally, the policy might also induce changes in reporting behavior among local

[9] Indeed, in an MPS review of the LCWS policy, Dark and Fuller [2002] note the ambiguity officers themselves faced in regards to establishing a clear threshold for what constituted a small quantity of cannabis possessed. Christie and Ali [2000] report that in the context of depenalization in South Australia, small quantities corresponded to less than $100g$ of cannabis or $20g$ of cannabis resin.

residents. If they view the policy as signalling the police were devoting less effort towards cannabis related crimes, residents might then be less inclined to report incidents involving cannabis possession. All else equal, this would cause a *reduction* in recorded cannabis offences, but this channel alone should have no impact on arrest and clear-ups rates for cannabis possession, nor on the incidence of non-drug crime. As we sequentially present evidence on the impacts of the LCWS policy on cannabis offences, on measures of police effectiveness related to cannabis crime, and on the incidence and police effectiveness against other types of non-drug crime, we will be able to narrow down the likely dominant channels through which the policy operates. It is these first order channels we then capture in our structural model, that allows us to take the key lessons from the localized LCWS policing experiment and predict the likely impacts of a counterfactual citywide depenalization policy.

2.2 Initial Public Reaction and the Evolution of the Policy

To gauge the initial local public reaction towards the LCWS, an IPSOS-MORI poll was commissioned during the six month policy experiment. This found broad support for the scheme among locals: 36% of surveyed residents approved outright of the policy; a further 47% approved provided the police actually reduced serious crime in Lambeth. Following this ground swell of support, at the end of the trial period, the policy was then announced to have been extended for a further six months. It is plausible this extension might have been interpreted by cannabis users and the police as representing a permanent change in drug policing strategy.

Anecdotal evidence then suggests local support for the scheme began to decline once the policy was announced to have been extended beyond the initial pilot. Media reports cited that local opposition arose due to concerns that children were at risk from the scheme, and that the LCWS had led to an increase in drug tourism in Lambeth. The LCWS formally ended on 31st July 2002. In part because of disagreements between the police and local politicians over the policy's true impacts, post-policy Lambeth's cannabis policing strategy did not return identically to what it had been pre-policy. Rather, it adjusted to be a firmer version of what had occurred during the pilot. More precisely, the MPS announced that in Lambeth officers would continue to record offences for cannabis possession, and they would continue to issue warnings rather than necessarily arrest those in possession of cannabis, but would now also have the discretion to arrest where the offence was aggravated. Aggravating factors included: (i) if the officer feared disorder; (ii) if the person was openly smoking cannabis in a public place; (iii) those aged 17 or under were found in possession of cannabis; (iv) individuals found in possession of cannabis were in or near schools, youth clubs or children's play areas.

3.2.2 Other Police Operations

To place the LCWS into the wider context of other police operations conducted in London, we have constructed a novel panel dataset of police operations by London borough-month for our sample period. This is described in Table A1: As shown in Panel A, for each borough specific police operation, we note the type of criminal offence targeted and dates of operation. Some operations occur like the LCWS, within one borough; others are coordinated across boroughs. The length of police operations varies between a few months and two years. There is no evidence of a spike in police operations immediately after the LCWS is introduced, to perhaps reinforce or compensate for its effects. Panel B shows borough specific police operations for which we have incomplete information on their dates of operation: many of these also operate within a single borough. Panel C shows police operations that are London wide. Panel D records police operations that are referred to in Metropolitan Police Authority (MPA) reports, but that we have insufficient detail on to code in Panels A to C. Overall, there is little evidence from Table A1 suggesting the impacts of the LCWS could be confounded with other police operations. In the Appendix we show the robustness of our baseline results when these other police operations are explicitly controlled for.

3.3 Data, Descriptives and Empirical Method

3.3.1 Data Sources

We exploit two sources of data to analyze how the LCWS impacted crime in each London borough. First, we use administrative records obtained from the London Metropolitan Police Service (MPS) to construct monthly panel data sets for various crime related series. For any criminal act – such as the supply of cannabis – the administrative records provide information on three crime series: the number of offences, the number of arrests, and the number of clear-ups. Each crime series panel covers all 32 London boroughs for each month from April 1998. The crime series cover drug related crime as well as seven broad categories of non-drug crime: violence against the person, sexual offences, robbery, burglary, theft and handling, fraud and forgery, and criminal damage.

Second, we use the *Quarterly Labor Force Survey Local Area* (QLFS-LA) data to obtain borough level demographic and labor market characteristics. We interpolate this quarterly data to the borough-month level, and use this to define our main outcome variable, offence rates for any given crime: the number of recorded offences for that crime per 1000 of the adult population (aged 16 and over). We also use the QLFS-LA data to control for demographics and unemployment rates at the borough-month level in our empirical specifications, as described later.

3.1.1 Crime Data: Series Definitions

We describe the core definitional issues related to each crime series, focusing on: (i) official Home Office guidelines for the recording of criminal offences; (ii) the link between offences and arrests data; (iii) the use of warnings by the police; (iv) the definition of clear-ups and their link to arrests data.[10] The Appendix documents some of the important changes the Home Office has instigated in the way in which offences and clear-ups are defined over our study period. Such nationally determined definitional changes in crime series data apply equally in *all* London boroughs, and so do not explain differences over time between Lambeth and other London boroughs.

Home Office guidelines state that as a result of a reported incident, whether from victims, witnesses or third parties, the incident will be recorded as a crime by the police for offences against an identified victim if, on the balance of probability: (a) the circumstances as reported amount to a crime defined by law (the police will determine this, based on their knowledge of the law and counting rules), and; (b) there is no credible evidence to the contrary. For offences against the state, the points to prove to evidence the offence must clearly be made out, before a crime is recorded.

There are additional guidelines specifically related to how drug offences are counted. While these do not appear to provide any exceptions to the above instructions for how drug related offences are recorded, these additional guidelines make clear that: (i) the general rule is one crime per offender, so for example, a stop and search of three individuals all carrying cannabis will lead to three recordings of cannabis possession; (ii)

[10] The Home Office is the UK government department that set the crime recording rules in our study period. It corresponds most closely to the Departments of Homeland Security and Department for Justice in the US.

when an individual is found to be carrying more than one drug, the most serious class of drug possessed is that recorded; (iii) if an individual is found with several Class-B drugs including cannabis, this is recorded as a cannabis offence.[11]

On the link between offences and arrests, a recorded offence of cannabis possession need not translate into an arrest if, for example, a member of the public witnesses the offence, but by the time the police show up to the scene (if at all) there are no individuals to arrest. Hence there can be a wedge between the number of offences and the number of arrests, and the size of this wedge differs across crime types because, for example, crimes vary in the extent to which: (i) they are reported by witnesses; (ii) they bring victims and perpetrators into direct contact etc.

On the issuance of warnings by police (rather than arrests), we note that for the bulk of our study period, warnings for cannabis possession were not separately recorded for all boroughs. From our correspondence with the statistical office of the MPS, they have also confirmed that during the period in which the LCWS was in operation, actual cannabis possession offences would continue to be recorded, but no arrests made or clear-up recorded. This is precisely as the policy was originally designed.[12] Hence, if the behavior of cannabis users remains unchanged, then the introduction of the LCWS policy should lead to *no* change in recorded offences for cannabis possession: this is because policy was designed and practiced to lead to no change in how the police should record offences related to cannabis possession, all else equal. However, under the policy, arrest and clear-up rates for cannabis possession should mechanically decline given such incidents have been depenalized under the LCWS.

Finally, for any crime to be counted as a clear-up, Home Office guidelines state that sufficient evidence must be available to claim a clear-up, and the following conditions must be met: (i) a notifiable offence has been committed and recorded; (ii) a suspect has been identified and has been made aware that they will be recorded as being responsible

[11] Home Office guidelines are available here (accessed Sunday June 9th 2013): www.gov.uk/government/uploads/system/uploads/attachment_data/file/177103/count-general-april-2013.pdf

[12] The *Crime in England and Wales 2006/7* Report states that, "From 1 April 2004 information on police formal warnings for cannabis possession started to be collected centrally as part of the information held (prior to this a pilot scheme was run in parts of London). Those aged 18 and over who are caught in simple possession of cannabis can be eligible for a police formal warning which would not involve an arrest. An offence is deemed to be cleared up if a formal warning for cannabis possession has been issued in accordance with guidance from the Association of Chief Police Officers." Hence for the bulk of our study period (that runs from April 1998 until January 2006) warnings for cannabis possession are not separately recorded for all boroughs.

for committing that crime and what the full implications of this are; (iii) a sanctioned clear-up or non-sanctioned clear-up method applies. In consequence, not every case where the police know, or think they know, who committed a crime can be counted as a clear-up, and some crimes are counted as a clear-up even when the victim might view the case as being far from solved. In short, a clear-up means that the case was closed, whether or not anyone was actually sentenced.

Hence, the primary reason why the series for arrests and clear-ups can diverge is because an individual is arrested for an offence, but is not charged.[13] The relative frequency with which this occurs varies across crimes. For some offences such as cannabis possession, arrest and clear-up time series are near identical. For other crimes, such as violent crime or sexual offences, there is a greater divergence between the number of arrests and clear-ups. In studying the impacts of the LCWS on drug and non-drug crime, we exploit information on both arrests and clear-up series: this information is crucial to measure the police's ability to effectively reallocate resources towards non-drug crime as a result of the depenalization of cannabis possession.

Drug Crime Data: Offence Types

For the crime series related to drug offences, the administrative records contain information at two fine levels of detail. First, the records specify the number of criminal offences by *drug type*, e.g. cannabis, heroin, cocaine etc. We focus attention on cannabis and Class-A drug crime as these account for 95% of all drug crime, as shown below. Second, for each drug type, the data records the *specific offence* committed: possession, trafficking, intent to supply etc. To shed light on whether any observed change in the number of cannabis offences is driven predominantly by demand or supply side factors, we split cannabis offence types into two categories: we proxy changes in demand with the number of offences related to cannabis possession, and we proxy changes in supply with the number of offences related to trafficking, intent to supply etc.[14] Both levels of disaggregation by drug and offence type are also available for the other two crime series: on arrests and clear-ups. We exploit the full richness of this data when studying the

[13] Charging must occur within 24 hours of arrest, unless the crime is serious, in which case it may be extended by a police superintendent (36 hours) or a court (96 hours).

[14] These supply side offences include: possession with intent, possession on a ship, production, supply, unlawful export, unlawful import, carrying on a ship, inciting others to supply, manufacture, and money laundering. There are a very small number of other offences that cannot be classified as either demand or supply related.

impacts of the depenalization of cannabis on drug crime in Lambeth relative to the rest of London.

To make clear the levels and patterns of drug crime pre-policy, Table 1 provides descriptive evidence on drug crime in Lambeth and other London boroughs before the LCWS was introduced. We define the offence rate for cannabis related crime as the number of offences per 1000 of the adult population (aged 16 and above). Panel A highlights that Lambeth has historically higher rates of drug offences than other London boroughs: in the average month pre-policy since April 1998, there were .608 offences per 1000 of the adult population in Lambeth, while the rest of London average was .400. To put this into perspective, we note the pre-policy adult population in Lambeth was approximately $240,000$, so around 146 drug related offences were being recorded in Lambeth each month pre-policy. Out of 32 boroughs, Lambeth would be ranked 6th highest in terms of drug related offence rates pre-policy.

Panel B highlights the *composition* of drug offences by drug type. In line with some of the motivations for depenalization, the majority of drug offences relate to cannabis: 60% of all drug offences relate to cannabis in Lambeth; for other London boroughs this figure is closer to 74%. The incidence of offences related to Class-B drugs (excluding cannabis) and Class-C drugs is relatively minor, corresponding to less than 5% of all recorded drug offences. In consequence, Lambeth has relatively more drug offences related to Class-A drugs that other London boroughs.

Panel C shows how cannabis offences break down by *crime types*, that can be roughly classified as demand and supply side offences. In Lambeth 91% of cannabis offences are for the cannabis possession, with the remainder mostly related to intent to supply offences. This breakdown by cannabis offence type is not significantly different between Lambeth and other London boroughs. The levels of cannabis related drug crime documented in Table 1 certainly make it plausible that a cannabis depenalization policy could save considerable amounts of police time and resource, that could potentially be reallocated towards Class-A drug crime or non-drug crime.

Descriptive Time Series Evidence on Crime

To begin to establish whether and how the LCWS policy might have impacted drug and non-drug crime in London, we present three pieces of descriptive evidence. Figure 1A shows the monthly time series for the number of cannabis drug offences per 1000 of the adult population, for Lambeth and the average for all other London boroughs. The period during which the LCWS is in place is indicated by the dashed vertical lines. Four points are of note.

First, prior to the introduction of the LCWS, there is a *downward* trend in cannabis offence rates in Lambeth and London more generally. Second, there is a large *increase* in cannabis offence rates in Lambeth during the policy. Averaging within the pre and policy periods, cannabis offences in Lambeth rose by 61% in the policy period relative to pre-policy. For the rest of London, there was no significant change in cannabis offences between these time periods. Third, the dramatic upturn in offences occurs six months after the policy starts – precisely the time when the policy extension is announced – rather then immediately after the policy experiment is first introduced. This suggests the impact of the announcement of the policy's extension, rather than its mere introduction, is key for understanding changes in cannabis crime. At face value this casts further doubt on whether all the change in cannabis offences can be understood through merely a net-widening effect of changes in police reporting behavior, or changes in reporting behavior of local residents. Fourth, the rise in cannabis offences is quantitatively large and appears permanent. There is little evidence from Figure 1A that the time series for Lambeth begins to converge back to its pre-policy level or those of the other boroughs in the post-policy period. Indeed, post-policy, cannabis related offences continue to rise by a further 46% in Lambeth.

Figure 1B then focuses exclusively on offences of cannabis possession. This time series mimics the pattern for cannabis offences as a whole so that possession related offences, that constitute the bulk of cannabis related crime as shown in Table 1, do indeed drive the increase in cannabis offences in aggregate.

It seems unlikely that these policy impacts simply reflect changes in the likelihood that either police or local residents report the cannabis possession offenses that they witness. Before, during, and after the LCWS policy, the police were required to report all cannabis offenses they observed. Furthermore, there is no reason to expect local residents to became more likely to report cannabis offenses during the LCWS since they had reason to expect that the introduction of LCWS decreased the probability that such reports would result in sanctions for offenders. Thus, our evidence strongly suggests that, both in levels and relative to other boroughs, cannabis use in Lambeth increased substantially following the implementation of the LCWS. In the remainder of the paper, we focus on how changes in the behavior of Lambeth police may have induced this increase in cannabis consumption.

A key dimension along which changes in police behavior could then impact crime is through *non-drug* crime. The final piece of descriptive evidence we therefore present is the time series for all non-drug offences aggregated to a single series for Lambeth and the rest of London. As Figure 1C shows, prior to the LCWS's introduction, we observe upward trends in such crime rates in Lambeth and across London as a whole. However, a few months into the policy period, rates of criminal offence for non-drug crime begin declining in Lambeth and this downward trend continues in the long run. In contrast for the rest of London, non-drug offences remain relatively constant for the second half of the sample period. While far from definitive, this is the first piece of evidence that hints at the importance of changes in police behavior and potential reallocations of police resources from cannabis related crime towards non-drug crime, that might then induce changes in behavior among cannabis uses, to best explain the full set of descriptive evidence.

3.3.2 Empirical Method

To establish whether there is a causal impact of the LCWS policy on crime, we estimate the following panel data specification for borough b in month m in year y,

$$\begin{aligned} \ln C_{bmy} &= \beta_0 P_{my} + \beta_1 [L_b \times P_{my}] + \beta_2 PP_{my} + \beta_3 [L_b \times PP_{my}] \\ &\quad + \gamma X_{bmy} + \lambda_b + \lambda_m + u_{bmy}, \end{aligned} \tag{3.1}$$

where C_{bmy} is the offence rate, for a given crime type. The offence rate is defined as the number of criminal offences per thousand of the adult population (aged 16 and over). P_{my}, PP_{my} are dummies for the policy and post-policy periods respectively. L_b is a dummy for the borough of Lambeth. The parameters of interest are estimated from within a standard difference-in-difference research design: β_1 and β_3 capture differential changes in crime rates in Lambeth during and after the LCWS policy period, relative to other London boroughs. β_0 and β_2 capture London-wide trends in offence rates during the policy and post-policy periods.

All other London boroughs are included as part of the sample when estimating (3.1). Given the interlinkages across locations in cannabis markets, it is likely that after the LCWS is introduced, some individuals will be induced to start travelling to Lambeth to purchase cannabis there. This impact is spread over all 31 other London boroughs (and beyond), and so is unlikely to lead to a discernible upward bias in the coefficients of interest. However, to shed some light on this, in the Appendix we present a robustness check that estimates (3.1) when boroughs neighboring Lambeth are excluded from the sample (and find very similar results to the baseline estimates presented).

While administrative data on offences is available for each month from April 1998 onwards, the QLFS-LA data from which the denominator for offence rates is measured, is only available until Q4 2005. Hence our study period for analyzing the impacts of the LCWS runs from April 1998 until January 2006, covering three years pre-policy, the 13 months of the policy, and three and a half years post policy. In X_{bmy} we control for the following borough-specific time varying variables: the share of the adult population that is ethnic minority, that is aged 20-24, 25-34, 35-49, and above 50 (those aged 16-19 are the omitted category), and the male unemployment rate. The fixed effects capture remaining time invariant differences in offence rates across boroughs (λ_b) and monthly variation in crime (λ_m). We weight observations by borough population. Finally, defining time t as the number of months since January 1990: $t = [12 \times (y - 1990)] + m$, in our baseline specification we assume a Prais-Winsten borough specific AR(1) error structure, $u_{bmy} = u_{bt} = \rho_b u_{bt-1} + e_{bt}$,where e_{bt} is a classical error term. u_{bmy} is borough specific heteroskedastic, and contemporaneously correlated across boroughs.

3.4 Results

3.4.1 Cannabis Crime in Aggregate

Table 2 presents estimates of (3.1) where we focus on how the policy affects the rate of cannabis offences in aggregate. Column 1 estimates (3.1) conditioning only on borough and month fixed effects. The results replicate the descriptive evidence presented earlier: offence rates for cannabis related crime rise by 32.5% more in Lambeth relative to the rest of London between the pre-policy and policy period. The coefficient on the policy period dummy, $\widehat{\beta}_0$, is close to zero, suggesting there is no citywide time trend in cannabis crime rates during the policy period. Comparing the pre-policy and post-policy periods, cannabis offences are 61.5% higher in Lambeth vis-à-vis the rest of London. The post-policy period dummy, $\widehat{\beta}_2$, is positive and significant suggesting that the long run rises in Lambeth occur against a backdrop of significantly smaller, but rising, offence rates for the rest of London between August 2002 and January 2006.

Column 2 shows the results to be robust to including the full set of covariates in (3.1). These baseline results suggest the depenalization of cannabis in Lambeth led to a significant *increase* in cannabis offences both during the policy period, and well after the policy officially ended. The next two specifications additionally control for *within-borough* linear and quadratic time trends respectively. As expected, the policy effects are less precisely estimated and of slightly smaller magnitude. As Columns 3 and 4 show, once we also control for a within-borough time trends it is no longer possible to identify an effect of the policy during its period of operation. This is hardly surprising given the policy is only in operation for 13 months. However in both specifications the post-policy effect remains highly significant suggesting that post-policy offence rates for cannabis crime were at least 41.4% higher than the rest of London, all else equal.[15]

Following the time series evidence in Figure 1A, the specification in Column 5 checks for differential policy responses during the first six months of the policy, when the LCWS was announced to be a temporary policing experiment, and the last seven months, after it was announced to have been extended. In line with the evidence in Figure 1A, *all* of the significant within policy effect on cannabis offences occurs after the second policy announcement. We can only speculate on why this second announcement is the trigger for cannabis offences to rise. If, for example, it is interpreted as a signal of the policy's permanence, then as there are fixed costs to re-structuring police resource allocations, the police might have incentives to delay any large changes in their organization until the policy is presumed to be permanently in place.

Clearly understanding such dynamic and announcement effects of policy needs more research, but this finding does help however to immediately address two issues. First, it suggests the LCWS was not introduced in response to rising cannabis crime rates: as Figure 1A shows, cannabis offences were generally trending *downwards* in Lambeth in the years prior to the introduction of the LCWS. Second, this casts doubt on whether *all* the change in cannabis offences can be understood through changes in reporting behavior of local residents, or solely through a net-widening effect caused by changes in the way

[15] As a related robustness check, we estimated (3.1) restricting the sample to a 12 month window around the policy, that is from July 2000 until July 2003. Hence the policy and post-policy effects are not identified assuming any particular underlying long run time trends. The previous results are robust to using this narrower time frame. Indeed, this specification shows that over this shorter time frame when drug offences are still found to have risen in Lambeth, drug offences are declining elsewhere in London as suggested by Figure 1A.

the police recorded cannabis offences. If so, we would expect such effects to be picked up as soon as the LCWS comes into effect amid much media publicity, and we would expect such effects to be impacted by the policy officially ending.[16]

In the Appendix we detail robustness checks on the baseline specification estimated in Column 2 of Table 2. These address concerns related to: (i) the exclusion of neighboring boroughs as valid controls; (ii) accounting for common citywide shocks to cannabis crime through the inclusion of year fixed effects; (iii) controlling for a series of dummies that capture each period when specific Home Office reporting guidelines are in place; (iv) controlling for other police operations in London; (v) estimating standard errors allowing for spatially correlated error structures. In all cases we find qualitatively similar results to the baseline estimates presented: the magnitude of the long run policy impact on cannabis offences in aggregate varies between 41.4% and 68.2% across the robustness checks, and is significantly different from zero in each specification.

3.4.2 Cannabis Crime: Demand and Supply lmpacts

We now further unpack the mechanisms lying behind the main result from Table 2, that aggregate cannabis crime rises in Lambeth relative to the rest of London, in both the short and long term, after the depenalization of cannabis possession in Lambeth. To do so we exploit the fact that the administrative crime records break down cannabis crime into specific *types* of crime. We do so along two natural margins: (i) offences related to cannabis possession, that might be more attributable to changes in the *demand* for cannabis; (ii) offences related to cannabis trafficking and supply, that might be more attributable to changes in cannabis *supply*.[17]

For both demand and supply side cannabis crimes, we also explore measures of police behavior such as (the log of) arrest rates and clear-up rates. As individuals are not necessarily immediately arrested for cannabis related offences they commit, we define the arrest rate as the number of arrests in the borough in period t divided by the number of offences committed between month t and the previous quarter within the borough. The

[16]We also estimated a specification breaking down the post-policy response for each year. This confirmed the post-policy effects on cannabis crime to be long-lasting: we cannot reject the null that the effect in Lambeth is the same in the first and fourth year post-policy. These helps address concerns that cannabis crime rates in Lambeth were naturally diverging away from the rest of London.

[17]Of course, this classification of offences into demand and supply related is only approximate. For example, it might be substantially more difficult to prove an offence of intent to supply, so that in practice the police use their discretion so some drug suppliers are charged with a lesser offence of possession.

clear-up rate is analogously defined: the number of clear-ups in the borough in period t divided by the number of offences committed between month t and the previous quarter within the borough.[18]

Table 3 presents the results. In each column, specifications analogous to (3.1) are estimated, where the crime series now refer to sub-categories of cannabis crime. Columns 1 to 4 have as dependent variables (C_{bmy}) crime series related to cannabis possession, proxying the demand for cannabis; Columns 5 to 8 explore crime series related to cannabis supply (the sample size drops slightly in these specifications because crimes related to cannabis supply do not necessarily occur in every borough-month). Furthermore, given the earlier finding in Column 5 of Table 2, we divide the policy period into two halves to more precisely understand the effects of the LCWS on the market for cannabis when it is announced as a temporary policy experiment vis-à-vis a more permanent change in policing strategy.

Cannabis Demand

On the demand for cannabis, Column 1 shows offence rates for cannabis possession only rise after the policy is announced to have been extended: this increase of 67.5% in offence rates for cannabis possession in the second half of the policy period closely matches the descriptive evidence in Figure 1B. We find no evidence that rates of cannabis possession in other London boroughs change significantly during the policy period. In the longer term, post-policy cannabis possession offence rates remain 68.6% higher in Lambeth relative to the rest of London.

To focus in on changes in police behavior that the LCWS induced, we next estimate (3.1) but where the dependent variable is the arrest rate for cannabis possession. Column 2 shows that relative to the pre-policy period, arrest rates for cannabis possession in Lambeth significantly *drop* by 43.6% in the first half of the policy period, and by 94.6% in the second half of the policy period. However, post-policy, arrest rates return back to their pre-policy levels ($\widehat{\beta}_3 = 0$).

The next specification considers another dimension of police behavior: clear-up rates for cannabis possession offences. Column 3 shows a significant fall in clear-up rates in Lambeth for cannabis possession as soon as the LCWS policy is introduced.[19] In the

[18] Ideally, the clear-up rate in time period t would be defined as the number of clear-ups in time t divided by the stock of *unsolved* offences at the time, but such data is unavailable.

[19] The fact that the impacts on arrest and clear-up rates for cannabis possession are qualitatively similar is

longer term, police effectiveness in Lambeth for crimes related to cannabis possession appears weakened relative to the pre-policy period: clear-up rates remain significantly lower. This occurs at a time when there are no London wide trends in clear-up rates ($\widehat{\beta}_2$ is not significantly different from zero in Column 3). At the same time, as previously noted in Column 1, in the longer term, post-policy offence rates remain 68.6% higher in Lambeth than in the pre-policy period suggesting that the demand for cannabis remains permanently higher long after the LCWS policy officially ends.

Perhaps the cleanest way to measure police effectiveness is to consider the (log of) clear-ups per arrest in any given period t month as the dependent variable in (3.1): this captures the rate of conversion of arrests into clear-ups as arrestees are charged for cannabis possession. The result in Column 4 shows a significant fall in clear-ups per arrest in Lambeth during the policy period, and more notably, a significant fall of 57.6% post-policy. This occurs against a backdrop of significantly *rising* clear-ups per arrest for cannabis possession in the rest of London in the post-policy period.

In summary, the measures of police behavior used in Columns 2 to 4 indicate that once depenalization is in place, the police immediately devote less effort towards targeting cannabis users. On the one hand, this is reassuring because it is precisely what the depenalization policy prescribes: cannabis possession no longer leads to arrests (although offences should be recorded in the same way as pre-policy) and so we expect to observe immediate falls in arrest and clear-up rates as soon as the policy is introduced. However, such a weakened deterrence effect of depenalization might in turn impact the behavior of cannabis users, ultimately feeding through to drive the significant rise in cannabis possession offences six months into the policy, as shown in Column 1.[20]

In the longer term, there remains evidence that police effectiveness against cannabis possession offences is lower than in the pre-policy period, in line with the description of the policy evolution given in Section 2.3: in the longer term, policing strategies in Lambeth did not revert back to identically what was in place pre-policy. This opens up the possibility that in Lambeth police resources are permanently reallocated towards Class-A drug crime and non-drug crime, as we explore in detail in Section 5.

not surprising: as described in Section 3.1.1, the arrest and clear-up series only diverge if individual are arrested but not charged for cannabis possession. This occurs far more rarely for cannabis possession offences than for some other non-drug crime we later analyze.

[20] Durlauf and Nagin [2010] provide a comprehensive overview of the literature on the evidence in favor of deterrence effects from a range of crime policies.

Cannabis Supply

The remaining Columns of Table 3 repeat the analysis for crime series related to the *supply* of cannabis. We find: (i) evidence the LCWS significantly increased offences related to cannabis supply during its official period of operation: by the second half of the policy period offence rates for cannabis supply were 50.5% higher in Lambeth relative to the pre-policy period, an impact significant at the 1% level; (ii) in the post-policy period, cannabis supply offences rose by 67.6% more in Lambeth relative to the rest of London, and there is no long term citywide time trend in such crimes. On police effectiveness against crime related to supplying cannabis, Columns 6 to 8 document no changes during the policy period in terms of arrests, and a fall in clear-up rates that is significant at the 10% level. For our preferred measure of police effectiveness, clear-ups per arrest do not change significantly during the policy period, and in the longer, rise slightly in Lambeth relative to the rest of London (an effect significant at the 10% level), at a time when citywide police effectiveness against cannabis supply related crime appears to be either falling (Columns 6 and 7) or stable (Column 8).[21]

Taken together the results suggest that any change in the underlying size of the market for cannabis in Lambeth as a result of the policy was driven by demand and supply side factors. However, while police effectiveness against demand side offences remaining permanently lower post-policy, police effectiveness against crimes related to cannabis supply marginally improved in Lambeth in the longer term even after the LCWS was officially ended.[22] This hints at the possibility that the police were able to reallocate their effort away from incidents related to cannabis possession, towards other drug crime and non-drug crime. We now explore this in more detail.

3.5 The Reallocation of Police Effort

The results in Tables 2 and 3 document changes in levels and composition of cannabis related crime following the depenalization of cannabis possession in Lambeth. These results suggest the primary mechanisms at play driving the policy impacts are changes

[21] We note all the results presented in Columns 2 to 4 and 6 to 8 are largely robust to defining arrest and clear-up rates as being per 1000 of the adult population, rather than per the number of offences in the previous quarter. The results are not therefore driven by the increase in offences previously noted.

[22] For brevity, we have not shown the dynamic policy response along these margins when we split the post-policy period year by year. Doing so we find the significant increase in cannabis possession offences remains in each of the four years post policy, as does the increase in cannabis supply related offences.

in behavior of the police and cannabis users. Focusing in on these channels, we now investigate the short and long term impacts the depenalization policy had on the incidence of, and police effectiveness against, crime related to Class-A drugs and non-drug crime in seven categories: violence against the person, sexual offences, robbery, burglary, theft and handling, fraud and forgery, and criminal damage.

3.5.1 Crime Related to Class-A Drugs

As the administrative crime data records drug crime by *drug-type*, we first examine whether the LCWS policy allowed police in Lambeth to reallocate their effort towards Class-A drugs, that constitute the bulk on non-cannabis drug crime (Table 1, Panel B). As described in Section 2, that the policy might enable the re-targeting of police resources towards crime related to Class-A drugs was one motivation behind the introduction of the LCWS, as if often the case for depenalization policies in other contexts.

We estimate specifications analogous to (3.1) breaking the results down along two margins: (i) crime series related to the possession of Class-A drugs, proxying the demand for such illicit substances; (ii) crime series related to the supply of Class-A drugs. As for cannabis crime, we do so for crime series on offence rates, and measures of police effectiveness such as arrest and clear-up rates. Table 4 shows the results. To facilitate comparison with the previously documented impacts on cannabis crime, we again divide the policy period into two halves.

On the demand side, Table 4 shows: (i) during the policy period there is an impact of depenalizing cannabis possession on the demand for Class-A drugs as proxied by possession offences for such substances (Column 1); (ii) in the longer term, offences related to the possession of Class-A drugs significantly rise by 12.0% in Lambeth relative to the rest of London – this increase occurs against the backdrop of no change in citywide offence rates for Class-A drug possession; (iii) there is little robust evidence of a change in police effectiveness against crime related to the possession of Class-A drugs, as measured by arrest rates, clear-up rates, and clear-ups per arrest (Columns 2 to 4). Hence, the evidence does *not* suggest the Lambeth police turned a blind-eye towards Class-A drug possession in Lambeth during or after the LCWS policing experiment.

The remaining Columns of Table 4 show crimes series related to supply of Class-A drugs. We find: (i) no evidence of the LCWS policy impacting offence rates related to the supply of Class-A drugs during the policy period, but a significant fall in such offences post-policy; (ii) somewhat mixed evidence on any impact on the police effectiveness against crimes related to the supply of Class-A drugs: we observe no significant changes in arrest or clear-up rates (Columns 6 and 7), but there is a significant increase of 12.3% in clear-ups per arrest (Column 8).

Taken together, the results shows that in the long term, the patterns of demand related Class-A drug crime in Lambeth along all three margins of offences, arrests and clear-ups, do not differ much from London-wide trends more generally. This is in sharp contrast to the previously documented effects on cannabis demand offences, arrests and clear-ups shown in Table 3. However, the evidence in the second half of Table 4 hints at the possibility the police might have reallocated effort towards supply related Class-A drug crime: offence rates for crimes related to the supply of Class-A drugs significantly fall in the longer term, and police effectiveness against such crimes, at least as measured by clear-ups per arrest, significantly rise.

3.5.2 Non-Drug Crime

Motivated by the earlier descriptive evidence from Figure 1C on trends in non-drug crime in Lambeth relative to other London boroughs, we now broaden the search for evidence of the reallocation of police effort, by examining seven types of *non-drug* crime. Table 5 reports the results. In Column 1 we first estimate (3.1) where the dependent variable is the (log of) offence rate for total non-drug crime. During the policy period, offence rates for total non-drug crime were not significantly different in Lambeth than other London boroughs. Remarkably, in the post-policy period, the offence rate for total non-drug crime in Lambeth significantly fell by 9.4% more than the London-wide average. Quantitatively, this translates into a large reduction in total crime in Lambeth: pre-policy, 97% of all offences in Lambeth are non-drug related. This long term reduction in Lambeth occurred in a period when city-wide offence rates for non-drug crimes are flat, as Figure 1C suggested.

The remaining Columns of Table 5 show significant falls post-policy in recorded offence rates for five out of seven crime types. These categories: robbery, burglary, theft and handling, fraud and forgery and criminal damage, account for 81% of all criminal offences pre-policy. The point estimates on the other three categories, violence, sexual offences and robbery, are all negative but not significantly different from zero. To aid exposition, Figure 2A shows the eight coefficients of interest ($\widehat{\beta}_2$) from Table 5, along with their associated 95% confidence intervals.

To pin down whether this long run decline in non-drug crime is due to a reallocation of police effort, Table A3 estimates the short and long run policy effects on our measures of police effectiveness: arrest rates (Panel A), clear-up rates (Panel B), and clear-ups per arrest (Panel C). Given the large number of coefficients to read in Table A3, Figures 2B to 2D show the coefficients of interest of the long-run policy impacts from each specification, along with their associated 95% confidence interval.

In terms of police effectiveness against non-drug crime, we find that: (i) arrest rates for total non-drug crime *rose* significantly (Table A3, Panel A, Column 1): the long run difference-in-difference estimate is 28.4% for Lambeth relative to the rest of London; (ii) considering specific crime types, the remaining Columns in Panel A and Figure 2B highlight how in the long run there are significant *increases* in arrest rates for nearly all crime types; (ii) Panel B of Table A3 and Figure 2C show these higher arrest rates actually feed into significantly higher clear-up rates, again for nearly all crime types;[23] (iii) Panel C of Table A3 and Figure 2D show that clear-ups per arrest do not change for most crime types. Hence the likelihood an arrestee is charged with the offence is not driving the earlier result; rather any change in police effort leads to more arrests and clear-ups *per se*, for these six broad crime types and for non-drug crime overall.

Taken together the evidence suggests a significant re-allocation of policing intensity after the introduction of the LCWS, away from cannabis crimes and towards other non-drug crimes (Table 5), but not especially towards Class-A drug crime (Table 4). This re-allocation appears to persist long after the LCWS officially ends, and is reflected in marked increases in arrest and clear-up rates for a broad range of crime types (Table A3, Panels A and B). These changes in police effectiveness of course feedback into lowering offence rates (Table 5).[24]

[23]The one exception relates to crimes of theft and handling, where we see no long run differential change between Lambeth and the rest of London in arrest or clear-up rates. As with some of the earlier evidence and existing literature, this might suggest such crimes are especially colinear with the market for cannabis, that is of course expanding in the long run in Lambeth. Unlike for offences related to cannabis possession, there is generally a divergence between arrest and clear-up numbers for these non-drug offences.

[24]These results are largely robust to defining arrest and clear-up rates as being per 1000 of the adult population, rather than per offences in the previous quarter. Hence these patterns in arrest and clear-up rates likely reflect real changes in police behavior rather than being driven solely by declines in the number of offences in each crime type.

3.5.3 Police Resources

Given the central role the re-allocation of policing effort plays in explaining changing patterns of crime and police effectiveness as a result of the depenalization policy, it is important to understand whether the results could in part be confounded by a change in *total* police resources, rather than a mere re-allocation of existing resources. While detailed borough-month level information on police manpower or task allocations does not exist for our study period, there is evidence from MPA reports that police officer numbers in Lambeth rose in the post-policy period.[25] These suggest that in the summer of 2001 the Lambeth police were running at 11% below their budgeted workforce target, equivalent to 102 officers below strength. By January 2002 the situation had improved with an additional 43 officers in Lambeth, reducing the deficit to 6.3%.

To investigate whether this change in Lambeth can explain the differential patterns of crime documented in Table 5, we have collated the available data on annual police numbers for all 32 London boroughs from 1997 to 2010. This shows that police numbers certainly rose in Lambeth during and after the policy: between 2001 and 2006, police numbers increased by 20.5% in Lambeth. However, this pattern is by no means exceptional to Lambeth. Over the same period, the police numbers for London as a whole rose by 22.7%, slightly more than in Lambeth. This suggests changing police strength in Lambeth vis-à-vis other London boroughs is unlikely to explain the large reductions in non-drug crime documented.[26]

A second way to understand whether changing police numbers might plausibly explain the documented impact on non-drugs crime is to use estimates from the literature on the elasticity of crime with respect to police strength. In this setting, the estimates provided by Draca *et al.* [2011] are perhaps most informative. They use the exogenous shift in police deployment following the July 2005 terror attacks in London to estimate an elasticity of crime with respect to police numbers to be around -0.3. For the LCWS, over the post-policy period from January 2002 to March 2006, police numbers in Lambeth increased by 13.2%. Ignoring the change in other London boroughs and so assuming the 13.2% increase in Lambeth represents the difference-in-difference with other boroughs, we can then combine the elasticity estimate from Draca *et al.* [2011] and our regression

[25] Source: http://www.mpa.gov.uk/committees/mpa/2002/020926/17/

[26] We have probed this time series on police numbers by borough-year to understand what drives changes in police strength. This suggests that police numbers track the borough population with some lag.

coefficient, this should have led to a 4% drop in non-drugs crime. Hence, even under this most conservative approach where we ignore changing police numbers in other boroughs, the drop in non-drugs crime that can be explained through this channel is just less than half the actual long run fall in non-drug crime we find of 8.8%.

In short, the evidence suggests the documented reduction in non-drug crime and increased police effectiveness against such crimes was primarily due to a differential reallocation of police resource in Lambeth relative to the rest of London, rather than increased numbers of police officers *per se*. As such, the policy likely had small monetary costs of implementation. The next section moves onto establishing the monetized welfare impacts of the policy on Lambeth residents.

3.6 House Prices

Understanding the *welfare* consequences of any given drugs policy is important given the large number of illicit drug users around the world. This is especially so for policies related to the market for cannabis, the most frequently used illicit drug in most countries. Miron [2010] estimates, in the US context, the budgetary consequences of liberalizing drug policy. We add to this nascent literature by evaluating the welfare effects of the localized LCWS depenalization policy.

From the documented impacts on crime, the welfare effects of the policy are ambiguous: although the policy caused total crime to fall, it also caused a dramatic change in the composition of crime. Depenalization led to an increase in cannabis offences, but on the other hand, many other types of crime were reduced in the longer term. To estimate the overall impact of the policy through these changing crime patterns, as well through other non-crime channels, we estimate the impact of the depenalization of cannabis possession on house prices in Lambeth relative to other London boroughs. This approach uses the intuition that the total social cost of depenalization (not just those arising from crime) should be reflected in house prices [Rosen 1974, Thaler 1978].

To do so, we exploit information at the zip code level on house prices from the *UK Land Registry* to estimate a specification analogous to (3.1). The unit of observation is zip code sector s in quarter q in year y, where zip code sectors are within borough.[27]

[27] A London zip code (e.g. WC1E 6BT) is generally 10-12 neighboring addresses (that would include flats and maisonettes, as well as separate houses). Our house price data was obtained from the UK Land Registry at a lightly more aggregated level, that of a zip code sector (e.g. WC1E). In London there are an average of 215 zip codes per zip code sector (so 2000-2500 addresses in each zip code sector). There

This allows us to later explore whether and how the effects of depenalization affect house prices *within* Lambeth. To begin with we estimate a panel data specification of the form,

$$\begin{aligned} \ln h_{sqy} &= \beta_0 P_{qy} + \beta_1 [L_b \times P_{qy}] + \beta_2 PP_{qy} + \beta_3 [L_b \times PP_{qy}] \\ &\quad + \gamma X_{bqy} + \lambda_s + \lambda_q + u_{sqy}, \end{aligned} \tag{3.2}$$

where h_{sqy} is the mean house price sale for terraced houses in zip code sector s in quarter q in year y, deflated to 1995 Q1 prices;[28] P_{qy}, PP_{qy} are dummies for the policy and post-policy periods respectively; L_b is a dummy for whether the zip code sector is in Lambeth. To reflect the lag between house buying decisions and recorded house sales, all time-varying covariates are lagged one quarter. In X_{bqy} we continue to control for socio-demographic controls, as in (3.1). We also allow for borough specific time trends ($\lambda_b \times qy$) to capture common house price movements, and control for fixed effects for zip code and quarter. The sample runs from January 1995 until December 2005, standard errors are clustered at the zip code-sector level, and observations are weighted by the numbers of terraced house sales in the zip code-sector during the quarter.

House price information is available for terraced houses, detached, semi-detached, and flats. When estimating (3.2) our baseline estimates focus on terraced housing to strike a balance between using a housing type that has both frequent sales, and high values per sale. When documenting the total impact of the policy on house prices in Section 6.2, we do so by aggregating the policy impacts across all four housing types.

3.6.1 Results

Table 6 reports the results. Column 1 presents the baseline finding: in the long run after the LCWS is introduced, house prices fall by 5.0% more in Lambeth relative to the London wide average, an effect significant at the 1% level. Column 2 shows the impact to be even more negative after controlling for borough specific linear time trends.

are on average 20 zip code sectors per borough. In Lambeth (that is of total are 10.36 square miles (26.82 km^2)), there are 31 zip code sectors, so that each covers on average .33 square miles (.87 km^2).

[28]The house price data cover 25 of the 32 boroughs used for the crime analysis. The boroughs not covered are Barking and Dagenham, Bexley, Harrow, Havering, Hillingdon, Kingston-upon-Thames and Sutton. There are 509 distinct zip codes in the final sample, with an average of 25.3 zip codes per borough. House prices are deflated to the first quarter of 1995 prices, using the Land Registry house price index for Greater London, which is based on repeat sales (see http://www1.landregistry.gov.uk/houseprices/housepriceindex/.) We drop zip code sectors that have the lowest 10% of house sales, as these are unlikely to correspond to residential neighborhoods. The reported results are robust to dropping zip codes that straddle borough boundaries.

To reiterate, these negative effects on house prices in the long run occur despite the overall *falls in total crime* experienced in Lambeth post-policy: as Table 5 showed, total non-drug crime fell by 9.4%. At the same time, the results from Table 2 showed the incidence of cannabis related crime rose by at least 40% in the longer term. To reconcile these policy impacts on crime and house prices, Lambeth residents might either place disproportionate weight on cannabis related crime relative to all other crimes, or there might exist other social costs beyond crime associated with a rapidly expanding market for cannabis.[29]

As house price data is available by zip-code, the remaining specifications in Table 6 examine whether there are heterogeneous effects of depenalization on house prices *within* Lambeth and other boroughs. The heterogeneity we focus on relates to the location of drug crime within each borough, and leads us to designate each zip code sector as a drug crime 'hotspot' or not. The Appendix describes in detail how we use disaggregated drug crime data to determine whether a zip code sector is a hotspot. We then explore whether house prices vary differentially within borough between hotspots and non-hotspots, using a triple-differenced estimation strategy, across boroughs, time, and hotspot/non-hotspot areas.

The disaggregated data from which hotspots are defined are 'ward' level crime statistics published by the MPS. Wards are small administrative districts nested within boroughs. There are, for example, 21 wards in Lambeth, that closely matches the London borough average. However, such ward level crime data only exists for each month from April 2001 onwards. Hence for our baseline results, we classify zip code sectors into hotspots based on crime rates measured *ex post* in 2008/9, long after the LCWS is initially implemented. Given obvious concerns over using such *ex post* data to define hotspots, we also use the available crime ward data for the few months pre-policy to re-estimate our main specification classifying zip code sectors into hotspots based on *ex ante* crime rates. To provide evidence of the geographic stability of hotspot locations in Lambeth over time, Figure A1 shows the classification of each Lambeth zip code sector into hotspots based on both definitions: reassuringly there is considerable stability in

[29] Other studies have found a negative association between certain crime types and house prices: Gibbons [2004] documents how a one standard deviation increase in property crime is associated with a 10% reduction in house prices in the UK; Linden and Rockoff [2008] present evidence from the US that the revelation of information of a sex offender being resident next door leads to a 12% reduction in house prices. Our results likely differ because the policy we evaluate impacts both the level and composition of crime.

these classifications over time. The Appendix presents further robustness checks based on alternative hotspot definitions.

Column 3 of Table 6 then presents estimates of this triple-differenced specification where we allow the policy impacts to vary across hotspots within each borough. We find all of the previously documented long run negative effect of depenalization on house prices within Lambeth occurs in drug crime hotspots. There is *no* significant effect of depenalization on house prices on non-hotspot zip codes in Lambeth. As a result, the magnitude of the house price fall in Lambeth hotspots, −13.4%, is significantly larger than in the earlier all-Lambeth estimates.[30] In the post-policy period, hotspot areas in other boroughs appear to have positive and significant house price rises, consistent with there being convergence in house prices across neighborhoods.

Column 4 then shows the main results to be very stable using *ex ante* ward level crime data to classify zip code sectors as hotspots: the relative house price decline in Lambeth hotspots is very similar at −13.5%, and we still observe rising prices in hot spots in other London boroughs in the post policy period (6.6%). The similarity of findings using *ex ante* and *ex post* hotspots is unsurprising given the geographic stability over time in where drug crime is concentrated in Lambeth, as Figure A1 shows.

The remaining Columns demonstrate the robustness of the results to alternative methods by which to calculate standard errors. In Column 5 we cluster at a higher level of aggregation: given the baseline estimates cluster by zip code sector, the natural next level of aggregation is to cluster by borough. Comparing this specification in Column 5 to the baseline definition using *ex post* hotspots in Column 3, we see the standard errors to be considerably smaller when clustering by borough, supporting the view that the baseline approach is conservative.[31]

The Appendix presents robustness checks that probe these results in two directions: (i) the policy impacts on other housing types; (ii) using alternative definitions of crime hotspots. In each case we find results very much in line with these baseline findings.

[30] May *et al.* [2007b] provide detailed descriptive evidence on drug dealing in Brixton: a hot spot area in our definition covering more than one zip code, and the most important commercial centre in Lambeth. They describe the geography of drugs crime in Brixton, how it affects other crimes.

[31] Cameron *et al.* [2008] note that cluster-robust standard errors may be downwards biased when the number of clusters is small, leading to an over-rejection of the null of no effect. The authors propose various asymptotic refinements using bootstrap techniques, finding that the wild cluster bootstrap-t technique performs particularly well in their Monte Carlo simulations. We have implemented this method on our preferred specification in Column 3, with 1000 bootstrap iterations and using rademacher weights for the procedure. The resulting estimated standard errors are very similar to those reported and all the reported coefficients remain of the same significance.

For all variant specifications we see that post-policy, house prices are significantly lower in Lambeth hotspots than other boroughs, where the magnitude of the impact varies between 7.7% and 13.9%.

The results from Table 6 suggest that for local residents, the total welfare impacts of depenalizing the possession of small quantities of cannabis likely went far beyond the impacts on crime. For example, there might have been other deleterious impacts on behaviors associated with the market for illicit drugs, such as alcohol use and other forms of visible anti-social behavior. These are important channels through which the effects of depenalization might operate in the long run [Miron and Zweibel 1995], and that we are investigating in ongoing research.[32] Such wider changes appear to reduce the willingness to pay to reside in these neighborhoods and increase within borough inequality in house prices between high and low drug crime zip codes.

The magnitude of these house price impacts can be compared relative to other studies, albeit in some cases, we have to extrapolate out of sample to have changes in local characteristics that would correspond to an equivalent reduction in house prices of −13.4%. Notwithstanding this caveat, comparing our estimates to those linking house prices with *school quality*, implies that an equivalent reduction in house prices could be generated by: (i) a 19% reduction in pupils achieving UK government targets at the end of primary school [Gibbons and Machin 2003]; (ii) a four standard deviation decrease in value-added scores of primary schools in the UK [Gibbons *et al.* 2013]; (iii) test scores that are 32% below the mean, based on US data estimates [Black 1999]. Comparing our estimates to those linking house prices with crime, we find that an equivalent reduction in house prices could be generated by either a greater than one standard deviation increase in property crime, based on UK data [Gibbons 2004]; for the US, Linden and Rockoff [2008] show the revelation of a sex offender residing next door reduces house prices by 12%. Finally, we can also benchmark our findings against the documented impacts of environmental quality on house prices: for the US, Davis [2004] shows a severe increase

[32] For example, Kelly and Rasul [2013] evaluate the impact of the LCWS on hospital admissions related to illicit drug use. They exploit administrative records on individual hospital admissions classified by ICD-10 diagnosis codes. They find the depenalization of cannabis had significant longer term impacts on hospital admissions related to the use of hard drugs, raising hospital admission rates for men. Among Lambeth residents, the impacts are concentrated among men in younger age cohorts. Model [1993] explores the effect decriminalizing cannabis in 12 US states between 1973 and 1978 had on hospital emergency room drug episodes. He finds evidence that decriminalization was accompanied by a significant reduction in episodes involving drugs other than marijuana and an increase in marijuana episodes suggesting consumers substitute towards the less severely penalized drug. There is mixed evidence on whether alcohol and cannabis are substitutes for young individuals: DiNardo and Lemieux [2001] and Conlin *et al.* [2005] find they are substitutes; Pacula [1998] finds them to be complements.

in the risk of pediatric leukemia is associated with a 14% reduction in house prices.

3.6.2 Interpretation

The documented impacts of the LCWS on house prices can reflect changing amenity values of residing in Lambeth, changes in the quality of the existing housing stock, or changes in value of newly constructed homes in Lambeth. To tease apart these explanations would require far more detailed information on housing characteristics that is not easily available. Although we use data on house prices and sales from the main UK data source, the *Land Registry*, even their most disaggregated administrative records on individual sales provide little information on house characteristics: they relate only to whether the house is a new build and information on its freehold/leasehold status.[33]

We now focus attention on estimating the total implied loss in property values in Lambeth as a result of the policy, proceeding as follows. First, we run our preferred house price specification (3.2) for each of the four housing categories in the *Land Registry* data: terraced houses, flats, semi-detached and detached houses. Table 7 shows the estimated β-coefficients from each specification, where each Column refers to a different housing type. The relevant parameter of interest is the long run post-policy impact on house prices: $\widehat{\beta}_3$. This is negative and significant for three of the four house types: semi-detached, terraced and flats.

These parameter values are then multiplied by the base level of house prices in Lambeth pre-policy, for each property type, and then multiplied by the number of property types actually sold over the post-policy sample period. Rows A and B show the mean and median pre-policy sales for each housing type. There is little divergence between the two and so for the remainder of the analysis we focus attention on using the mean price in row A. Row C shows the number of house price sales in the post-policy period until December 2005 by housing type.

Combining this information then provides an implied total loss in value for a given property type. We first provide a *lower* bound estimate on this implied loss by assuming that *only* those houses that are actually sold experience any loss in value. Doing so, row D shows for each housing type, the implied loss in value over the post-policy period.

[33] However, we note that there is very limited scope for new builds in Lambeth (as for all inner London boroughs). With more that 280,000 residents, Lambeth is one of the most densely populated boroughs in the country, with more than 100 residents per hectare. As such, our prior is that the documented house price effects reflect changing amenity values and changing quality of the existing housing stock.

Summing across the four housing types in Columns 1 to 4, the final Column on the right hand side of Table 7 gives the total implied loss: this amounts to £233mn.[34]

This corresponds to a *lower bound* on welfare losses because it ignores any reductions in property price values that are experienced by those residents that chose not to sell. To better capture any such impacts, we conduct another thought experiment assuming *all* properties in Lambeth of a given type experience the same implied loss in value, irrespective of whether or not they are actually sold post-policy. This approach requires additional information on the total housing stock. This is shown in the far right column in row E: there are $119,000$ properties in Lambeth. However, as the information to break this down by property type does not exist, we assume the share of all properties sold of a given type for the post-policy period (based on row C) is the same as the share of all households that exist of a given type in Lambeth. This share is then given in row F.

Using this information, we are then able to derive something more akin to an upper bound estimate of the implied total loss in property value: row G gives the implied loss for the entire post-policy period: £1.1bn, almost five times the lower bound estimate derived in row D. In short, whichever way the implied loss in Lambeth property values is calculated, it dwarfs any direct costs of the LCWS, a policing change that largely amounted to a change in how existing police resources were allocated, rather than any change in the level of resources *per se*.

3.7 Citywide Depenalization

The reduced form analysis emphasized how a *localized* depenalization of cannabis possession impacts the levels and composition of crime. We now build on the key lessons from this policing experiment to shed light on what would be the impacts on crime if the same policy were to be applied *citywide*, as is relevant for many current policy debates around the world. To do so, we develop a structural model of the market demand for cannabis, accounting for the endogenous choices of the police and cannabis users. We first calibrate the model to the localized policing experiment in Lambeth, and then consider

[34]This aggregate loss in property value is almost unchanged if we ignore any impacts on detached houses, as shown in Column 1. The likely reason for a non-significant impact for such house types is because there are only 52 recorded sales of such homes in Lambeth post-policy.

a counterfactual policy experiment of citywide depenalization.[35,36]

3.7.1 A Model of Cannabis Use, Non-Drug Crime and Policing

Cannabis Users

There are two locations, indexed by b: the borough of Lambeth ($b = 1$) and the rest of London ($b = 0$), with a population N_{bt} in location b at time t. Individuals make two choices: whether to buy (and thus consume) cannabis, and if they buy, which location b to buy cannabis from. Individuals are heterogenous in two dimensions: the propensity to consume cannabis, and the cost of moving from one location to another. We assume individuals can only be caught for cannabis crime in the location of purchase.

The utility of consuming cannabis comprises three components: an individual specific utility δ, the moving cost incurred *if* the individual travels to the other location to purchase cannabis λ^D, and a cost of being apprehended with cannabis by the police if purchasing in location b, denoted $\alpha_{bt}\pi^D_{bt}$. π^D_{bt} is the (endogenous) likelihood that an individual is caught in possession of cannabis, and we refer to this as the 'detection rate'. α_{bt} is the location specific cost when apprehended. This is indexed by time t as the LCWS experiment in Lambeth can be seen as partly operating though a reduction in α_{1t} relative to α_{0t}, as those caught in possession of cannabis are no longer arrested unless there are additional aggravating factors, as described in Section 2.

Assume individual i resides in location b. Her utility from consuming in her own borough is denoted u^D_{ibt}, her utility from consuming in the other borough is $u^D_{i,-bt}$, and her utility from not consuming is u^{ND}_{it}, and we normalize this last term to zero. Hence the utility of consuming cannabis is given by u^D_{it},

$$u^D_{it} = \max[u^D_{ibt}, u^D_{i,-bt}], \tag{3.3}$$

[35]The structural model does not emphasize how the behavior of cannabis suppliers might alter with depenalization, and as such, the model is not used to make price predictions on cannabis across locations. We make this modelling choice because: (i) information about the criminal supply side is lacking; (ii) information on drug prices at the borough-month level is also unavailable, and it is unclear how reliable such price information would be given that it is often based on selective samples of drug busts, and there is considerable dispersion in price-quality ratios for illicit drugs [Galenianos *et al.* 2012].

[36]Our approach is related to Imrohoroglu *et al.* [2004], Conley and Wang [2006] and Fu and Wolpin [2013], who develop equilibrium models of crime and policing. Our approach differs as we allow for endogenous mobility across location and specialization in different types of crime. Moreover, identification of the parameters of the model is achieved using quasi-experimental variation through the introduction of the LCWS policy.

$$
\begin{aligned}
u^D_{ibt} &= \delta - \overline{\delta}_b - \alpha_{bt}\pi^D_{bt} && \text{if consuming in } b, \\
u^D_{i,-bt} &= \delta - \overline{\delta}_b - \lambda^D - \alpha_{-bt}\pi^D_{-bt} && \text{if consuming in } -b.
\end{aligned} \tag{3.4}
$$

An individual purchases cannabis from some location if $u^D_{it} > 0$. We assume δ is uniformly distributed over $[0, 1]$. The parameter $\overline{\delta}_b$ determines the share of the population that consumes cannabis absent policing (if $\pi^D_{bt} = 0$). We allow this parameter to vary across locations, to capture different preferences between Lambeth residents and the rest of London. We assume the moving cost λ^D, is uniformly distributed over $[0, \overline{\lambda}]$ and that δ and λ^D are uncorrelated.[37]

D_{bt} denotes the market demand for cannabis in location b and period t, namely the number of cannabis users in b. This is the sum of the number of users that reside in location b and prefer to consume there, and users from location $-b$, that prefer to move and buy cannabis from b:

$$
D_{bt}(\pi^D_{bt}, \pi^D_{-bt}) = N_{bt}\Pr(u^D_{ibt} > u^D_{i,-bt}, u^D_{ibt} > 0) + N_{-bt}\Pr(u^D_{i,-bt} > u^D_{i,-bt}, u^D_{i,-bt} > 0). \tag{3.5}
$$

The model makes precise the interlinkages in cannabis markets across locations. The equilibrium market size for cannabis in each borough is a function of: (i) the detection rates in both boroughs $(\pi^D_{bt}, \pi^D_{-bt})$ that are endogenously determined as described below; (ii) the punishment for cannabis related criminal activities in both locations $(\alpha_{bt}, \alpha_{-bt})$; (iii) the populations of both boroughs (N_{bt}, N_{-bt}). As cannabis markets across locations are interlinked, depenalization policies in one borough will change the behavior of cannabis users in *all* boroughs, and potentially induce drug tourism across boroughs.

As the population in the rest of London (N_{0t}) is orders of magnitude larger than that in Lambeth (N_{1t}), there can be very large impacts on the size of cannabis market in Lambeth as the result of a localized depenalized policy. As made precise below, this channel of consumers moving location to buy cannabis would be considerably weakened in the presence of a citywide depenalization policy that ensures the punishment for cannabis related criminal activities remained homogenous across locations $(\alpha_{bt} = \alpha_{-bt})$.

[37] The assumption that δ and λ^D are uncorrelated is driven by the available data: we do not have individual crime data to identify the provenance of offenders, so any correlation between these parameters cannot be identified.

Policing and Arrests for Cannabis Offenses

Each borough has its own police force, and we assume each acts independently of the other.[38] The size of the police force, or total police resources, in location b is denoted P_{bt}. A fraction, ϕ_{bt}, of these resources are devoted to cannabis related crime. The number of individuals arrested for cannabis crime is a function of police resources allocated towards such crime and the market demand for cannabis in location b, D_{bt}.[39] We postulate a Cobb-Douglas specification for this relation,

$$\text{Arrests}_{bt}^{D} = \gamma_D(\phi_{bt}P_{bt})^{\omega_D} D_{bt}^{1-\omega_D},\ \omega_D \in [0,1]. \tag{3.6}$$

7.1.3 Non-Drug Crime

Individuals from both locations choose whether to commit non-drug crime, and where to commit it. Following a similar formulation as above, we assume individuals are heterogenous in two dimensions: the propensity to commit crime, and the cost of moving from one borough to another. The utility of committing crime depends on: (i) an individual specific utility component, χ; (i) the moving cost if there is a change of location, λ^C; (iii) the cost of being apprehended by the police, $\beta\pi_{bt}^C$: π_{bt}^C is the (endogenous) detection rate for non-drug crime in location b at time t, where we assume individuals are caught for non-drug crime in the location of the crime. β is the cost of committing non-drug crime when apprehended and is the same across locations. Normalizing the utility from not committing crime to zero, the utility of committing crime in one of the two locations is then given by u_{it}^C where,

$$u_{it}^C = \max[u_{ibt}^C, u_{i,-bt}^C], \tag{3.7}$$

$$\begin{aligned} u_{ibt}^C &= \chi - \overline{\chi}_b - \beta\pi_{bt}^C && \text{if committing crime in } b, \\ u_{i,-bt}^C &= \chi - \overline{\chi}_b - \lambda^C - \beta\pi_{-bt}^C && \text{if committing crime in } -b. \end{aligned} \tag{3.8}$$

[38]This matches the evidence in Table A1 on police operations in London boroughs in our study period: there is little evidence of a spike in police operations in other London boroughs around the time of the LCWS to potentially offset any of its impacts.

[39]We are implicitly assuming that all (or a fixed fraction of) cannabis crimes are notified to the police, so the number of cannabis offences equals $D_{bt}(\pi_{bt}^C, \pi_{-bt}^C)$ (or some fraction of $D_{bt}(.)$). As discussed earlier, the depenalization policy should have no impacts on police behavior in terms of their searching for cannabis offences. Hence we focus on how these offences convert to arrests, that is a margin directly affected by the policy.

Individual i commits crime if $u^C_{it} > 0$. We assume χ is uniformly distributed over $[0, 1]$; $\overline{\chi}_b$ determines the share of individuals that commit crime in the absence of policing (if $\pi^C_{bt} = 0$). Again, we allow for different propensities to commit crime between Lambeth and the rest of London, by allowing $\bar{\chi}_b$ to vary across location. We assume χ is uncorrelated with the moving cost λ^C. In consequence, χ is also then uncorrelated with δ so that an individual's underlying propensity to use cannabis is unrelated to their underlying propensity to commit non-drug crime.[40] The number of crimes committed in location b is then given by,

$$C_{bt}(\pi^C_{bt}, \pi^C_{-bt}) = N_{bt} \Pr(u^C_{ibt} > u^C_{i,-bt}, u^C_{ibt} > 0) + N_{-bt} \Pr(u^C_{i,-bt} > u^C_{i,-bt}, u^C_{i,-bt} > 0), \quad (3.9)$$

and we assume all crimes are notified to the police, so the number of non-drug criminal offences equals $C_{bt}(\pi^C_{bt}, \pi^C_{-bt})$. As with the market demand for cannabis, the number of crimes committed in location b depends on characteristics and police behavior across *both* locations.

Finally, the number of arrests for non-drug crime in location b will then depend on the fraction $(1 - \phi_{bt})$ of police resources P_{bt} are devoted to non-drug crime in location b, and the actual number of non-drug crimes committed. We again assume a Cobb-Douglas relationship so that,

$$\text{Arrests}^C_{bt} = \gamma_C((1 - \phi_{bt})P_{bt})^{\omega_C} C_{bt}^{1-\omega_C}, \omega_C \in [0, 1]. \quad (3.10)$$

Equilibrium Detection Rates

The key endogenous outcomes in the model are detection rates for cannabis and non-drug crime in each location, (π^D_{bt}, π^C_{bt}). Detection rates are the ratio of the number of offenders caught by the police, to the total number of offenders. Hence they are determined through an interaction of the police and cannabis users and are the solution to the following system

[40] Of course this assumption could be relaxed to capture the fact that cannabis markets might correlate with some non-drug crimes, such as property crime [Fergusson and Horwood 1997, Corman and Mocan 2000]. However, we would need to find more detailed individual crime data, that for example recorded multiple offences where relevant, to incorporate this feature into the model.

of equations:

$$\begin{aligned}\pi_{bt}^{D} &= \frac{\gamma_D(\phi_{bt}P_{bt})^{\omega_D} D_{bt}(\pi_{bt}^{D}, \pi_{-bt}^{D})^{\omega_D}}{D_{bt}(\pi_{bt}^{D}, \pi_{-bt}^{D})}, \\ \pi_{bt}^{C} &= \frac{\gamma_C((1-\phi_{bt})P_{bt})^{\omega_C} C_{bt}(\pi_{bt}^{C}, \pi_{-bt}^{C})^{\omega_C}}{C_{bt}(\pi_{bt}^{C}, \pi_{-bt}^{C})}.\end{aligned} \tag{3.11}$$

Given the non-linearity of this system, there are no closed form solutions for (3.11). We therefore solve the model numerically, by searching for the detection rates that bring the left- and right-hand sides in (3.11) as close as possible, where a solution consists of four detection rates: $\{\pi_{0t}^{D}, \pi_{1t}^{D}, \pi_{0t}^{C}, \pi_{1t}^{C}\}$. By looking at the whole support of the detection rates, $[0, 1]$, we find all the sets of detection rates that solve the system of equations (3.11), for a given value of the parameters. For any set of equilibrium detection rates we then compute the market demand for cannabis, the number of offences for cannabis and non-drug crime, the number of arrests for non-drug crimes in all locations. This is done by using equations (3.5), (3.6),(3.9) and (3.10). There can be multiple equilibria generated, and how we make the choice between these equilibria is explained below when we detail the calibration procedure.

Modeling the Localized Policing Experiment

We define two time periods and denote by $t = t_B$ the time period *before* the policy is implemented (corresponding to the period from April 1998 to June 2001) and denote by $t = t_A$ the time period *after* policy is introduced (from January 2002 to March 2004). We discard the first six months of the policy to allow for transitional dynamics. We model the localized policing experiment in Lambeth as operating through two channels. First, a reduction in the penalty of being caught in possession of cannabis, closely matching the policy description in Section 2. The penalty is α_{1t_B} in Lambeth before the policy, and decreases to $\alpha_{1t_A} < \alpha_{1t_B}$ in the post-policy period.[41] We assume that in the rest of London, the penalty for cannabis arrest is the same during the two periods, and that pre-policy it is similar to the penalty in Lambeth ($\alpha_{0t_B} = \alpha_{0t_A} = \alpha_{1t_B}$).

Second, we allow the police to reallocate their resources between cannabis and non-drug crime. In our model, this is captured by the fact that, in Lambeth, $\phi_{1t_A} < \phi_{1t_B}$.

[41] As described in Section 2.2, Lambeth's cannabis policing strategy did not return identically to what it had been pre-policy. Rather, it adjusted to be a firmer version of what had occurred during the pilot. As evidenced in Columns 3 and 4 of Table 3, there was a permanent reduction in police effectiveness against cannabis possession crime in Lambeth.

We assume that in the rest of London, there is no change in the fraction of the police force dealing with cannabis crime ($\phi_{0t_B} = \phi_{0t_A}$). This channel creates a linkage between cannabis crime and non-drug crime, that the reduced form evidence suggested was an important policy impact to consider.

Such a localized policy change operating only in Lambeth ($b = 1$), will then have two impacts on the market demand for cannabis in Lambeth ($D_{1t_A}(\pi^D_{1t_A}, \pi^D_{0t_A})$): (i) Lambeth residents will be more prone to consume cannabis; (ii) residents in the rest of London will be more inclined to travel to Lambeth to purchase cannabis. These changes will affect the equilibrium detection rates for cannabis crime ($\pi^D_{bt_A}$), that will in turn determine the equilibrium proportion of the population consuming cannabis and the number of cannabis users caught by the police. If the policy allows a re-allocation of the police force towards non-drug crime (so $1 - \phi_{bt}$ increases), the policy impacts will then spill over to other crimes, changing the equilibrium detection rates for non-drug crime ($\pi^C_{bt_A}$) and thus the proportion of the population that chooses to commit non-drug crime.

3.7.2 Calibrating the Model to the Localized Policing Experiment

Calibration Method

The model has 16 parameters: (i) five parameters describe preferences towards cannabis consumption, moving across boroughs, and penalties associated with arrests: $\bar{\delta}_0$, $\bar{\delta}_1$, $\bar{\lambda}$, α_{1t_B}, α_{1t_A}; (ii) three parameters describe non-drug crime preferences and penalties: $\bar{\chi}_0$, $\bar{\chi}_1$, β; (iii) eight parameters describe the arrest production functions: γ_{D0}, γ_{D1}, ω_D, γ_C, ω_C, ϕ_{0t_B}, ϕ_{1t_B}, ϕ_{1t_A}. We allow the arrest technology parameter for cannabis crime γ_D to vary between boroughs, as the two locations have different arrest rates, conditional on offences. For non-drug crime, a good model fit is achieved with a common parameter γ_C.

We calibrate all but two of the model parameters based on the localized LCWS policy to reproduce key features in the data. It is difficult to identify the parameter ϕ_{bt}, which is the fraction of the police force devoted to cannabis crime, based only on observed crime in the pre-policy period. We therefore identify this parameter from other sources of data as detailed below. The variation introduced by the LCWS, and in particular the differential change in non-drug crimes across boroughs and time then allows us to identify the *change* in the fraction of police time devoted to cannabis crime in Lambeth (i.e. ϕ_{1t_A}/ϕ_{1t_B}).

We rely on data moments computed for Lambeth and the rest of London, and for two periods: before the LCWS policy is in place, and the post-policy period. We have a total of 17 moments which describe: (i) the prevalence of cannabis consumption; (ii) the number of recorded offences for cannabis; (iii) the number of offences for other crimes; (iv) the number of arrests for other crimes; (v) the share of cannabis users in Lambeth from other London boroughs pre-policy. These moments are chosen because they are direct outputs of the model and because they best capture all the key policy impacts documented in the earlier reduced form evidence. We now describe how each of these empirical moments is measured.

On (i), data on cannabis consumption for the rest of London is derived from the British Crime Survey (BCS), that asks about cannabis usage. We use the 2000/1 and 2006 survey waves to measure cannabis consumption pre- and post-policy in the rest of London. As the BCS has only few respondents in Lambeth, we estimate the prevalence of cannabis consumption in Lambeth by scaling the BCS-derived figure for the rest of London by the ratio of cannabis offences in Lambeth to those in the rest of London. We do so for the pre- and post-policy periods. Implicit in this scaling is the assumption that the relationship between cannabis use and offences for cannabis possession is the same in all locations. As highlighted throughout, LCWS policy would not alter how the police would track or record offences, all else equal.

For moments (ii) to (iv), data on offences and arrests are taken from the same administrative crime records from the MPS as used in the reduced form analysis. For the calibration exercise, offence and arrest rates are expressed per 1,000 inhabitants. Finally, on (v) the share of cannabis consumers in Lambeth from outside the borough in the pre-period is recovered from an MPA document.[42]

Our model requires three additional inputs: population size, the number of police officers in each location, and the fraction of police time dealing with cannabis crime. The former is obtained from the QLFS-LA data described earlier. For the second, we use data from MPA reports described in Section 5.3, that reports the number of police officers both in Lambeth and the rest of London, during the pre-policy and the post-policy periods. As described earlier, during this time span, the number of officers have increased in both locations, at approximately equal rate.

[42]The share of cannabis consumers in Lambeth from outside the borough in the pre-period is mentioned in Appendix 6 of the minutes of the following MPA committee meeting: http://policeauthority.org/Metropolitan/committees/mpa/2002/020926/17/index.html

To compute the fraction of police devoted to cannabis crime before the policy, ϕ_{bt_B}, we rely on additional data that characterize the number of hours taken up by arrests linked to cannabis possession and total effective police time. We denote by $Hours\ Proc^D$ the hours taken to process a cannabis arrest, which includes the transfer of the offender to the police station, file processing and time spent in prosecution. We use data from police reports which evaluates the time required to process each arrest linked to cannabis to about seven hours [Wood 2004].[43]

We obtain an estimate of total effective police time by multiplying the size of the police force in a given borough, as recorded by the MPA and discussed in Section 5.3, by an estimate of the time spent by the average police officer on effective policing in London each year (namely net of time on holiday, sick days, training attendance and other administrative work). Herbert *et al.* [2007] provide an estimate of this effective police time. The fraction of police time devoted to cannabis arrests is then obtained by

$$\phi_{bt_B} = \frac{\overline{Arrests}^D_{bt_B} * \text{Hours Proc}^D}{\text{Total effective police time}_{bt_B}}. \tag{3.12}$$

where $\overline{Arrests}^D_{bt_B}$ is the average number of arrests for cannabis offences in borough b, in the pre-policy period.[44]

Given these inputs to the model from other data sources, the calibration of the remaining parameters is obtained using a minimum distance method, where we minimize the quadratic distance between the observed and predicted moments, equally weighting each moment. For a given value of the parameters, we may have several predictions, due to multiple equilibria. We compute the distance for all possible equilibria and select the one that brings the predicted and observed moments the closest. The model was solved numerically using 20,000 simulations draws, a number large enough so that increases in

[43] The PRS consultancy group, which evaluated the pilot scheme at the 6 month point, estimated that for every individual apprehended with cannabis where a caution rather than an arrest was issued, three police hours were saved by avoiding custody procedures and interviewing time. However, the MPA noted that the three hours per offence figure was conservative, as it "was based on the premise of an officer working alone. It took no account of the time spent transporting the arrested person to a police station and the time waiting to book them in on arrival". A later MPA report following the nationwide declassification stated the time saving was five hours dealing with a cannabis arrest and two more hours operational time at police stations [Wood 2004]. We use this stated seven hour reduction in processing time to calibrate the model.

[44] Hence we focus on modeling the time devoted to processing arrests rather than the time devoted to recorded offences or warnings. On the time devoted to offences, there should be no change in how offences are recorded because of the policy, as discussed earlier. On time devoted to warnings, we make the simplifying assumption that the time involved issuing a warning is negligible compared to the time involved in arresting and processing an offender. This seems reasonable as a warning can be issued verbally with no formal paperwork being required.

simulations did not change the objective function. The search was done using a gradient free optimizer built on the Simplex method. Finally, we note that the estimation was started with many different initial parameter values, to ensure that it converged to a global minimum.

Results

Panel A of Table 8 presents the observed and predicted moments described above: the model does a good job in matching the moments. For 8 (15) of the moments, the difference between the observed and predicted moment is less than 5% (10%). A χ^2 goodness-of-fit does not reject the hypothesis that the predicted moments are jointly the same as the observed ones. Column 5 of Table 8 displays a transformation of the key moments related to crime: the difference-in-difference for recorded log offences of cannabis and non-drug crimes. These are calculated across locations and time and transformed into percentages, and are therefore comparable to the reduced-form results discussed earlier. Along this dimension, our model is able to reproduce two of the keys impacts of the policy quite well: (i) the model predicts a 66.4% increase between the pre and post policy periods, in recorded cannabis offences in Lambeth relative to the rest of London (compared to an observed difference-in-difference increase of 64.8%); (ii) the model predicts a 4.95% reduction in non-drug crime, compared to an observed decrease of 7.26%.

Moreover the model highlights an important mechanism that was not captured in the reduced form results, shown in Panel B: there is a re-location of cannabis consumers from the rest of London towards Lambeth post policy. The share of cannabis consumers in Lambeth that are from the rest of London matches the observed one (39%) before the policy was in place. The model predicts that this share rises from 39% pre-policy to 60% under the localized depenalization policy. This near doubling of drug tourists shows how the interlinkage in cannabis markets across locations is a key reason why offence rates for cannabis related crime in Lambeth rises so much with the localized depenalization policy.

Table A5 shows the calibrated parameter values from this exercise. Panel A focuses on the two parameters describing the initial (exogenous) channels through which the policy operates as discussed above: $\alpha_{1t_A}/\alpha_{1t_B}$ and ϕ_{1t_A}/ϕ_{1t_B}. As shown in the first row, the data is matched with a reduction in the penalty of getting caught with cannabis in Lambeth by about 82%. This captures the fact that all recorded offences lead to arrests pre-policy, while most offenders were left with only a caution afterwards (the exception being those offences that occurred post-policy that had aggravating factors). The policy is also associated with a re-allocation of about 53% of police time in Lambeth devoted to cannabis pre-policy, to non-drug crime afterwards. To be clear, this change in Lambeth should be interpreted as the combined effect from any re-allocation of police resources, changes in processing times for arrests post-policy, or the differential hiring of police for cannabis and other crimes post-policy: all these channels are captured in a reduction in ϕ_{1t_B} relative to ϕ_{1tA}.[45]

Panel C of Table 8 displays the equilibrium detection rates for cannabis and non-drug crime, for each location and period. The detection rates for cannabis consumption are very small, reflecting the fact that a sizeable fraction of the population uses cannabis and very few of them are actually arrested each year. For non-drug crimes, offences are rarer and arrests relatively more frequent: Panel C shows around 12% of non-drug crimes lead to an arrest (in contrast, only 0.2% of cannabis users are arrested). In Column 5 of Table 8 we also report the difference-in-difference for the detection probabilities, again normalized by their pre-policy levels in Lambeth. Detection rates for cannabis crime declined in Lambeth relative to the rest of London by around 5.13%, while the detection rate for non-drug crimes remains almost unchanged.

To assess the plausibility of our calibrated model, we compute the elasticity of total recorded criminal offences with respect to the size of the police force, namely the elasticity of $C_{bt}(\pi^C_{bt}, \pi^C_{-bt}, .) + D_{bt}(\pi^C_{bt}, \pi^C_{-bt}, .)$ with respect to P_{bt}, the total number of police officers in location b. Earlier studies have estimated this elasticity, exploiting very different research designs. Our structural model predicts an elasticity of -0.3 in Lambeth and about -0.9 in the rest of London. The estimates of this elasticity in the literature range from 0 [McCrary 2002] to -0.9 [Lin 2009], and many studies find an elasticity of the order of -0.3 to -0.5 [Levitt 1997, 2002, Corman and Mocan 2000, Draca *et al.* 2011]. Hence, although our model was not calibrated to match these elasticities, they appear to be consistent with previous results and provide external validity to our method.

[45] On the other calibrated parameters, Panel B of Table A5 shows the preference parameters are such that a higher share of the Lambeth population would consume cannabis absent policing ($\bar{\delta}_1 < \bar{\delta}_0$), but that the disutility from committing crime is near identical across locations ($\bar{\chi}_1 = \bar{\chi}_0$). Panel C shows the calibrated policing technology parameters and suggests the TFP-like parameter on the apprehension technology for cannabis crime is higher in London than Lambeth ($\gamma_{D0} > \gamma_{D1}$). The corresponding TFP-like parameter for non-drug crime is fixed to be the same across locations, but we note its value is orders of magnitude higher ($\gamma_C > \gamma_{D0}, \gamma_{D1}$) so that individuals are far more likely to be arrested for non-drug crime than for cannabis related crime.

3.7.3 A Counterfactual Policy Experiment: Citywide Depenalization

We now use the calibrated model to perform a counterfactual policy analysis, which decreases the penalty of cannabis consumption citywide. Hence in *both* locations we allow the penalty to fall by the same extent, as captured by the ratio $\alpha_{1t_A}/\alpha_{1t_B}$. We also adjust the police time devoted to cannabis crime in each borough to match the change we observe (ϕ_{1t_A}/ϕ_{1t_B}). Table 9 shows the change over time in a number of key statistics, expressed as a percentage change from the baseline level of the statistic in the pre-policy period, as a result of a citywide depenalization.

This exercise shows the following. First, Panel A highlights that the citywide depenalization of cannabis possession leads to a modest increase in the prevalence of cannabis consumption, of about 1% in Lambeth and 2% in London (where the baseline prevalence is lower).[46] Second, other crimes in the rest of London would actually fall in the citywide policy (by around .3%) as all police forces reallocate effort towards non-drug crime.

Third, Panel B highlights that in Lambeth, the share of cannabis users originating from outside the borough decreases by 4% compared to the baseline (and by more than 60% compared to the actual localized policy period). In short, a citywide policy would much eliminate drug tourism, that is a key driving force in the localized experiment. Fourth, Panel C highlights how a citywide policy would impact equilibrium detection probabilities across crime types: in both locations the structural model predicts a fall in the detection rate for cannabis consumption by around 7%, and an *increase* in equilibrium detection rates for non-drug crimes of around .2%.[47]

Linking these findings back to the documented welfare impacts in Section 6, we see that because citywide depenalization eliminates incentives for drug tourism, the cannabis market in Lambeth increases in size less dramatically than under a localized depenalization policy. As such, any anti-social behaviors that are correlated to the size of

[46] This result contributes to the literature on the impact of drug policies on drug usage, on which the evidence remains mixed [DiNardo and Lemieux 2001, Pudney 2010, Damrongplasit *et al.* 2010]. Braakmann and Jones [2012] evaluate the impact of the declassification of cannabis in the UK in 2004 on cannabis consumption: they find the policy to increase cannabis consumption, predominantly because of individuals starting to consume cannabis.

[47] We can validate some of the model's predictions using the actual nationwide depenalization of cannabis possession that took place from January 2004 until January 2009. This was implemented in a rather similar way as the Lambeth policy. We estimate the reduced form impacts of this policy on crime using a simple before-after comparison, that is obviously subject to far more caveats than the difference-in-difference design we used to evaluate the LCWS. In addition, the demographic controls from the QLFS-LAD data are only available until 2006 Q1 so these have to be extrapolated until 2010 to estimate the impacts of the nationwide policy. Doing so we find that crimes related to cannabis possession significantly rise when the nationwide policy is in place, and that offence rates for other non-drug crimes significantly fall during this period (and police effectiveness against them rises).

the cannabis market but are not captured in crime rates, might then be reduced. Hence citywide depenalization might then have far smaller negative impacts on property prices in Lambeth compared to the documented impacts of a localized policing experiment.

3.8 Conclusion

Cannabis users account for 80% of the 200 million illicit drug users in the world [WDR 2010]. Understanding the impacts of government intervention in the market for cannabis is of huge importance. In this paper we study the impacts of a common intervention: the depenalization of cannabis, where the possession of small quantities of cannabis no longer leads to individuals being arrested (although such incidents are still recorded as offences). More precisely, we evaluate the impacts on the level and composition of crime, and social welfare as measured by house prices, of a localized depenalization policy that was implemented in the London borough of Lambeth.

We have documented how the policy changed crime patterns during and after the depenalization policy, using administrative records on criminal offences by drug type, by specific drug offences that proxy demand and supply side criminal activities, and for seven types of non-drug crime. We find that depenalization in Lambeth led to an increase in cannabis possession offences that persisted well after the policy experiment ended. We find evidence the policy enables the police in Lambeth to be able to re-allocate their effort towards non-drug crime: there are significant long run reductions in five non-drug crime types, and significant improvements in police effectiveness against such crimes as measured by arrest and clear-up rates.

The totality of evidence is best interpreted through the depenalization policy causing a behavioral response of the police among two dimensions: to reduce the penalties of being caught in possession of cannabis, and to reallocate resources towards non-drug crime. Both channels then cause an endogenous response among potential users of cannabis in terms of the choices over whether and where to buy and consume cannabis from. We use the key lessons from this localized policing experiment to shed light on what would be the impacts on crime if the same policy were to be applied *citywide*, by developing and calibrating a model of the market for cannabis and crime, accounting for the behavior of police and cannabis users.

While our model highlights some novel and important channels through which a depenalization of cannabis affects the level and composition of crime, it still leaves open areas for future research on how illicit drug policy affects the behavior of drug suppliers and the police. In particular, on drug suppliers, research on how drug policies change the organization of criminal activity remains scarce; and on police behavior, much remains to be understood regarding the extent to which police across jurisdictions should coordinate strategies.

We have provided a comprehensive review of the impact of depenalization policies along four margins: drug and non-drug crimes, the location of crimes, short and long run policy responses, and impacts on welfare as measured by house price changes. Our detailed and nuanced reduced form and structural form results are relevant for other settings given the depenalization policy we study reflects how liberal drugs policies have been implemented by many other countries [Donohue *et al.* 2011], and the issue of whether and how governments should intervene in illicit drug markets remains at the top of the political agenda across the world.[48]

.1 A Appendix

.1.1 A.1 Crime Data: Definitions

Home Office counting rules for criminal offences are periodically revised, including in 1998, so coinciding with the start of our sample period. Importantly, changes in Home Office guideline/definition are uniformly applied across *all* London boroughs, and hence will not drive the difference-in-difference estimates on crime. There was another revision in the recording of crime in April 2002, with the introduction of the National Crime Recording Standard (NCRS). The *Crime in England and Wales 2004/5 Report* states the NCRS "aimed to introduce greater consistency to the process of recording crime and to establish a more victim-oriented approach to recording. The impact of the NCRS. . . was to increase the numbers of crimes recorded and less serious violent offences were particularly affected." In a robustness check in Table A2, we re-estimate our baseline results on the impact of the LCWS policy on drug crime by additionally adding in a series of dummies equal to one for when each data regime is in place, and zero otherwise.

[48] For example, Colorado and Washington states legalized possession of one ounce or less of marijuana for recreational use by adults (those 21 years or older) in November 2012. At least twelve other states are considering similar policies. In Europe, Croatia decriminalized the possession of small amounts of cannabis in 2013. In Latin America, Uruguayan president José Mujica has proposed to put into place a legal state-controlled market for cannabis.

There have been a number of changes to recording practices and the sanctions available that have affected the recorded clear-up (detection) rates. The Home Office Counting Rules for recorded crime changed from April 1998. These brought new offences into the series with varying clear-up rates. It is estimated that the effect of the changes was to increase the overall clear-up rate from 28% to 29%. Additional changes were implemented with effect from April 1999. Any recorded clear-up required: 'sufficient evidence to charge', and, an interview with the offender and notification to the victim. In addition, clear-ups obtained by the interview of a convicted prisoner ceased to count. The overall effect of the April 1999 change is estimated as a single percentage point decrease in clear-up rates (although the effect varied between crime types). Finally, the implementation of the NCRS in April 2002 is thought to have had an inflationary effect on recorded crime and the assumption is that it has depressed clear-up rates since additional recorded crimes are generally less serious and possibly harder to clear-up.

.1.2 A.2 Cannabis Crime: Robustness Checks

Table A2 presents a series of robustness checks on the baseline result documented in Table 2, that the LCWS policy led to a significant increase in offence rates for cannabis related crime in Lambeth relative to the rest of London between the pre-policy and policy period; this effect persisted in the long run post-policy.

The first robustness check excludes geographic neighbors to Lambeth when estimating (3.1). We define the geographic neighbors of Lambeth to be the boroughs that have contiguous land borders with Lambeth: Croydon, Merton, Southwark and Wandsworth. Given the interlinkages between cannabis markets and the dense network of public transport across boroughs in Lambeth, we expect cannabis users to travel to Lambeth to purchase cannabis in response to the policy (the lower costs of apprehension and the endogenous reduction in detection rates). If such users originate only from neighboring boroughs, then by excluding such neighbors from (3.1), we will estimate the true impact of the policy on cannabis crime in Lambeth. The result in Column 2 shows the impacts to be almost unchanged if the neighbors to Lambeth are excluded as controls. This suggests that cannabis users that switch to purchasing cannabis from Lambeth because of the policy are likely to originate from all over London.

The next robustness check in Column 2 accounts for common citywide shocks to cannabis crime through the inclusion of year fixed effects into (3.1). The differential impacts of the LCWS policy in Lambeth during and after the policy period are identified because these periods cut across years. We see the coefficients of interest are very similar to the baseline specification. Column 3 then shows the results to be robust to including a series of dummy variables for when different data regimes are in place (as described in the subsection above); Column 4 shows the baseline results to be robust to additionally including the full set of police operations operating in single or groups of boroughs (Panel A in Table A1) where start and end dates of the operation are known.

Finally, Column 5 allows for spatially correlated error structures. Given the inter-linkages in cannabis markets across locations, as well as the possibility of police across boroughs coordinating strategies, then there might be correlation in the error structure in (3.1). To account for this possibility we model the error term as follows,

$$u_{bmy} = u_{bt} = \rho W u_{bt} + e_{bt}, \tag{13}$$

where ρ is the coefficient on the spatially correlated errors, and W is the spatial weighting matrix of dimension (32×32) as there are 32 London boroughs. We specify W to be a contiguity spatial weighting matrix, where $w_{ij} = 1$ if borough j neighbors borough i, and 0 otherwise. The result in Column 5 is similar to the baseline estimate: the parameters of interest remain of the same sign and significance, and both point estimates are marginally larger. For this model, $\widehat{\rho} = .346$ with a standard error of .0224, indicating the presence of spatial correlation in the error terms. We have also experimented with several other W specifications, including inverse distance and inverse distance squared matrices (distance is calculated as the Euclidian distance from the centroid of each borough to all others), and found results to be robust to these different weighting matrices.

.1.3 A.3 Defining Crime Hotspots

In analyzing the impact of the depenalization policy on house prices in Lambeth relative to other London boroughs, we exploit the fact that data on house prices and crime is available, for some years, at a more disaggregated level within each borough. House price data is available at the zip code sector level from the *UK Land Registry*. Ward-level crime data is available monthly from April 2001, from the MPS. We use the ward level crime data to first define each ward as a crime hotspot, and we first describe how this is done. We then describe how we match ward level crime data to zip code sectors that house price data is available for (as wards and zip codes do not correspond to the same geographic areas), to ultimately define zip codes as being crime hotspots.

Given our policy focus, our primary hotspot measure is based on the incidence of drug crime in each ward. A ward is defined as a hotspot if drug offences are above the median for all wards in the same borough. One of the robustness checks described below experiments with using an alternative threshold for defining ward hotspots.

The ward-level crime data is available monthly from April 2001. We use this to create hotspots based on two definitions: (i) *ex ante* levels of drug crime, using the three months of data prior to the start of the LCWS; (ii) *ex post* levels of drug crime, based on ward level drug crime rates in the period October 2007 to September 2009.

Once the ward-level hotspots are defined, these must be mapped onto zip code sectors, to be able to create zip code sector hotspot markers to include as three-way interactions in the house price regression (3.2). In general, zip code sectors are smaller than wards, but more importantly the two do not perfectly overlap. The average number of wards in a zip code sector is 4.1 (even though zip code sectors are the smaller unit of the two). For our baseline specifications, we then define a zip code sector (e.g. WC1E), to be a hotspot if any ward within a zip code sector is defined as a drug crime hotspot. A second set of robustness checks described below experiment with using alternatives methods for defining a zip code as being a hotspot. Each zip code sector is then ascribed to be either a hotspot or not. Figure A1A shows for Lambeth, the classification of zip code sectors into hotspots and non-hotspots based on the *ex-post* definition. Given the concerns described of using an *ex post* definition, Figure A1B shows the classification of zip code sectors into hotspots if we use the three months of *ex ante* ward level crime data to define hotspots. Reassuringly, there is considerable stability in the definition of hotspots over this time period and using this method: as a result the empirical house price results are very similar when using either definition (Columns 3 and 4, Table 6).

.1.4 A.4 House Price Impacts: Robustness Checks

Table A4 presents robustness checks on the main house price regression in Table 6. Column 1 repeats the baseline specification using zip code sector hotspots, but for another housing type: flats, that actually correspond to the most frequent house type sale in our study period (although the lowest price per sale for any house type). The basic pattern of results holds for this housing type also: post-policy, house prices for flats are significantly lower in Lambeth than other boroughs, and there is enormous variation within Lambeth between zip codes classified as hotspots (where house prices are 20.2% lower than in other London boroughs post-policy), and zip codes in Lambeth that are not classified as hotspots (where house prices are actually 5.3% higher in Lambeth than comparable areas in other London boroughs post-policy).

The remaining robustness checks examine the robustness of the findings to alternative definitions of hotspots. The first check redefines how a ward is first defined to be a hotspot. More precisely, we define a ward as a hotspot if drug offences are above the 75th percentile median for all wards in the borough. We then define a zip code to be a hotspot if it contains any hotspot wards so defined. Column 3 examines the robustness of the baseline result to changing how we translate ward hotspots into defining a zip code sector as being a hotspot. While the baseline specification denotes the zip code sector to be a hotspot if any ward is defined to be a hotspot, in Column 3 the zip code is defined to be a hot spot if the modal ward is itself defined to be a hotspot. Column 4 then uses an alternative method to define zip code sectors as hotspots that uses information on all wards in the zip code sector. In this case, the hotspot variable is no longer binary, but rather a weighted average of all wards' hotspot classifications within the zip code sector. These weights are based on the percentage of the zip code that overlaps with the ward. Finally, Column 5 uses information on total crimes (not drug crime) to redefine wards and then zip codes as hotspots using otherwise the same method as the baseline specification.

The results in Columns 2 to 5 on Table A4 are all very much in line with the baseline findings in Table 6. In particular, for all variant specifications we see that post-policy, house prices are significantly lower in Lambeth hotspots than other boroughs, where the magnitude of the impact varies between 7.7% and 13.9%.

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Table 1: Detailed Drug Offences, Pre-policy Period

Means and standard deviations in parentheses

	(1) Lambeth	(2) Other London Boroughs
A. Total		
Total drugs offences per 1000 of adult population	.608	.400
	(.124)	(.298)
B. Drug Type		
Share of drug offences relating to any cannabis offences	.600	.735
	(.052)	(.108)
Share of drugs offences related to Class-A drugs	.344	.204
	(.054)	(.106)
Share of drugs offences related to Class-B drugs (including cannabis)	.628	.770
	(.057)	(.110)
Share of drugs offences related to Class-C drugs	.002	.004
	(.004)	(.010)
C. Cannabis Offences Breakdown		
Share of cannabis offences relating to having possession of cannabis	.907	.918
	(.044)	(.055)
Share of cannabis offences relating to having possession of cannabis with intent to supply	.055	.049
	(.031)	(.043)
Share of cannabis offences relating to production/being concerned in production of cannabis	.015	.013
	(.016)	(.021)
Share of cannabis offences relating to supply or offer to supply cannabis	.023	.019
	(.020)	(.027)

Note: The pre-policy period runs from April 1998 until June 2001. Other London boroughs are all London boroughs, except Lambeth. Standard deviations are in parentheses. Class-A drugs are cocaine, crack, crystal-meth, Heroin, LSD, MDMA and methadone; Class-B drugs are amphetamines and cannabis (in the pre-policy period); Class-C drugs are anabolic steroids, GHB and ketamine.

Table 2: The Effect of the Depenalization on Cannabis Offences in Aggregate

Dependent Variable: Log (total recorded cannabis offences, per 1000 of adult population)

	(1) Fixed Effects	(2) Baseline	(3) Borough Specific Linear Time Trend	(4) Borough Specific Quadratic Time Trend	(5) Within Policy Dynamics
Lambeth x Policy Period	.325***	.293**	.195	.182	
	(.117)	(.118)	(.148)	(.145)	
Policy Period	.018	.034	.023	.182***	.034
	(.056)	(.056)	(.065)	(.051)	(.056)
Lambeth x Post-Policy Period	.615***	.610***	.414**	.479**	.682***
	(.092)	(.096)	(.201)	(.186)	(.076)
Post-Policy Period	.171***	.181***	.160*	.237***	.180***
	(.043)	(.047)	(.090)	(.066)	(.047)
Lambeth x Policy Period [1-6 months]					-.026
					(.120)
Lambeth x Policy Period [7-13 months]					.647***
					(.118)
Borough and Month Fixed Effects	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	No	Yes	Yes	Yes	Yes
Observations	3008	3008	3008	3008	3008

Note: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. The sample period runs from April 1998 until January 2006. Control boroughs are all other London boroughs. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total London population that month-year in the borough. The policy period dummy variable is equal to one from July 2001 until July 2002, and zero otherwise. The post-policy period dummy variable is equal to one from July 2002 onwards, and zero otherwise. Column 1 only additionally controls for borough and month fixed effects. In Column 2 onwards, the following socio-demographic control variables, measured in logs, are controlled for at the borough-month-year level: the share of the adult population that is ethnic minority, that is aged 20 to 24, 25 to 34, 35 to 49, and aged above 50, and the male unemployment rate. Column 3 (4) additionally controls for a borough specific linear (quadratic) time trend.

Table 3: The Effect of the Depenalization on the Demand and Supply of Cannabis Related Crime

Crime Series:	Cannabis Possession (Demand)				Cannabis Supply			
Offence Type:	(1) Offences	(2) Arrests	(3) Clear-ups	(4) Clear-ups per Arrest	(5) Offences	(6) Arrests	(7) Clear-ups	(8) Clear-ups per Arrest
Lambeth x Policy Period [1-6 months]	-.036	-.436**	-1.556***	-1.199***	.236	-.250	-.287*	-.043
	(.127)	(.192)	(.349)	(.212)	(.167)	(.176)	(.173)	(.087)
Lambeth x Policy Period [7-13 months]	.675***	-.946***	-1.558***	-.490*	.505***	-.149	-.095	.039
	(.124)	(.181)	(.393)	(.266)	(.165)	(.166)	(.163)	(.081)
Policy Period	.035	-.010	-.027	-.017**	-.016	-.024	-.023	.007
	(.055)	(.063)	(.065)	(.008)	(.064)	(.043)	(.043)	(.015)
Lambeth x Post-Policy Period	.686***	-.094	-1.047***	-.576**	.676***	-.007	.077	.077*
	(.080)	(.102)	(.357)	(.288)	(.101)	(.093)	(.089)	(.046)
Post-Policy Period	.192***	-.049	-.028	.022***	.034	-.069**	-.064**	.003
	(.046)	(.047)	(.048)	(.007)	(.043)	(.032)	(.031)	(.012)
Borough and Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3008	3008	3008	3008	2756	2722	2711	2987

Note: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. The sample period runs from April 1998 until January 2006. Control boroughs are all other London boroughs. The dependent variable in Columns 1 and 5 is the log of the number of offences for each offence type, per 1000 of the adult population. The dependent variable in Columns 2 and 6 is the arrest rate for each offence type, defined as the log of the number of arrests divided by the number of offences in the borough in the same month and previous quarter. The dependent variable in Columns 3 and 7 is the clear-up rate for each offence type, defined as the log of the number of clear-ups divided by the number of offences in the borough in the same month and previous quarter. The dependent variable in Columns 4 and 8 is the ratio of clear-ups to arrests, defined as the log of the number of clear-ups divided by the number of arrest in the same month. In Columns 1-4 the offence type relates to cannabis possession. In Columns 5 to 8 the offence type is the sum of all offences related to cannabis supply including: possession with intent, possession on a ship, production, supply, unlawful export, unlawful import, carrying on a ship, inciting others to supply, manufacture, and money laundering. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total London population that month-year in the borough. The policy period dummy variable is equal to one from July 2001 until July 2002, and zero otherwise. The post-policy period dummy variable is equal to one from July 2002 onwards, and zero otherwise. The following socio-demographic control variables, measured in logs, are controlled for at the borough-month-year level: the share of the adult population that is ethnic minority, that is aged 20 to 24, 25 to 34, 35 to 49, and aged above 50, and the male unemployment rate. In addition, the log of the adult population is included as a control in Columns 2 to 4 and Columns 6 to 8.

Table 4: The Effect of the Depenalization on the Demand and Supply of Class-A Drugs Related Crime

Crime Series:	Class-A Drugs Possession (Demand)				Class-A Drugs Supply			
Offence Type:	(1) Offences	(2) Arrests	(3) Clear-ups	(4) Clear-ups per Arrest	(5) Offences	(6) Arrests	(7) Clear-ups	(8) Clear-ups per Arrest
Lambeth x Policy Period [1-6 months]	-.236**	-.114	-.059	.034	-.343	-.380	-.335	.028
	(.115)	(.155)	(.149)	(.024)	(.340)	(.347)	(.389)	(.110)
Lambeth x Policy Period [7-13 months]	.081	-.070	-.098	-.026	-.330	.188	.210	.031
	(.109)	(.144)	(.138)	(.023)	(.303)	(.320)	(.362)	(.102)
Policy Period	-.036	-.118	-.107	.007	.292***	-.077	-.061	.013
	(.043)	(.080)	(.081)	(.007)	(.081)	(.105)	(.107)	(.018)
Lambeth x Post-Policy Period	.120*	-.032	-.028	-.001	-.316**	-.088	.019	.123**
	(.070)	(.080)	(.076)	(.013)	(.146)	(.137)	(.155)	(.059)
Post-Policy Period	.005	-.040	-.015	.020***	.241***	-.096	-.078	-.003
	(.035)	(.058)	(.058)	(.006)	(.067)	(.083)	(.088)	(.015)
Borough and Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2950	2944	2943	3005	2558	2543	2517	2978

Note: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. The sample period runs from April 1998 until January 2006. Control boroughs are all other London boroughs. Class-A drugs are cocaine, crack, crystal-meth, Heroin, LSD, MDMA and methadone. The dependent variable in Columns 1 and 5 is the log of the number of offences for each offence type, per 1000 of the adult population. The dependent variable in Columns 2 and 6 is the arrest rate for each offence type, defined as the log of the number of arrests divided by the number of offences in the borough in the same month and previous quarter. The dependent variable in Columns 3 and 7 is the clear-up rate for each offence type, defined as the log of the number of clear-ups divided by the number of offences in the borough in the same month and previous quarter. The dependent variable in Columns 4 and 8 is the ratio of clear-ups to arrests, defined as the log of the number of clear-ups divided by the number of arrest in the same month. In Columns 1 to 4 the offence type relates to possession of Class-A drugs. In Columns 5 to 8 the offence type is the sum of all offences related to Class-A drugs supply including: possession with intent, possession on a ship, production, supply, unlawful export, unlawful import, carrying on a ship, inciting others to supply, manufacture, and money laundering. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total London population that month-year in the borough. The policy period dummy variable is equal to one from July 2001 until July 2002, and zero otherwise. The post-policy period dummy variable is equal to one from July 2002 onwards, and zero otherwise. The following socio-demographic control variables, measured in logs, are controlled for at the borough-month-year level: the share of the adult population that is ethnic minority, that is aged 20 to 24, 25 to 34, 35 to 49, and aged above 50, and the male unemployment rate. In addition, the log of the adult population is included as a control in Columns 2 to 4 and Columns 6 to 8.

Table 5: The Effect of Depenalizing Cannabis on Non-Drug Related Crime

Dependent Variable: Log (recorded offences of a given type, per 1000 of adult population)

Crime Type:	(1) Total (without drugs)	(2) Violence Against the Person	(3) Sexual	(4) Robbery	(5) Burglary	(6) Theft and Handling	(7) Fraud or Forgery	(8) Criminal Damage
Lambeth x Policy Period	.023	.010	-.112	-.053	-.007	.064*	-.257*	-.046
	(.033)	(.038)	(.084)	(.096)	(.060)	(.037)	(.141)	(.053)
Policy Period	.033	.077***	.100***	.223***	-.012	.049**	-.031	-.012
	(.020)	(.027)	(.025)	(.053)	(.021)	(.021)	(.065)	(.020)
Lambeth x Post-Policy Period	-.094***	-.046	-.096	-.321***	-.250***	-.083**	-.355***	-.090**
	(.033)	(.034)	(.060)	(.093)	(.049)	(.033)	(.128)	(.044)
Post-Policy Period	.024	.200***	.110***	.228***	-.113***	.039**	-.183***	-.064***
	(.018)	(.024)	(.020)	(.046)	(.017)	(.018)	(.055)	(.018)
Share of All Offences Pre-policy	.973	.155	.009	.034	.128	.401	.089	.159
Borough and Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3008	3008	3008	3008	3008	3008	3008	3008

Note: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. The sample period runs from April 1998 until January 2006. Control boroughs are all other London boroughs. In Column 1 the dependent variable is the log of the number of all non-drugs related crime per 1000 of the adult population. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total London population that month-year in the borough. The policy period dummy variable is equal to one from July 2001 until July 2002, and zero otherwise. The post-policy period dummy variable is equal to one from July 2002 onwards, and zero otherwise. The following socio-demographic control variables, measured in logs, are controlled for at the borough-month-year level: the share of the adult population that is ethnic minority, that is aged 20 to 24, 25 to 34, 35 to 49, and aged above 50, and the male unemployment rate. At the foot of the table we show the proportion of all criminal offences (drug and non-drug related) that each category makes up in the pre-policy period in Lambeth from April 1998 until June 2001.

Table 6: The Effect of Depenalizing Cannabis on House Prices

Dependent Variable: Log (zip code-quarter mean house price, deflated to 1995 Q1 prices)

	(1) Baseline	(2) Time Trends	(3) Ex Post Hotspot	(4) Ex Ante Hotspot	(5) Higher Level Clustering
Lambeth x Policy Period	.026**	-.028	.022	-.021	.022
	(.013)	(.019)	(.037)	(.021)	(.016)
Policy Period	.004	-.025***	-.054***	-.036**	-.054***
	(.006)	(.006)	(.011)	(.014)	(.013)
Lambeth x Post-Policy Period	-.050***	-.126***	-.016	-.011	-.016
	(.016)	(.034)	(.030)	(.031)	(.029)
Post-Policy Period	.033***	-.046***	-.111***	-.108***	-.111***
	(.010)	(.011)	(.015)	(.017)	(.028)
Lambeth x Hotspot			-.087**	-.084*	-.087**
			(.044)	(.046)	(.039)
Hotspot			.039	-.211***	.039
			(.024)	(.019)	(.026)
Lambeth x Policy Period x Hotspot			-.062*	-.009	-.062***
			(.036)	(.021)	(.012)
Policy Period x Hotspot			.033***	.012	.033**
			(.011)	(.015)	(.012)
Lambeth x Post-Policy Period x Hotspot			-.134***	-.135***	-.134***
			(.022)	(.020)	(.021)
Post-Policy Period x Hotspot			.073***	.066***	.073***
			(.014)	(.016)	(.021)
Zip code and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes
Borough-Specific Linear Time Trend	No	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes
Observations	17331	17331	17331	17331	17331

Note: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the zip code-sector-quarter-year level. House prices are deflated to the first quarter of 1995 prices, using the Land Registry house price index for Greater London, which is based on repeat sales. More information on the index can be found at http://www1.landregistry.gov.uk/houseprices/housepriceindex/. For all specifications, the sample runs from January 1995 until December 2005, and observations are weighted by the numbers of sales for terraced housing in that quarter-year in the specific zip code-sector. Standard errors are clustered by zip code sector in Columns 1 to 4, and by borough in Column 5. To reflect the lag between the house buying decision and the recorded sale of the house, all time-vary explanatory variables are lagged by one quarter. The (one quarter lagged) policy period dummy variable is equal to one from the fourth quarter (starts October 1) of 2001 until the third quarter of 2002 (ends September 30), and zero otherwise. The (one quarter lagged) post-policy period dummy variable is equal to one from the fourth quarter of 2002 onwards, and zero otherwise. The following socio-demographic control variables, measured in logs, are controlled for at the borough-month-year level: the share of the adult population that is ethnic minority, that is aged 20 to 24, 25 to 34, 35 to 49, and aged above 50, and the male unemployment rate. All of these socio-economic variables are lagged one quarter. We also control for fixed effects for zip code and quarter throughout. In Column 2 onwards we also control for a borough specific linear time trend. In Columns 3 and 5 zip code sectors are defined to be hotpots based on *ex post* ward level crime data. In Column 4 we use *ex ante* ward level crime data.

Table 7: Implied Loss in House Prices due to the Depenalization Policy

Dependent Variable: Log (zip code-quarter mean house price, deflated to 1995 Q1 prices)

Housing Type:	(1) Detached	(2) Semi-Detached	(3) Terraced	(4) Flats	
Lambeth x Policy Period	-.244***	-.028	-.028	-.018	
	(.087)	(.031)	(.019)	(.018)	
Policy Period	-.017	-.030***	-.025***	-.024***	
	(.026)	(.008)	(.006)	(.006)	
Lambeth x Post-Policy Period [β_3]	-.070	-.118***	-.126***	-.099***	
	(.121)	(.041)	(.034)	(.031)	
Post-Policy Period	-.087***	-.050***	-.046***	-.089***	
	(.033)	(.011)	(.011)	(.009)	
A. Mean Pre-Policy House Price (deflated to 1995 Q1 Prices)	£201,653	£140,697	£122,691	£70,208	
B. Median Pre-Policy House Price (deflated to 1995 Q1 Prices)	£185,792	£118,086	£110,311	£62,487	Row Total
Lower Bound Estimate: Assume Unsold Houses Experience No Loss in Value					
C. Post-Policy Sales Total	51	1200	5796	17707	24754
D. Mean Loss Based on Post-Policy Sales Total = β_3 x A x C	-£719,903	-£19,922,653	-£89,600,527	-£123,073,484	-£233,316,567
Upper Bound Estimate: Assume All Households Experience Same Loss in Value					
E. Number of Households in Lambeth in 2001	UNKNOWN	UNKNOWN	UNKNOWN	UNKNOWN	119000
F. Housing Type Share of Post-Policy Sales Total	0.002	0.048	0.234	0.715	
G: Mean Loss Based on Post-Policy Total = β_3 x A x E x F	-£3,460,791	-£95,774,246	-£430,736,962	-£591,651,636	-£1,121,623,634

Note: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the zip code-sector-quarter-year level. House prices are deflated to the first quarter of 1995 prices, using the Land Registry house price index for Greater London, which is based on repeat sales. More information on the index can be found at http://www1.landregistry.gov.uk/houseprices/housepriceindex/. For all specifications, the sample runs from January 1995 until December 2005, standard errors are clustered by zip code, and observations are weighted by the numbers of sales for the housing type in that quarter-year in the specific zip code-sector. To reflect the lag between the house buying decision and the recorded sale of the house, all time-vary explanatory variables are lagged by one quarter. The (one quarter lagged) policy period dummy variable is equal to one from the fourth quarter (starts October 1) of 2001 until the third quarter of 2002 (ends September 30), and zero otherwise. The (one quarter lagged) post-policy period dummy variable is equal to one from the fourth quarter of 2002 onwards, and zero otherwise. The following socio-demographic control variables, measured in logs, are controlled for at the borough-month-year level: the share of the adult population that is ethnic minority, that is aged 20 to 24, 25 to 34, 35 to 49, and aged above 50, and the male unemployment rate. All of these socio-economic variables are lagged one quarter. When calculating the higher house price estimates (row E down), we do not know the number of household in Lambeth for each property type. Iin 2001, there were 119000 households (source: https://www.gov.uk/government/statistical-data-sets/live-tables-on-household-projections). We then estimate the number of each type of houses, using the sales shares from the post-policy period multiplied by the total number of owned houses in Lambeth.

Table 8: Fit of the Structural Model

		Pre-Policy Period (April 1998 - June 2001)		Post-Policy Period (January 2002 to March 2004)		
		(1) Lambeth	(2) Rest of London	(3) Lambeth	(4) Rest of London	(5) Difference in Difference: Lambeth versus Rest of London
A. Matched Moments						
Cannabis Consumption	Observed	.184	.123	.187	.123	
	Predicted	.176	.135	.197	.117	
Cannabis Crime Offence Rate	Observed	.366	.284	.825	.335	64.8%
	Predicted	.366	.288	.820	.332	66.4%
Non-drug Crime Offence Rate	Observed	18.9	14.1	18.2	14.6	-7.26%
	Predicted	18.2	14.9	18.6	16.0	-4.95%
Non-drug Crime Arrest Rate	Observed	2.30	2.40	2.04	1.96	
	Predicted	2.02	1.88	2.00	1.96	
B. Drug Tourism						
Share of Cannabis Offenders in Lambeth from the Rest of London	Observed	.39				
	Predicted	.39		.60		
C. Detection Probabilities						
Cannabis Crime	Predicted	.00127	.0022	.0017	.0031	-5.13%
Non-drug Crime	Predicted	.111	.126	.107	.122	-.444%

Note: Offences and arrests are expressed per 1000 inhabitants. The difference-in-difference in percentages reported in Column 5 is calculated as ((Col 3 - Col 1) - (Col 4 - Col 2)) for each observed and predicted moment, where each value is first logged. The data on offences and arrests are taken from the administrative crime records from the MPS. Data on cannabis consumption are derived from the British Crime Survey (that has borough identifiers) and from data on recorded offences.

Table 9: Predicted Impacts of Citywide Depenalization

	(1) Lambeth	(2) Rest of London
A. Cannabis and Crime		
Cannabis Consumption	1%	2%
Cannabis Crime Offence Rate	-7.4%	-4.0%
Non-drug Crime Offence Rate	-.3%	-.3%
Non-drug Crime Arrest Rate	0%	-.1%
B. Drug Tourism		
Share of Cannabis Offenders in Lambeth from the Rest of London	-4%	
C. Detection Probabilities		
Cannabis Crime	-6.9%	-7.4%
Non-drug Crime	.20%	.22%

Note: Offences and arrests are expressed per 1000 inhabitants.

Table A1: Coding Police Operations

Information Source	Operation Name	Borough	Start	End	Focus	URL	Other Links
A. Borough Specific Police Operations, Complete Information on Start and End Dates							
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Recover	Greenwich, Lewisham, Southwark, Bromley, Croydon	10/2005	17/12/2007	Recovery of Abandoned Stolen	http://www.mpa.gov.uk/committees/mpa/2007/071220/10/	http://www.mpa.gov.uk/committees/x-f/2008/080221/11/
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Blunt	Lambeth, Southwark, Hackney, Newham, Haringey, Tower Hamlets, Brent, Croydon, Waltham Forest, Lewisham, Enfield, Hammersmith and Fulham	11/2004	11/2005	Knife Crime	http://www.mpa.gov.uk/committees/mpa/2005/050526/10/	http://cms.met.police.uk/news/major_operational_announcements/we_launch_the_next_phase_of_operation_blunt
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Safer Streets	Lambeth, Westminster, Southwark, Hackney, Haringey, Camden, Tower Hamlets, Brent, Islington	04/02/2002	31/03/2002	Street Crime	http://www.mpa.gov.uk/committees/mpa/2002/020523/11/	
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Safer Streets Phase 2	Lambeth, Westminster, Southwark, Hackney, Haringey, Camden, Tower Hamlets, Brent, Islington, Newham, Ealing, Waltham Forest, Lewisham, Wandsworth, Croydon	15/04/2002	31/03/2003	Street Crime	http://www.mpa.gov.uk/committees/mpa/2002/020523/11/	
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Strongbox-Windmill	Lambeth	08/05/1999	02/07/1999		http://www.mpa.gov.uk/committees/mpa/2002/020523/10/	http://www.mpa.gov.uk/committees/mpa/2001/010208/07/
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Strongbox-Empire	Hackney	17/07/1999	10/09/1999		http://www.mpa.gov.uk/committees/mpa/2002/020523/10/	http://www.mpa.gov.uk/committees/mpa/2001/010208/07/
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Strongbox-Regis	Camden, Islington	02/10/1999	03/12/1999		http://www.mpa.gov.uk/committees/mpa/2002/020523/10/	http://www.mpa.gov.uk/committees/mpa/2001/010208/07/
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Strongbox-Victory	Westminster	22/01/2001	18/03/2001	Volume Crime: Burglary, Robbery, Vehicle Crime, Drugs	http://www.mpa.gov.uk/committees/mpa/2002/020523/10/	http://www.mpa.gov.uk/committees/mpa/2001/010208/07/
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Strongbox-Castille	Haringey	17/04/2001	10/06/2001		http://www.mpa.gov.uk/committees/mpa/2002/020523/10/	http://www.mpa.gov.uk/committees/mpa/2001/010208/07/ http://www.mpa.gov.uk/committees/mpa/2001/010726/08/
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Strongbox-Claymoor	Brent	16/07/2001	09/09/2001		http://www.mpa.gov.uk/committees/mpa/2002/020523/10/	http://www.mpa.gov.uk/committees/mpa/2001/010208/07/
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Strongbox-Sabre	Tower Hamlets	17/09/2001	09/12/2001		http://www.mpa.gov.uk/committees/mpa/2002/020523/10/	http://www.mpa.gov.uk/committees/mpa/2001/010208/07/
Planning, Performance & Review Committee reports archive http://www.mpa.gov.uk/committees/x-ppr/reports/	Safer Homes	Barnet, Bromley, Croydon, Enfield, Greenwich, Harrow, Hilling don, Hounslow, Lewisham, Redbridge, Southwark, Waltham Forest, Wandsworth	28/10/2002	6/2004	Burglary	http://www.mpa.gov.uk/committees/x-ppr/2003/030313/10/	http://www.mpa.gov.uk/committees/x-ppr/2003/030109/06/
MPA - Annual Reports	Solstice	Brent, Hackney, Westminster, Hammersmith & Fulham, Lewisham, Camden	01/12/2003	08/12/2003	Transport Crime	http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports	Alnwick	Haringey	16/09/2002	13/10/2002	Street Crime	http://www.mpa.gov.uk/downloads/publications/annualrep2002-03.pdf	www.haringeycpcg.org.uk/documents/Police_Report_Nov_2002.doc
Draca et al (2008)	Theseus	Westminster, Camden, Islington, Tower Hamlets, Kensington & Chelsea	7/7/2005	17/08/2005	7/7 Bombings	Draca et al 2008: http://cep.lse.ac.uk/pubs/download/dp0852.pdf	http://www.mpa.gov.uk/committees/x-f/2005/050915/07/
B. Borough Specific Police Operations, Incomplete Information on Start and End Dates							
MPA - Annual Reports	Bantam	Hackney	11/2001	Unknown	Trident-related	http://www.mpa.gov.uk/downloads/publications/annualrep2002-03.pdf	
MPA - Annual Reports	Footbrake	Redbridge	04/2003	03/2004	Vehicle Crime	http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports	Anuric	Kennington			Drug Trafficking	http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports	Dobbi	Enfield			Unlicensed Minicabs	http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports	Michaelmas	Enfield			Street Crime, Burglary	http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports	Garm	Tower Hamlets			Robbery	http://www.mpa.gov.uk/downloads/publications/annualrep2004-05.pdf	
MPA - Annual Reports	Lewark	Lewisham, Southwark			Robbery	http://www.mpa.gov.uk/downloads/publications/annualrep2004-05.pdf	
MPA - Annual Reports	Challenger	Lambeth, Southwark, Hackney, Brent, Lewisham, Tower Hamlets			Robbery	http://www.mpa.gov.uk/downloads/publications/annualrep2004-05.pdf	
MPA - Annual Reports	Orion	Hackney			Drugs	http://www.mpa.gov.uk/downloads/publications/annualrep2004-05.pdf	
MPA - Annual Reports	Foist	Hackney, Haringey, Newham			Uninsured Cars	http://www.mpa.gov.uk/downloads/publications/annualrep2006-07.pdf	
Other Sources - ref URL	Alliance	5 boroughs South London	11/2007	Unknown	Gang Crime	http://www.mpa.gov.uk/committees/mpa/2008/080529-agm/06/#h2002	http://ken.3cdn.net/d23b2ee136d273b37d_xrm6bhcgf.pdf
Other Sources - ref URL	Kartel	11 Boroughs		25/02/2008		http://www.mpa.gov.uk/committees/mpa/2008/080529-agm/06/#h2004	
Other Sources - ref URL	Coalmont	Southward, Lambeth, Lewisham			Gun Crime	http://www.mpa.gov.uk/committees/x-eodb/2008/080207/07/	
C. London Wide Police Operations							
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Blunt 2	All London	14/05/2008	Present	Youth Knife Crime	http://www.mpa.gov.uk/committees/mpa/2008/080529-agm/06/	http://police.homeoffice.gov.uk/news-and-events/news/operation-blunt-2?version=1
MPA http://www.mpa.gov.uk/committees/mpa/reports/	Blunt	All London	12/2005	Unknown	Knife Crime	http://www.mpa.gov.uk/committees/mpa/2005/050526/10/	
Planning, Performance & Review Committee reports archive http://www.mpa.gov.uk/committees/x-ppr/reports/	Maxim	All London	24/03/2003	Unknown	Immigration, People Trafficking	http://www.mpa.gov.uk/committees/x-ppr/2006/061109/08/	http://www.mpa.gov.uk/committees/x-ppr/2003/030508/09/, http://www.mpa.gov.uk/committees/x-ppr/2004/040212/11/
Planning, Performance & Review Committee reports archive http://www.mpa.gov.uk/committees/x-ppr/reports/	Safer Homes	All London	25/10/2002	27/10/2002	Burglary	http://www.mpa.gov.uk/committees/x-ppr/2003/030313/10/	
MPA - Annual Reports	Payback	All London	09/2003			http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports		All London			Hate Crime	http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports	Rainbow	All London			Terrorism	http://www.mpa.gov.uk/downloads/publications/annualrep2004-05.pdf	
MPA - Annual Reports	Copernicos	All London			High-valued Property Theft	http://www.mpa.gov.uk/downloads/publications/annualrep2004-05.pdf	
MPA - Annual Reports	Halifax IV	All London	17/01/2005	28/02/2005	Fail to Appear Warrants	http://www.mpa.gov.uk/downloads/publications/annualrep2004-05.pdf	
MPA - Annual Reports	Bluesky	All London			Immigration	http://www.mpa.gov.uk/downloads/publications/annualrep2005-06.pdf	
MPA - Annual Reports	Jigsaw	All London			Sex Offenders	http://www.mpa.gov.uk/downloads/publications/annualrep2005-06.pdf	
MPA - Annual Reports	Anchorage 2	All London			Violent Crime	http://www.mpa.gov.uk/downloads/publications/annualrep2005-06.pdf	
MPA - Annual Reports	Erica	All London			Anti Social Behaviour	http://www.mpa.gov.uk/downloads/publications/annualrep2007-08.pdf	
MPA - Annual Reports	Argon	All London	09/2007	01/2008	Gun Crime in Nightclubs		
Other Sources - ref URL	Curb	All London	06/2007	03/2008	Youth Violence	http://www.mpa.gov.uk/committees/mpa/2008/080529-agm/06/#h2003	
Other Sources - ref URL	Kontiki	All London			Human Trafficking	http://www.mpa.gov.uk/committees/x-ppr/2006/061109/08/	
Other Sources - ref URL	Sterling	All London			Fraud	http://www2.le.ac.uk/ebulletin/news/press-releases/2000-2009/2009/02/nparticle.2009-02-13.8756898007	
Other Sources - ref URL	Evader	All London				http://www.mpa.gov.uk/committees/x-ppr/2003/030109/06/	
D. Police Operations, Incomplete Information							
MPA - Annual Reports	Enver			19/12/2003	Tamil Criminals	http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports	Tullibardine					http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports	Grafton		04/2003		Crime Around Heathrow	http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports	Bright Star				Anti-terror	http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports	Amethyst				Child Sex Abuse	http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf	
MPA - Annual Reports	Nemo				Drugs	http://www.mpa.gov.uk/downloads/publications/annualrep2004-05.pdf	
MPA - Annual Reports	Vanadium				Drugs	http://www.mpa.gov.uk/downloads/publications/annualrep2004-05.pdf	
MPA - Annual Reports	Chicago				Bus Crime	http://www.mpa.gov.uk/downloads/publications/annualrep2006-07.pdf	
MPA - Annual Reports	BusTag				Bus Vandalism	http://www.mpa.gov.uk/downloads/publications/annualrep2006-07.pdf	
MPA - Annual Reports	Overt				Anti-terror	http://www.mpa.gov.uk/downloads/publications/annualrep2006-07.pdf	
MPA - Annual Reports	Overamp				Anti-terror	http://www.mpa.gov.uk/downloads/publications/annualrep2006-07.pdf	
Other Sources - ref URL	Suki						
Other Sources - ref URL	Lateen				Violent Crime	http://www.haringeycpcg.org.uk/documents/CPCG%20police%20report%20April%2008.pdf	

Note: All websites were accessed in September and October 2009.

Table A2: The Effect of the Depenalization on Cannabis Offences in Aggregate
Robustness Checks

Dependent Variable: Log (total recorded cannabis offences, per 1000 of adult population)

	(1) Neighbors Excluded as Control Boroughs	(2) Year Fixed Effects	(3) Data Regime Fixed Effects	(4) Police Operation Controls	(5) Spatially Correlated Errors
Lambeth x Policy Period	.298**	.335***	.349***	.259**	.151***
	(.117)	(.105)	(.103)	(.112)	(.028)
Policy Period	.038	-.008	.066	.019	.001
	(.056)	(.065)	(.055)	(.053)	(.008)
Lambeth x Post-Policy Period	.606***	.623***	.636***	.555***	.253***
	(.095)	(.082)	(.080)	(.091)	(.020)
Post-Policy Period	.185***	.034	.072	.179***	.052***
	(.047)	(.094)	(.081)	(.046)	(.006)
Borough, Month Fixed Effects	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes
Observations	2632	3008	3008	3008	3008

Note: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. The sample period runs from April 1998 until January 2006. For all Columns except Column 1, control boroughs are all other London boroughs. In Column Lambeth's neighbors (Croydon, Merton, Southwark and Wandsworth) are excluded as controls. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. The exception is Column 5 where a spatial error model is estimated. The spatial weighting matrix used here is a contiguity matrix; all neighbors are allocated ones, and all non-neighbors are allocated zeroes. We also experimented with several other spatial weighting matrices, including inverse distance (between borough centroids) and inverse distance squared weighting matrices. The results are robust to these different spatial error specifications. Observations are weighted by the share of the total London population that month-year in the borough. The exception again are Columns 1 and 5. In Column 1 observations are weighted by the share of the (non-neighboring borough) total London population that month-year in the borough. In Column 5 observations are not weighted. The policy period dummy variable is equal to one from July 2001 until July 2002, and zero otherwise. The post-policy period dummy variable is equal to one from July 2002 onwards, and zero otherwise. The following socio-demographic control variables, measured in logs, are controlled for at the borough-month-year level: the share of the adult population that is ethnic minority, that is aged 20 to 24, 25 to 34, 35 to 49, and aged above 50, and the male unemployment rate. Data regime fixed effects allow for any changes in the recording of the data in each of these separate time periods, as well as a dummy for the change in crime recording rules from April 2002 onwards. The police operation controls variables are indicators for whether the borough was part of a recent Police Operation. Operations that targeted a group of specific boroughs include the Safer Streets Initiative Phase 1 (04/02/2002 – 31/03/2002) and Phase 2 (15/04/2002 – 31/03/2003), Operation Recover (10/2005-17/12/2007), Operation Blunt 1 (11/2004-11/2005),Operation Safer Homes (28/10/2002-06/2004) and Operation Solstice (01/12/2003-08/12/2003). Lambeth was part of Safer Streets Phase 1 and 2, and Blunt 1. Further operations (past of a larger operation named Strongbox) that targeted single boroughs include Operation Windmill (Lambeth: 08/05/1999-02/07/1999), Operation Empire (Hackney: 17/07/1999-10/09/1999), Operation Regis (Camden, Islington: 02/10/1999-03/12/1999), Operation Victory (Westminster: 22/01/2001-18/03/2001), Operation Castille (Haringey: 17/04/2001-10/06/2001), Operation Claymoor (Brent: 16/07/2001-09/09/2001) and Operation Sabre (Tower Hamlets: 17/09/2001-09/12/2001).

Table A3: The Effect of the Depenalization on Police Effort on Non-Drug Crime

A. Dependent Variable: Log (arrest rate for a given crime category)

Crime Type:	(1) Total (without drugs)	(2) Violence Against the Person	(3) Sexual	(4) Robbery	(5) Burglary	(6) Theft and Handling	(7) Fraud or Forgery	(8) Criminal Damage
Lambeth x Policy Period	.065	.096	.158	.383***	-.197	-.152*	.058	.024
	(.108)	(.128)	(.182)	(.142)	(.142)	(.090)	(.160)	(.158)
Policy Period	-.101*	-.178**	-.164***	-.242***	.128***	-.173***	-.154*	-.168***
	(.058)	(.087)	(.054)	(.054)	(.049)	(.044)	(.080)	(.062)
Lambeth x Post-Policy Period	.284***	.344***	.454***	.417***	.325***	-.062	.567***	.299**
	(.105)	(.124)	(.132)	(.106)	(.105)	(.072)	(.121)	(.130)
Post-Policy Period	-.015	-.076	-.114***	-.112***	.185***	-.209***	-.056	-.033
	(.048)	(.072)	(.043)	(.043)	(.039)	(.035)	(.062)	(.048)
Share of All Arrests Pre-policy	.861	.281	.016	.034	.086	.297	.049	.098
Borough, Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3008	3008	2936	2986	3008	3008	3006	3008

B. Dependent Variable: Log (clear-up rate for a given crime category)

Crime Type:	(1) Total (without drugs)	(2) Violence Against the Person	(3) Sexual	(4) Robbery	(5) Burglary	(6) Theft and Handling	(7) Fraud or Forgery	(8) Criminal Damage
Lambeth x Policy Period	.028	.066	.161	.317**	-.192	-.119	.063	.131
	(.112)	(.129)	(.179)	(.145)	(.146)	(.090)	(.274)	(.159)
Policy Period	-.073	-.159*	-.169***	-.176***	.154***	-.154***	.001	-.157**
	(.062)	(.088)	(.053)	(.054)	(.048)	(.045)	(.046)	(.063)
Lambeth x Post-Policy Period	.270**	.319**	.484***	.436***	.314***	-.077	.554***	.305**
	(.115)	(.128)	(.131)	(.109)	(.109)	(.072)	(.194)	(.134)
Post-Policy Period	.067	-.022	-.094**	-.039	.242***	-.145***	.396***	.026
	(.052)	(.073)	(.042)	(.042)	(.038)	(.036)	(.041)	(.049)
Share of All Clear-ups Pre-policy	.846	.311	.019	.029	.084	.293	.007	.104
Borough, Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3008	3008	2934	2980	3007	3008	2630	3008

C. Dependent Variable: Log (clear-up per arrest)

Crime Type:	(1) Total (without drugs)	(2) Violence Against the Person	(3) Sexual	(4) Robbery	(5) Burglary	(6) Theft and Handling	(7) Fraud or Forgery	(8) Criminal Damage
Lambeth x Policy Period	.018	.021*	-.006	-.037	.012	.030**	-.027	.057*
	(.014)	(.012)	(.038)	(.070)	(.039)	(.015)	(.145)	(.031)
Policy Period	.023***	.022***	.002	.072***	.029**	.018***	.194***	.027***
	(.008)	(.006)	(.012)	(.021)	(.015)	(.007)	(.054)	(.008)
Lambeth x Post-Policy Period	.010	.006	.015	.014	-.014	-.020*	-.019	.025
	(.010)	(.009)	(.028)	(.051)	(.028)	(.011)	(.099)	(.022)
Post-Policy Period	.081***	.066***	.030***	.088***	.056***	.064***	.465***	.061***
	(.006)	(.005)	(.010)	(.017)	(.012)	(.005)	(.039)	(.007)
Share of All Clear-ups Pre-policy	.846	.311	.019	.029	.084	.293	.007	.104
Borough, Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3008	3008	3002	3002	3007	3008	2632	3008

Note: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. The sample period runs from April 1998 until January 2006. Control boroughs are all other London boroughs. In Panel A the dependent variable is the log of the number of arrests divided by the number of offences in the borough in the same month and previous quarter, for each crime type. In Panel B the dependent variable is the log of the number of clear-ups divided by the number of offences in the borough in the same month and previous quarter, for each crime type. In Panel C the dependent variable is the log of the number of clear-ups divided by the number of arrests in the borough, in the given month. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total London population that month-year in the borough. The policy period dummy variable is equal to one from July 2001 until July 2002, and zero otherwise. The post-policy period dummy variable is equal to one from July 2002 onwards, and zero otherwise. The following socio-demographic control variables, measured in logs, are controlled for at the borough-month-year level: the share of the adult population that is ethnic minority, that is aged 20 to 24, 25 to 34, 35 to 49, and aged above 50, and the male unemployment rate. At the foot of each panel we show the proportion of all arrests and clear-ups (drug and non-drug related) that each category makes up in the pre-policy period in Lambeth from April 1998 until June 2001.

Table A4: Robustness Checks on the Effect of Depenalizing Cannabis on House Prices

Dependent Variable: Log (zip code-quarter mean house price, deflated to 1995 Q1 prices)

	(1) Flats	(2) Ward Hotspot Definition: 75th PC	(3) Zip Code Sector Hotspot Definition: Modal Ward	(4) Zip Code Sector Hotspot Definition: Weighted Average of Wards	(5) Hotspots Based on Total Crime
Lambeth x Policy Period	.011	-.013	-.032	-.027	.010
	(.022)	(.023)	(.024)	(.027)	(.020)
Policy Period	-.050***	-.044***	-.033***	-.041***	-.057***
	(.015)	(.007)	(.008)	(.008)	(.011)
Lambeth x Post-Policy Period	.070**	-.083**	-.099***	-.064	-.010
	(.027)	(.041)	(.038)	(.039)	(.028)
Post-Policy Period	-.155***	-.079***	-.068***	-.091***	-.107***
	(.016)	(.012)	(.012)	-0.013	(.012)
Lambeth x Hotspot	.006	-.056*	-.126***	-.296***	-.086*
	(.039)	(.029)	(.027)	(.081)	(.044)
Hotspot	-.058**	-.091*	-.045**	-.001	-.001
	(.027)	(.054)	(.018)	(.212)	(.014)
Lambeth x Policy Period x Hotspot	-.033	-.017	.011	-.002	-.044**
	(.030)	(.025)	(.023)	(.028)	(.018)
Policy Period x Hotspot	.031**	.031***	.016**	.031***	.035***
	(.015)	(.008)	(.008)	(.010)	(.012)
Lambeth x Post-Policy Period x Hotspot	-.199***	-.070***	-.077***	-.139***	-.133***
	(.030)	(.025)	(.022)	(.026)	(.018)
Post-Policy Period x Hotspot	.080***	.051***	.043***	.085***	.066***
	(.016)	(.010)	(.011)	(.013)	(.011)
Zip code and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes
Borough-Specific Linear Time Trend	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes
Observations	20706	17331	17331	17331	17331

Note: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the zip code-sector-quarter-year level. House prices are deflated to the first quarter of 1995 prices, using the Land Registry house price index for Greater London, which is based on repeat sales. More information on the index can be found at http://www1.landregistry.gov.uk/houseprices/housepriceindex/. For all specifications, the sample runs from January 1995 until December 2005. In Column 1 (2 to 5) observations are weighted by the numbers of sales for flats (terraced housing) in that quarter-year in the specific zip code-sector. Standard errors are clustered by zip code sector throughout. To reflect the lag between the house buying decision and the recorded sale of the house, all time-vary explanatory variables are lagged by one quarter. The (one quarter lagged) policy period dummy variable is equal to one from the fourth quarter (starts October 1) of 2001 until the third quarter of 2002 (ends September 30), and zero otherwise. The (one quarter lagged) post-policy period dummy variable is equal to one from the fourth quarter of 2002 onwards, and zero otherwise. The following socio-demographic control variables, measured in logs, are controlled for at the borough-month-year level: the share of the adult population that is ethnic minority, that is aged between 20 to 24, aged between 25 to 34, aged between 35 to 49, aged above 50, and the male unemployment rate. All of these socio-economic variables are lagged one quarter. We also control for fixed effects for zip code and quarter throughout, and a borough specific linear time trend. In Column 2 we define a ward as a hotspot if drug offences are above the 75th percentile median for all wards in the borough. We then define a zip code to be a hotspot if it contains any hotspot wards so defined. In Column 3 the zip code sector is defined to be a hot spot if the modal ward is itself defined to be a hotspot. In Column 4 the hotspot variable is no longer binary, but rather a weighted average of all wards' hotspot classifications within the zip code sector. These weights are based on the percentage of the zip code that overlaps with the ward. Finally, Column 5 uses information on total crimes (not drug crime) to redefine wards and then zip codes as hotspots using otherwise the same method as the baseline specification.

Table A5: Calibrated Parameters

	Notation	Location	Calibrated Parameter
A. Direct Depenalization Policy Channels			
Penalty Reduction During Policy	α_{policy}	Lambeth	.178
Reduction in Police Hours During Policy for Cannabis Crime	ρ_1	Lambeth	.530
B. Preference Parameters			
Disutility of Cannabis Consumption	$\bar{\delta}_1$	Lambeth	.799
	$\bar{\delta}_0$	Rest of London	.823
Disutility of Committing Non-drug Crime	$\bar{\chi}_1$	Lambeth	.956
	$\bar{\chi}_0$	Rest of London	.955
Maximum Mobility Cost	$\bar{\lambda}$	All	.753
C. Policing Technology			
Apprehension Technology for Cannabis Crime	$\gamma_{D,1}$	Lambeth	.0127
	$\gamma_{D,0}$	Rest of London	.0191
Apprehension Technology for Non-drug Crime	γ_C	All	.218
Cobb Douglas Parameter, Cannabis Crime Arrests	ω_D	All	.270
Cobb Douglas Parameter, Non-drug Crime Arrests	ω_C	All	.356
D. Other			
Penalty of Arrest, Cannabis Crime	α_1	All	21
Penalty of Arrest, Non-drug Crime	β	All	0.229

Figure 1A: Aggregate Cannabis Offences

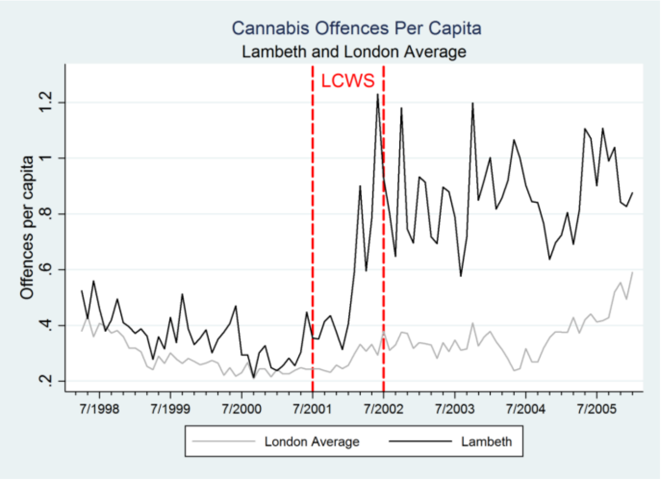


Figure 1B: Cannabis Possession Offences

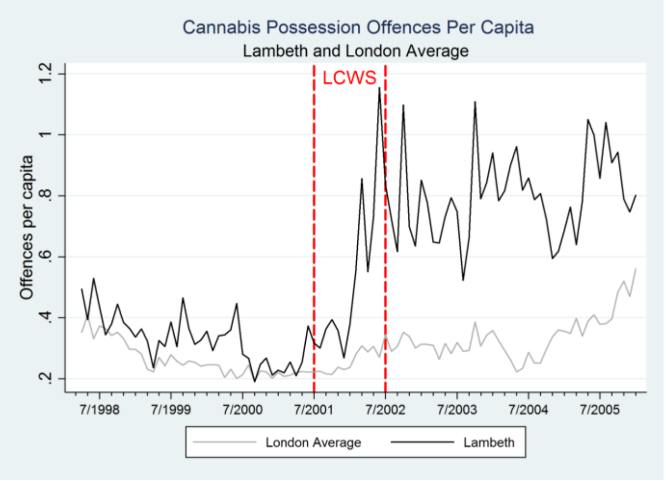
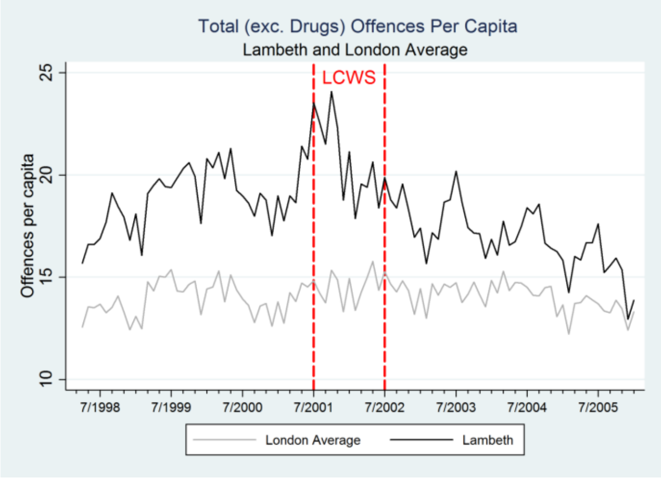
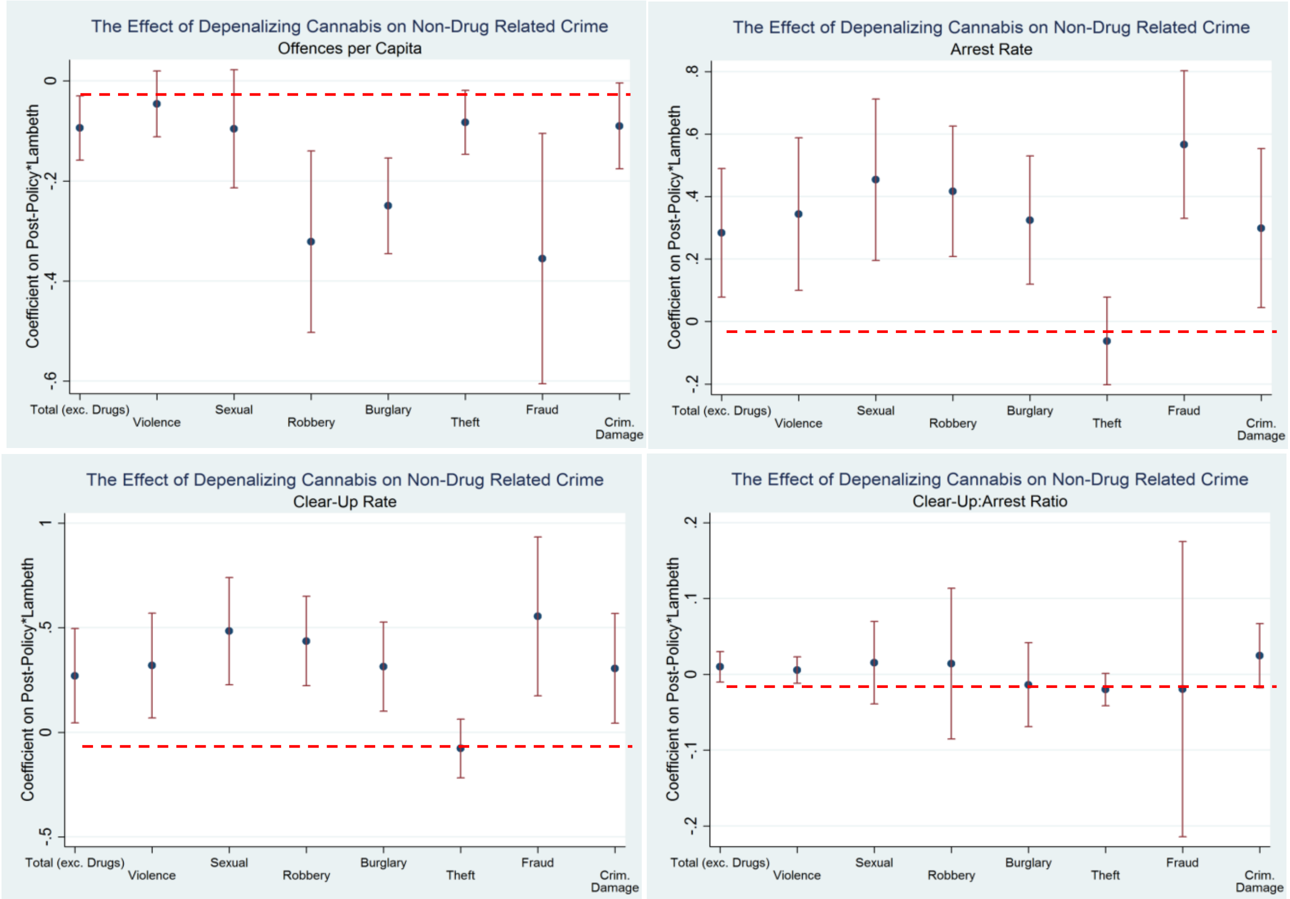


Figure 1C: Aggregate Non-Drug Offences



Note : The sample period runs from April 1998 until January 2006. The two red vertical lines represent the start and end of the Lambeth policy (July 2001 and July 2002 respectively). In each Figure, the black time series represents the relevant time series for Lambeth. The grey series represents the mean offences per capita for the rest of London, Figure 1A shows the time series for the number of cannabis related offences in aggregate, per 1000 of the adult population. Figure 1B shows the time series for the number of cannabis possession offences, per 1000 of the adult population. Figure 1C shows the time series of the number of non-drug offences, per 1000 of the adult population. Non-drug offences include those for violence against the person, sexual offences, robbery, burglary, theft and handling, fraud or forgery, and criminal damage.

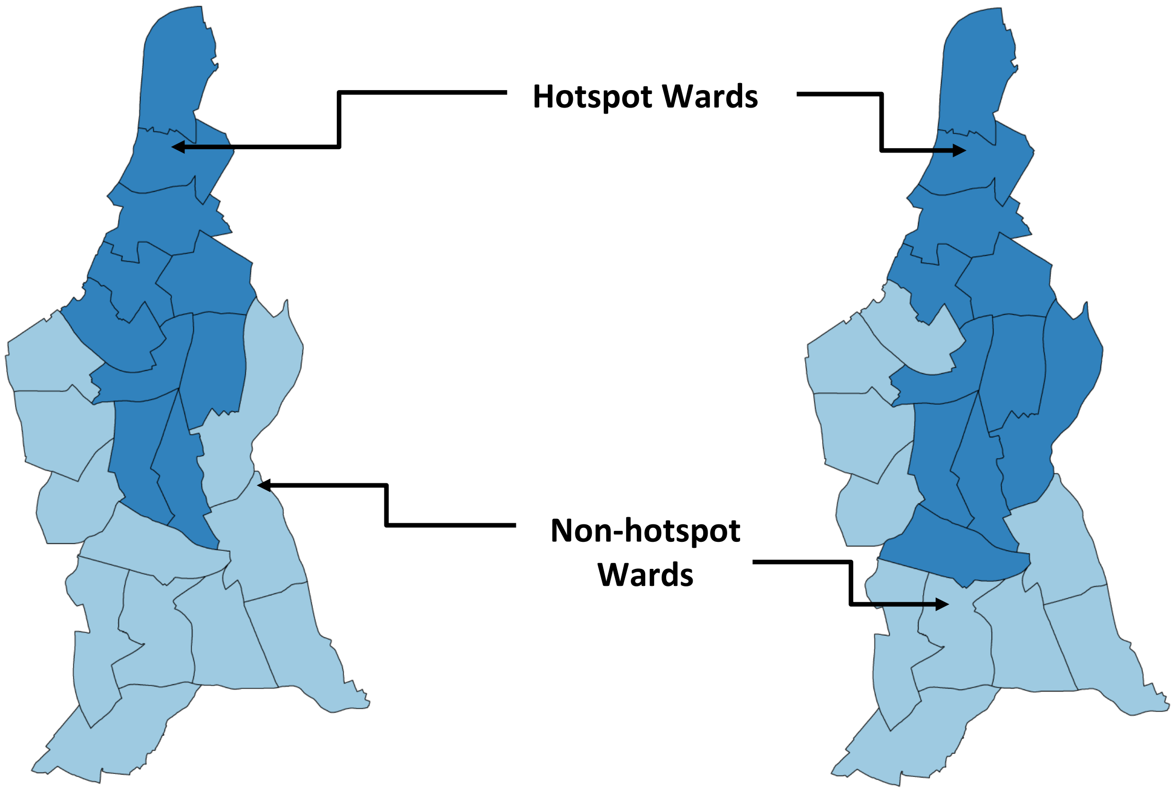
Figure 2: Impacts of the Depenalization Policy on Non-Drug Crimes



Note: Each point on the graph above represents the point estimate on the Post-Policy*Lambeth interaction term from a separate regression. The lines represent 95% confidence intervals. The point estimates are from regressions as described as follows. All observations are at the borough-month-year level. The sample period runs from April 1998 until January 2006. Control boroughs are all other London boroughs. The dependent variable in the offence graph is the log of the number of offences for each offence type, per 1000 of the adult population. The dependent variable in the arrest graph is the arrest rate for each offence type, defined as the log of the number of arrests divided by the number of offences in the borough in the same month and previous quarter. The dependent variable the clearance graph is the clearance rate for each offence type, defined as the log of the number of clearances divided by the number of offences in the borough in the same month and previous quarter. The dependent variable in the clearance: arrest ratio graph is defined as the log of the number of clear-ups divided by the number of arrest in the same month. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total London population that month-year in the borough. The policy period dummy variable is equal to one from July 2001 until July 2002, and zero otherwise. The post-policy period dummy variable is equal to one from July 2002 onwards, and zero otherwise. The following socio-demographic control variables, measured in logs, are controlled for at the borough-month-year level: the share of the adult population that is ethnic minority, that is aged between 20 to 26, aged between 25 to 34, aged between 35 to 49, aged above 50, and the male unemployment rate. The log of the total borough population (by month-year) aged 16 and over is also included as a control in all except the offence regressions.

Figure A1A: Ex Post Drug Hotspots in Lambeth

Figure A1B: Ex Ante Drug Hotspots in Lambeth



Note: Hotspots are set to one if total drug offences in the ward are equal to or above the median within the borough,. The ex post period runs from October 2007-September 2009. The ex ante period runs from April-June 2001. The darker shaded wards are those that are defined to be a hotspot using the ex post and ex ante data. The lighter shaded wards are those defined to be non-hotspot wards under each definition.

Chapter 4

Conclusion

By considering 9/11 as an exogenous shift to tastes for discrimination in the US, and linking this change in discrimination to changes in ethnic sentencing differentials, the first paper aims to address the crucial issue of the source of racial and ethnic disparities in criminal sentencing outcomes. It is necessary to understand the underlying causes of such disparities in order to best assess what can be done to address the issue; each of the explanations implies a very different policy response. There is still much to do in this work, not least in order to identify the key agents driving the results. In on-going work, I have collected data on previous stages of criminal justice procedure (similar to that of Rehavi and Starr [2012]), in order to link defendants from arrest to sentence. Different criminal justice agents (including the initial hearing judge, district attorneys, sentencing judges and defense counsel) play different roles at each of these stages. Using the same identification strategy, but considering earlier stages, will allow me to better understand the key channels underlying the observed post-9/11 effect

While the model in the second paper highlights important channels by which the depenalization of cannabis impacts the level and composition of crime, there still remain key questions regarding how police and drug suppliers respond to changes in illicit drugs policy. Research on how drugs policies affect the organization of criminal activity remain scarce, not least due to the paucity of data regarding suppliers. Related in part to this issue is a study by Dobkin and Nicosia [2009], which examines the impact of a large government intervention to reduce supply of methamphetamine precursors found large, but short lived impact on both the purity and price of the drug. This gives some insight into the dynamic nature of the supply side. Less is known, however, about how drug suppliers alter both the size and location of their operation(s) as a response to both supply-side interventions and policy changes.

Better understanding of how police across different areas should coordinate activity is also required, particularly where policies are implemented unilaterally across jurisdictions. There is a small body of research, mainly theoretical, considering cross-jurisdiction policing coordination, motivated by the concept of spatial displacement effects of crime [Hesseling, 1994; Gonzalez-Navarro, 2013]. Theoretical work, including that of Marceau [1997], has found that where spatial externalities in crime exist, communities may compete in deterring crime (in order to displace criminals elsewhere), and thus over-invest in policing (Detterring crime also makes communities more attractive to investors).

The spatial displacement effects of crime as a response to differentials in policing are difficult to identify for several reasons. Individuals Tiebout sort over areas, which may be reflected in differential levels of local spending, as well as result in different levels of crime. Second, the relationship between policing levels and crime are endogenous [Levitt, 1997; Draca et al., 2011; Buonanno and Mastrobuoni, 2012], and thus the correlation between policing and crime across different jurisdictions is likely to conflate the true impact of police on crime, as well as spatial displacement of crime resulting from differences in jurisdictional policing strength [Newlon, 2001].

The house price analysis provides evidence of how the policy impacted upon valuations of the Lambeth area during this time. What is noteworthy is that although cannabis drugs crime increased during and after the policy, the area experience a marked decline in the majority of other types of crime post-policy. At the same time house prices fell significantly, particularly in drug hot-spot areas. As noted in the paper, there were likely other factors influencing the documented declines in Lambeth house prices associated with the change in illicit drug use in the area. Of interest to me for future research, however, is the role of crime perceptions in the measured effects. What role do the visibility of different crime types play in when individuals form percetions of crime in an area, and how does better information on crime (such as publicly available crime mapping websites) impact these perception formations?

A final point to consider is the role played by both dynamic behavior and behavioral biases in how crime is modeled, and studied empirically. The Becker model is static. However, there are various reasons to suspect that current crime may affect future crime (for instance by lowering social capital and thus the perceived cost of crime [Sickles and Williams, 2008], or by increasing criminal capital accumulation through experience), and also that current sanctions may affect future crime (through affecting legal work options [Grogger, 1995], or by exposure to criminals when incarcerated [Bayer et al., 2009]). There is evidence that in the study of the LCWS, the increase in cannabis offenses in Lambeth occurred with a lag, rising only after six months, and then persisted long after the policy had commenced. Recent work has begun to consider such dynamics [Kleiman and Kilmer, 2009; Lee and McCrary, 2009; McCrary, 2010], but as yet few papers empirically incorporate such dynamics.

Related to the issue of dynamics is the importance of considering the decisions made by individuals engaging in crime. Is rationality a reasonable assumption? One potential avenue of relevance to the dynamic nature of both crime and the related sanctions is the role of time-inconsistent preferences. The benefits of committing a crime are realized in the present, although sanctions are future-based (and uncertain). Work by Mark Kleiman [Kleiman, 2009] on changing the nature of sanctions to be more certain and more instant (as well as less severe in nature) speaks to this issue. Other authors have also suggested the role played by individuals' perception of the criminal justice system (notably, the "legitimacy" of the system), and how this shapes the criminal participation decision. If individuals view the criminal justice system as illegitimate, then these people may be more likely to disregard the law and commit crimes [LaFree, 1998]. A key application of this, and of relevance to the implications of the first paper in this thesis, is the over-representation of ethnic minorities in the criminal justice system, and how this may affect future criminal engagement of such groups. A recent paper by Rocque [2011] considers precisely this issue from a theoretical framework. Regarding legitimacy theory, it is worth noting that one of the key justifications for implementing the LCWS was to ease community tensions caused by the criminalizing of individuals in the community for possessing small amounts of cannabis. Such a shift in policy may have played a role in changing local perceptions of the legitimacy of the local police, and thus change to criminal behavior. Developing a better understanding of how such perception impacts criminal participation (not present in the standard Becker model) would enrich our understanding of the causes and the dynamics of crime.

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