CRIME AND THE DECRIMINALIZATION OF CANNABIS: EVIDENCE FROM A LOCALIZED POLICING EXPERIMENT*

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Abstract

We evaluate the effects on crime of a localized policing experiment that decriminalized cannabis possession in the London borough of Lambeth between 2001 and 2002. We find that decriminalization led to a surge in drug related offences, and a collapse in arrest and clear-up rates for drug related crimes in Lambeth. These effects are quantitatively large and persist well after the policy experiment ends. However, the policy does allow the Lambeth police to reallocate their effort towards non-drug related crimes, leading to permanent reductions in nearly all other crime types. We also find the policy to have spillovers onto boroughs neighboring Lambeth. As drug consumers and suppliers relocate to Lambeth after decriminalization, drug crime rates significantly fall in neighbors to Lambeth. To understand the benefits of coordinating drugs policies across jurisdictions, we compare these results to the effects on crime of the nationwide decriminalization of cannabis in the UK from 2004 to 2009. We find that nationwide decriminalization does not lead to a growth in drugs related crime, but does allow the police to reallocate effort towards non-drug related crime. We interpret the results through a Hotelling-style model that makes precise the behavioral response to decriminalization of the police, suppliers and demanders of drugs.

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1 Introduction

In nearly every country the market for illicit drugs remains pervasive, despite long running attempts to restrict such activities. While countries such as Australia, Germany, the Netherlands, Portugal and Spain exhibit liberal attitudes of law enforcement towards cannabis possession – *de facto* or *de jure* decriminalization, the US policy approach, as manifested in the 'war on drugs', emphasizes a punitive approach. The tough policy stance in the US has proved to be expensive, estimated to cost tens of billions of dollars annually, and it remains unclear whether this approach has successfully reduced the size of the illicit drugs market. In 2007, there were over 1.8 million arrests in the US for drug abuse violations, with an estimated 3.7 million individuals regular consumers of illicit drugs, the majority of whom consume cannabis [DHHS 2008].

The need to intervene in the market for illicit drugs is reinforced by specific characteristics of the market: illicit drugs damage user's health; the use of some drugs providing a gateway to more addictive drugs; and the potentially large spillover effects on crime and other forms of anti-social behavior. All these features result in the market generating large private and social costs, as embodied in the criminal justice and health systems [Miron and Zweibel 1995].[1]

In this paper we study the effects of one such market intervention in the UK: the decriminalization of cannabis, so the possession of small quantities of cannabis is no longer a prosecutable offence. We present evidence to evaluate the causal impact of such a decriminalization of cannabis on crime, considered to be a major impact of the illicit drugs market on society.[2] A causal link between crime and drugs occurs for at least three reasons. First, the substance itself could lead to more violent or criminal behavior by users. Second, users might commit crimes to obtain money to buy drugs. Third, violence occurs between drug suppliers to control selling areas. The evidence we present relates to how the decriminalization of cannabis affects crime through the first two channels. Moreover, our analysis makes precise that another channel linking drugs and crime operates through police behavior. We highlight theoretically and empirically that the decriminalization of cannabis might free up police resources to focus on crime related to other illicit drugs, or to non-drug crimes. Such changes in police behavior might reinforce or possibly even reverse the aforementioned direct mechanisms linking drugs and crime.[3]

[1] The causal relationship between the consumption of illicit drugs and harm to the user is controversial [Arseneault *et al* 2004, van Ours and Williams 2009]. For cannabis, harm to users is thought to depend on the relative content of two psycho-active ingredients, Δ^9-tetrahydrocannabinol (THC) and cannabidiol (CBD) [Zuardi *et al* 1982]. There are some evidence on some illicit drugs providing a gateway to other drugs [van Ours 2003]. Estimates on the social costs via the criminal justice and health systems vary from .2% of GDP to Canada to 1.7% in the US in 2002 and 1.8% in the UK in 2000 [UNDC 1998, ONDCP 2004].

[2] Clearly there are many policy interventions that might be relevant [Miron and Zweibel 1995]. As discussed in detail by Pudney [2010], the seminal model of rational addiction [Becker and Murphy 1988] emphasizes users are still sensitive to current and future prices so corrective taxes can curtail consumption. Behavioral models explaining drug use through intertemporal failures and multiple selves [Gruber and Koszegi 2001] emphasize the need for such taxes to correct for internalities as well as externalities of drug consumption; temptation [Gul and Pesendorfer 2001] or cue-conditioned irrationality models [Bernheim and Rangel 2004] emphasize prohibition may be welfare-increasing and taxation distortionary. Irrespective of the underlying rationale for drug use, the law can affect behavior directly – criminal penalties disincentivise consumption and supply among forward looking individuals, and the law might serve an educational purpose by signaling the harms from consumption.

[3] Current evidence links the size of drugs markets to crime rates both at the aggregate level for US counties

The decriminalization policy we evaluate relates to a policy instigated by the local police in one London borough, Lambeth, in July 2001: the Lambeth Cannabis Warning Scheme (LCWS). We describe the motivation behind the policy and its implementation in more detail later. There are however four key policy features worth emphasizing. First, many aspects of the policy reflect how other decriminalization policies have been implemented around the world: (i) the possession of cannabis for personal consumption was decriminalized; (ii) the primary motivation was to free up police time and other resources to focus on crimes related to other drugs or other types of crimes; (iii) the policy did not alter penalties for cannabis supply.

Second, the LCWS was first announced as a temporary policing experiment to only 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 represent a permanent change in policing strategy in the borough of Lambeth. 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 was formally ended at the end of July 2002, having run for 13 months. We exploit the fact the policy was initially announced to be temporary, then announced as permanent, and then switched off altogether, to assess the short and long run effects on crime of the decriminalization policy.

Third, the policy is localized as it is only implemented in Lambeth. Given transportation links and the relatively short distances involved between London boroughs, there might be short and long run effects in adjacent illicit drugs markets. We therefore infer drugs tourism from changes in drugs possession and trafficking offences in geographic neighboring boroughs to Lambeth.

Finally, we compare the effect of a temporary localized decriminalization policy, to a nationwide decriminalization of cannabis initiated from January 2004 to January 2009. With such policies there is no scope for drugs tourism, but there remains scope for police resources to be freed up for other crimes. This comparison is relevant for understanding whether drugs policies ought to be coordinated across jurisdictions.

To guide the empirics and interpret the evidence, we develop a Hotelling-style model of two borough locations. This shows how the link between decriminalization and crime is driven by the behavioral responses of police officers, suppliers of drugs, and consumers of drugs. The framework makes precise the multiple channels through which a decriminalization policy unilaterally implemented in one borough, affects the equilibrium market size and price for drugs, police effort towards drugs and non-drugs crimes, and the numbers of drugs crimes and non-drugs crimes.

[Pacula and Kilmer 2003], and for metropolitan areas in specific relation to the size of the market for crack cocaine [Grogger and Willis 2000]. Fergusson and Horwood [1997] use longitudinal data for a birth cohort of New Zealand children to investigate the relationship between early onset of cannabis and subsequent crime. 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]. There is more robust evidence of links between drug use and property crime [Corman and Mocan 2000]. On drug suppliers crimes, Kuziemko and Levitt [2004] find the effect of 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.

To help structure the empirical analysis, the model emphasizes what these effects will be in the decriminalizing borough, and the potential spillover effects on the other borough.

We combine various data sources to construct a panel data set on crime for each of the 32 London boroughs, for each month since April 1992. We do so for drug related crimes – that is the aggregation of offences for drug possession, trafficking etc. for all drug types (cannabis and non-cannabis); and seven other crime types: violence against the person, sexual offences, robbery, burglary, theft and handling, fraud and forgery, and criminal damage. For each crime type, we construct separate series by borough-month for the number of recorded offences, as well as two measures closely related to police behavior: the number of individuals arrested, and the number of crimes cleared-up by the police.[4]

Our main results are as follows. Focusing first on the within Lambeth effects, we find the LCWS led to a surge in drug related offences, driven predominantly by offences for drugs possession. These increases occur both during the period when the LCWS is formally in place, and in the post-policy period. Closer examination of the data reveals that during the policy period, drugs crime only rises in Lambeth six months into the policy – precisely the time when the policy is announced to be permanent. In the post-policy period, there is no evidence of reversion to the mean in drugs crime in Lambeth suggesting the short run decriminalization policy had permanent effects on Lambeth's illicit drugs market. In contrast, over the same policy and post-policy periods, we find no evidence of London-wide increases in drug crime rates.

Second, examining police behavior in Lambeth, we find significant declines during and after the LCWS's introduction in arrest rates for drug-related offences, and in actual clear-up rates for drug offences. Again, over the same time periods we find no evidence of London-wide trends in rates of arrests or clear-ups for drug related crime. Overall the data suggests police effectiveness appears to have dropped considerably with regards to drugs crime in Lambeth after the LCWS is introduced. Of course, such reduced police effort might itself feedback to drive the higher rates of drug related offences previously documented. In short, with the introduction of the LCWS, the police reallocate their effort away from the illicit drugs market as a whole and this leads to a large and sustained surge in drug offences in Lambeth.

Our third set of results therefore explore whether such an approach is beneficial in that it allows the police in Lambeth to reallocate their effort towards other crime types, as made precise in the theoretical framework. We find that in the post-policy period, total crime in Lambeth falls significantly by 8.4% relative to the rest of London. For nearly all other crime types – violence against the person, sexual offences, robbery, burglary, theft and handling, fraud or forgery, and criminal damage – crime rates fall in Lambeth in the long run after the LCWS is introduced. These crime falls in Lambeth go against the London-wide trend of increases in each type of crime over the same period. Moreover, the Lambeth police become significantly more efficient in tackling other types of crime as measured by increases in arrest rates.

[4]The number of offences is the number of offences recorded by the police. These offences might be reported by the public or the police. For any crime type, there can be more than one individual arrested for any recorded offence, and a clear-up is a crime for which an individual is successfully prosecuted.

Our fourth set of results explore the across borough effects of the LCWS policy. We find that drug crime significantly falls in the geographic neighbors of Lambeth, both during the period when the LCWS is in place, and in the long run. This is driven by falls in offences related to drugs possession and drug trafficking in neighboring boroughs. In line with the model, the results indicate drugs tourism and suppliers locating closer to Lambeth after decriminalization.

Finally, we compare the effects of the localized decriminalization experiment in Lambeth, to the nationwide decriminalization of cannabis in the UK, initiated in January 2004. In line with our theoretical framework, nationwide decriminalization has few effects on either drugs or non-drugs related crime rates, but does allow the police to reallocate their effort towards other crime types as reflected in significant increases in accusations per offence for non-drugs crime.

The paper is organized as follows. Section 2 develops a Hotelling-style model of the market for illicit drugs that pins down the effects of decriminalization on the behavior of the police, suppliers of drugs, and consumers of drugs. Section 3 describes the LCWS, the motivation behind it, its implementation, and reasons for its abandonment. Section 4 details our data sources, presents descriptive evidence on drugs crime in Lambeth and other boroughs over time, and describes our empirical method. Section 5 presents evidence on the effects of the LCWS on drug crime and police efficiency in Lambeth, and presents evidence on the effects of the LCWS on other crime types. Section 6 documents the LCWS's effect on crime in neighboring boroughs, the effects on crime of the nationwide cannabis decriminalization, and uses information on house prices by zip code sector to shed light on how Lambeth residents value the total social effects of decriminalization in the long run, not just those operating through changes in crime. Section 7 concludes. Proofs and robustness checks are in the Appendix.

2 Theoretical Framework

We develop a Hotelling-style model of the drugs market, accounting for utility maximizing drug consumers, profit maximizing drug suppliers, and crime minimizing police. The model highlights the multiple channels through which localized decriminalization policies affect drugs crime, other types of crime, and have potential spillover effects on neighboring boroughs. The framework also allows us to compare the effects of local and nationwide decriminalization policies.

2.1 Consumers

Consumers are uniformly located on a segment of unit length between two boroughs, and face transport costs t per unit. Denoting the distance from A as x, consumers therefore face transport costs of tx to travel to borough A. There are two drug suppliers, each located at an end of the line, taken to be the center of boroughs A and B. Each borough has its own police force. In borough b, the relative price of cannabis is π_b and the intensity of policing drug crimes is P_b^d. Policing imposes a cost on buyers, αP_b^d, where α reflects the punishment or incarceration conditional on being apprehended by the police, and P_b^d reflects the probability of being detected engaging in the

illicit drugs market. Hence, the utility of a consumer located in each market is,

$$\begin{aligned} u_A &= -\pi_A - tx - \alpha_A P_A^d, \\ u_B &= -\pi_B - t(1-x) - \alpha_B P_B^d. \end{aligned} \tag{1}$$

The marginal consumer is indifferent between traveling to A or B, and so is located at,

$$x^* = \frac{1}{2} + \frac{\pi_B - \pi_A}{2t} + \frac{1}{2t}(\alpha_B P_B^d - \alpha_A P_A^d). \tag{2}$$

All consumers closer to borough A than this marginal consumer will purchase drugs from the supplier located in A and so the size of the market for illicit drugs in borough A is $D_A = x^*$.[5]

2.2 Suppliers

Drugs suppliers simultaneously set prices to maximize profits.[6] The profits for the supplier in borough b are $\Pi_b = \pi_b D_b$, so the Nash equilibrium prices in each borough are,

$$\begin{aligned} \pi_A &= t + \frac{1}{3}(\alpha_B P_B^d - \alpha_A P_A^d), \\ \pi_B &= t - \frac{1}{3}(\alpha_B P_B^d - \alpha_A P_A^d). \end{aligned} \tag{3}$$

The equilibrium market size for illicit drugs in each borough is therefore a function of the punishment for drug related criminal activities in *both* boroughs, and the drugs related policing intensity in *both* boroughs,

$$\begin{aligned} D_A(t, \alpha_A, \alpha_B, P_A^d, P_B^d) &= \frac{1}{2} + \frac{1}{6t}(\alpha_B P_B^d - \alpha_A P_A^d), \\ D_B(t, \alpha_A, \alpha_B, P_A^d, P_B^d) &= \frac{1}{2} - \frac{1}{6t}(\alpha_B P_B^d - \alpha_A P_A^d). \end{aligned} \tag{4}$$

The inter-linkage of drugs markets across boroughs highlights that decriminalization policies in one borough, can change the behavior of drugs demanders and suppliers in other boroughs. Such concerns over drug tourism have been central to the policy debate regarding decriminalization, and whether drugs policy should be coordinated across jurisdictions. We address this issue head on later by using the model to understand spillover effects of the LCWS on neighboring boroughs, and comparing the differential effects of local and national decriminalization policies.[7]

[5] We focus on the effects of decriminalization on the relative size of drugs markets across boroughs. In reality, individuals are also faced with a choice of whether to consume or not. In this case, decriminalization, by reducing equilibrium drug prices, can induce individuals to become consumers. This is a key argument articulated by opponents of decriminalization. We return to this issue later.

[6] If suppliers also endogenously choose their location, then it is relatively straightforward to show that with decriminalization in borough b, both suppliers will find it optimal to relocate closer to borough b. This will have very similar effects both within borough b and across boroughs as those described.

[7] We have no data on drug prices by borough and so we do not emphasize the price predictions. It also remains unclear how reliable such price information would be given that it is often collected as part of some law enforcement exercise, or drug quality is unmeasured. Galenianos *et al* [2008] present evidence from the US on there

2.3 Crime and Policing

To map the model precisely to the data at hand, we assume there are two crime types: drug related (d) and other (o) crimes. The police in borough b choose the intensity with which to police each crime type, denoted P_b^d and P_b^o. The number of drug crimes (C_b^d) and other crimes (C_b^o), are both affected by the intensity of policing towards the crime, and potentially, on the size of the drugs market within the borough. Hence,

$$\begin{aligned} C_b^d &= \delta_{d(b)} D_b(t, \alpha_A, \alpha_B, P_A^d, P_B^d) - e(P_b^d), \\ C_b^o &= \delta_o D_b(t, \alpha_A, \alpha_B, P_A^d, P_B^d) - e(P_b^o). \end{aligned} \tag{5}$$

The first term captures that the illicit drugs market generates drug related and other crime types, and the second term captures that police effort towards a crime type reduces the number of those types of crime. We assume: (i) $\delta_{d(b)} > 0$, $\left|\delta_{d(b)}\right| \geq |\delta_o|$, implying the drugs market has a larger effect on drugs crime than on other crimes; (ii) for each crime type $j \in \{d, o\}$, $e'(P_b^j) > 0$, $e''(P_b^j) < 0$, $e(0) < 0$, so police intensity reduces crime at a diminishing rate.[8]

The police minimize a weighted sum of all crimes in the borough, under a resource constraint,

$$\min_{P_b^d, P_b^o} \lambda_b C_b^d + (1 - \lambda_b) C_b^o \ s.t. \ P_b^d + P_b^o = P, \tag{6}$$

where λ_b reflects the relative weight of drug related crimes in the police objective function, and P is the total police resources that can be allocated across the crime types. The optimal drug related policing intensity is implicitly defined through the first order condition,

$$\begin{aligned} (1 - \lambda_b) e'(P - P_b^d) - \lambda_b e'(P_b^d) &= \frac{\alpha_b}{6t} [\lambda_b \delta_{d(b)} + (1 - \lambda_b) \delta_o], \\ P_b^d &= P_b^d(\delta_{d(b)}, \delta_o, \alpha_b, \lambda_b, t, P). \end{aligned} \tag{7}$$

Hence the optimal policing efforts towards drugs crimes depends on the relationship between the drugs market and drugs crime in the borough and spillover effects onto other crime types ($\delta_{d(b)}, \delta_o$), punishments for engaging in drug related activities (α_b), the weight the police place on drugs crime (λ_b), transportation costs (t) and aggregate police resources (P).[9] Substituting (7) into (5), total

being considerable dispersion in the price-quality ratio for illicit drugs.

[8] We have made two simplifying assumptions: (i) any spillover effects of the drugs market on crime occur within the same borough; (ii) police effort towards one crime type has no impact on other crimes. The results are unaffected by relaxing these assumptions. A less innocuous assumption is that we rule out dynamic effects of policing on crime so that, for example, the effect of police intensity on drugs crime in (5) in period t is $e(P_b^{d,t}, P_b^{d,t-1})$. This would capture in reduced form, any learning-by-doing effects in policing, or the fact that it takes time for police to act on intelligence gathered in the previous period. As a consequence, the short run enforcement of the LCWS might have longer run effects on policing and hence the market for illicit drugs. We leave this theoretical extension for future work, but estimate both short and long run effects in our empirical analysis.

[9] As the right hand side of (7) is positive, $\frac{e'(P - P_b^d)}{e'(P_b^d)} \geq \frac{\lambda_b}{1 - \lambda_b}$. Hence even if $\lambda_b = \frac{1}{2}$, relatively more police effort is devoted to drugs crimes, $P_b^d \geq P_b^o$ because the externality the market for illicit drugs has on other crimes.

drugs crime in borough A is then given by,

$$C_A^d = \delta_{d(A)} D_A(t, \alpha_A, \alpha_B, P_A^d(\delta_{d(A)}, \delta_o, \alpha_A, \lambda_A, t, P), P_B^d((\delta_{d(B)}, \delta_o, \alpha_B, \lambda_B, t, P))) - e(P_A^d(\delta_{d(A)}, \delta_o, \alpha_A, \lambda_A, t, P)). \tag{8}$$

With these building blocks in place, we can now shed light on the multiple channels through which decriminalization affects crime. We assume throughout that borough A unilaterally introduces a decriminalization policy.

2.4 Decriminalization

The first channel is labelled a 'mechanical effect' in that some behaviors – the possession of small quantities of cannabis – are no longer recorded as drug offences, so $\delta_{d(A)}$ falls. Hence the market for illicit drugs has a smaller impact on drugs related crime All else equal, decriminalization should reduce recorded drug offences. However, the model makes precise that there will also be an endogenous response in police behavior, $P_A^d(.)$, due to the fall in $\delta_{d(A)}$. The police optimally devote less effort to drugs related policing, and this countervails the direct effect of a reduction in δ_d on drugs crime in (8). This reallocation of police effort from drugs related crime to other crimes, can reduce crime rates for non-related drugs. To summarize,

Result 1a (Mechanical Effect): *Suppose decriminalization reduces $\delta_{d(A)}$. This reduces drugs related police intensity, and has ambiguous effects on drugs related crime. Other crimes are more likely to decrease the smaller is the spillover effect of the drugs market on other crimes (δ_o).*

To see the intuition for the effect on other crimes, note that the larger is the spillover effect δ_o, the more any reallocation of police effort towards other crimes is partially offset by an increased drugs market. In the limiting case of $\delta_o = 0$ other crimes must necessarily fall because only the beneficial effect of the police reallocating effort towards other crimes is relevant.

A second channel through which decriminalization in borough A affects drug crime is that it reduces the costs to consumers of being apprehended by the police, α_A. We think of this is a weakened 'incarceration effect'. As (4) shows, this has a direct effect on the equilibrium size of the drugs market in borough A, and, as (7) shows, an indirect effect through the endogenous police response, $P_A^d(.)$. In particular, the police optimally devote less effort to drugs policing as the returns to such effort is diminished. These effects reinforce each other so that drugs crime in borough A unambiguously increases. As with the mechanical effect, as the police reallocate effort from drugs related crime towards other crimes, non-related drugs crimes might fall. To summarize,

Result 1b (Incarceration Effect): *Suppose decriminalization reduces α_A. This reduces drugs related police intensity, and unambiguously increases drugs related crime. Other crimes are more likely to decrease the smaller is the spillover effect of the drugs market on other crimes (δ_o).*

The third channel through which the local decriminalization of drugs in borough A manifests itself in this framework is through a change in police preferences, namely a reduction in λ_b, the relative weight of drug related crimes in (6). We label this as a weakened 'deterrence effect'. This directly affects the optimal effort the police devote to drugs and other crimes, and through this on

the equilibrium size of the drugs market and crime rates as seen in (8). Given the multiplicative interaction between α_A and P_A^d in consumer preferences (1), the effects through this channel are observationally equivalent to those operating through the incarceration effect. To summarize,

Result 1c (Deterrence Effect): *Suppose decriminalization reduces λ_A. This reduces drugs related police intensity, and unambiguously increases drugs related crime. Other crimes are more likely to decrease the smaller is the spillover effect of the drugs market on other crimes (δ_o).*

The model makes clear that *a priori*, the overall effect of decriminalization on drugs crime is ambiguous. While our data does not allow us to separately identify each effect, finding an overall *decrease* in drugs crime would suggest the direct mechanical effect of a reduction in δ_d dominates all the other effects. On the other hand, if decriminalization causes an overall increase in drugs crime, this suggests the other effects dominate and that it is important to take into account the behavioral response of consumers, suppliers, and the police.

The model also makes precise the inter-linkages between the drugs markets in the two boroughs. These only occur through the incarceration and deterrence effects described above. To see why, note that drugs related police intensity in B, $P_B^d(.)$ is independent of policy parameters in A: $\delta_{d(A)}$, α_A, and λ_A. Hence spillover effects occur because policy parameters in borough A change the size of the drugs market in B. As (4) shows, this only occurs through the incarceration effect, α_A, in the other borough, and the deterrence effect in the other borough altering $P_A^d(.)$ and hence $D_B(.)$. To summarize,

Result 2 (Spillovers): *The mechanical effect of decriminalization implies there are no spillover effects on crime in neighboring borough B. The incarceration and deterrence effects of decriminalization unambiguously imply a reduction in drugs related crime, and other crimes in the neighboring borough.*

By exploring the effects of a localized decriminalization on neighboring boroughs, it is possible to determine whether the mechanical effect is dominated by the other two. Given the multiplicative nature in which the incarceration (α_b) and policing parameters (P_b^d) interact in this, and many other models of individual criminal behavior, it is impossible to separately identify these effects in the absence of a more structural approach.

Finally, it is straightforward to establish the effects of a nationwide decriminalization policy where all parameters are common to both boroughs. Due to symmetry across boroughs, the drugs market is equally split so $D_b = \frac{1}{2}$. To summarize,

Result 3 (National Decriminalization): *The mechanical effect causes drugs crime to fall in each borough, although there is no change in the size of the drug markets overall ($D_b = \frac{1}{2}$). However, each channel implies the police reallocate their effort towards other crimes, unambiguously reducing crime rates for non-drugs crime.*

We have emphasized the distinction between drug and non-drug related crime, because this division of criminal offences is what our data is best suited to shed light on. However, there clearly exist important questions on how the decriminalization of cannabis affects the market for other illicit drugs. To think through such effects, suppose we interpret cannabis related crime

as drugs crime above (C_b^d), and non-cannabis related crime as other crimes (C_b^o). Then the framework highlights the decriminalization of cannabis allows the police to reallocate effort towards non-cannabis drugs crime. However, this might increase or decrease total drugs crime ($C_b^d + C_b^o$) depending on the strength of the spillover between cannabis and other drugs crimes (δ_o). Reasonably, given complementarity in the supply of different drugs, δ_o should be large and so decriminalization might cause crimes related to other drug types to also increase. We discuss scenarios along these lines in various thought experiments in Section 5 and the Appendix.

3 The Lambeth Cannabis Warning Scheme (LCWS)

3.1 Background

The trigger for the localized decriminalization policy in Lambeth that we focus on can be traced back to the publication of the Runciman Report in 2000. This was a high profile inquiry commissioned by the Police Foundation. The report's remit was to review and suggest amendments to the primary piece of legislation in the UK governing the policing of illicit drugs, the Misuse of Drugs Act 1971. The UK has a three tiered drug classification system, with assignment from Class-C to Class-A intended to indicate increasing potential harm to users, but not necessarily to society. The Runciman Report called for the classification system to more closely follow the scientific evidence of relative harms, and consequently that cannabis should be reclassified from Class-B to a Class-C drug. The report emphasized three benefits of making cannabis possession a non-arrestable offence: (i) reduce the number of individuals being criminalized; (ii) remove a source of friction between the police and local communities; (iii) free up police time.

Subsequent to the Runciman Report, the Metropolitan Police Authority (MPA) produced their own report on drugs policing, 'Clearing the Decks.' This first suggested the idea of a workable decriminalization 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 decriminalization 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.[10]

With the sanctioning of the Police Commissioner, Sir John Stevens, the LCWS was introduced in Lambeth as a pilot project on 4th July 2001. It was announced to run for six months. Under the scheme, those found in possession of small quantities of cannabis for their personal use: (i) had

[10] Police officers also reported concerns, following a recent disciplinary case, that they might face disciplinary action 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 [Warburton *et al* 2005, May *et al* 2007b].

the drugs confiscated; (ii) were given a warning. Prior to the policy such individuals would have been arrested and an offence recorded [Fuller and Dark 2002]. The police continued to prosecute cannabis suppliers, so there was no attempt to segment the markets for cannabis and other illicit drugs by for example, incentivizing suppliers to switch from supplying cannabis.[11] Following the support for the policy, at the end of the initial six month trial period, the policy was then announced to represent a permanent change in policing strategy.

To gauge public opinion to the policy experiment, an IPSOS-MORI poll was commissioned. This found broad local support for the scheme. 36% of surveyed residents approved outright of the policy. A further 47% approved provided the police actually reduced serious crime in Lambeth. Following the groundswell of support for the policy, at the end of the initial six month trial period, the policy was then announced to represent a permanent change in policing strategy.

The specific details announced with regards to the permanence of the policy could lead to differential effects on crime *within* the policy period. For example, if there are fixed costs to re-structuring criminal networks, then drugs suppliers might have incentives to delay any changes in their operation until the policy was announced to be permanent, six months into its operation. In contrast, if the policy effects are predominantly driven by changes in police behavior or reporting behavior of the public, it is more reasonable to suppose the LCWS to impact drug crime rates immediately after its introduction, and only while it is in operation.

3.2 Why Was the Policy Ended?

No single reason emerges as to why the policy was ended. Anecdotal evidence suggests local support for the scheme began to decline once the policy was announced to be permanent. 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. It was decided the LCWS should end and it formally ended on 31st July 2002. Ironically, this was just after the Home Secretary announced that cannabis would be declassified from a Class-B to a Class-C drug, a nationwide decriminalization that was eventually implemented in January 2004. In part because of disagreements over the policy's efficacy, in the post-policy period, the policing strategy towards illicit drugs 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 MPA announced that in Lambeth officers would continue to issue warnings but 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.

[11] If those found in possession of cannabis were juveniles, their parents would have been notified. The police also maintained records of previous warnings and retained the power to prosecute.

4 Data, Descriptives and Empirical Method

4.1 Data Sources

4.1.1 Crime Data

We construct a panel data set on crime for each London borough from multiple sources. For the most recent data, since January 2000, the MPA publishes monthly crime statistics for each of the 32 London boroughs. For each crime type, these statistics are collated by sub-offence. For example, in relation to drug related offences, there are statistics for the sub-offences of drugs possession, drug trafficking, and other drug crime offences, but these cannot be further split by drug type. We aggregate data at the sub-offence level into eight broad crime types: drugs, violence against the person, sexual offences, robbery, burglary, theft and handling, fraud or forgery, and criminal damage. For each crime type, the statistics measure the number of recorded offences, arrests, and clear-ups for each borough-month.

To continue these series back into the 1990s, we obtained individual crime data for all crimes committed in London between 1990 to 1997, from the National Digital Archive of Datasets (NDAD). For each crime, this details the date, borough, and type of crime committed. Using Home Office classifications we aggregate these individual records to construct the number of crimes at the borough-month level for each of the eight broad crime types described above. From these individual records, it is again possible to construct the number of offences, arrests, and clear-ups for each crime type by borough-month. Finally, for the intermediate period between the end of the NDAD based data (1997) and the start of the MPA based data (2000) we obtained data at the borough-month-crime type level directly from the MPA on request. One contribution of our study is to be the first to combine these data sources to create a panel data set for crime over almost two decades by borough-month in London.

4.1.2 Police Operations and Other Data

It is important to be able to measure whether the LCWS shifts crime patterns over and above the effects of all other police activities. To do so we construct a database of police operations across London boroughs in the sample period. To the best of our knowledge, this is the first time such a comprehensive database of police operations has been constructed and utilized.

As documented in Panel A of Table A1, we detail for each borough specific police operation, the type of criminal offence targeted and the dates of operation. Such operations cover a wide range of offences. Some operate, like the LCWS, within one borough; others are coordinated across boroughs. The length of police operations varies between a few months and two years. On many dimensions therefore the LCWS is not atypical of police operations more generally. Moreover, 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. Panel C shows police operations that are London wide – and so are

implicitly controlled for in our empirical analysis using year fixed effects, and Panel D records police operations that are referred to in MPA reports, but that we have insufficient detail on to code in any of the previous panels.[12]

To control for characteristics of the borough economy, we use the *Quarterly Labor Force Local Area Data*. This provides quarterly information by borough on demographic and labor market characteristics from April 1992 until December 2005. We interpolate this data to obtain a series at the borough-month level.

To summarize, for our core analysis evaluating the effect of the LCWS on crime rates, the unit of observation is borough b in month m in year y. The final data dimensions for this analysis covers 32 London Boroughs for nearly every month between April 1992 and December 2005. There are eight crime types: drugs, violence against the person, sexual offences, robbery, burglary, theft and handling, fraud and forgery, criminal damage, and for each crime type, but the series for drug offences has a break from April 1997 to March 1998. Finally, for each crime type we exploit information on the number of recorded offences, number of arrests, and number of clear-ups by borough-month.

4.2 Descriptive Evidence

Table 1 presents descriptive evidence on rates of drug offences in Lambeth and other London boroughs over time. We define the crime rate for drugs offences as the number of recorded drug related criminal offences per 1000 of the adult population. Recall, this total number of drug offences is the aggregation of offences for drug possession, trafficking etc. for all drug types (cannabis and non-cannabis). Four points of are of note. First, Lambeth has historically higher rates of drug offences than the average London borough, as shown in Column 1. Pre-policy, Lambeth had the 7th highest rate of drugs related crimes, 8th highest crime rate, 6th largest adult population, and 6th highest male unemployment rate.

Second, during the period the LCWS was in operation, drug offences in Lambeth rose by 95% relative to the pre-policy period. In the post-policy period, drug offences continue to rise by a further 37% in Lambeth. Both increases are significantly different from zero.

The second row of Table 1 shows that London-wide, the increase in drug offences between the policy and pre-policy period was significantly smaller than in Lambeth, at 39%. In the post-policy period, drug offence rates continue to diverge between Lambeth and the rest of London. The final row shows drug offences among geographic neighbors to Lambeth. If there is any drugs tourism into Lambeth during the policy period, it might stem predominantly from these boroughs. Interestingly, in contrast to the London-wide averages shown in the second row, drug offence rates do not rise significantly among Lambeth's neighbors when the LCWS is in place. Among Lambeth's neighbors, in the long run, drug offences do rise by 37% between the policy period and the post-policy period.

[12] Other police operations that have been studied in detail include Draca *et al's* [2008] study of the effects of Operation Theseus of the redeployment of police officers subsequent to the terrorist bombings in London in July 2005, and Machin and Marie [2005] on the Street Crime Initiative.

While suggestive, these patterns clearly highlight the need to control for time trends in drug offences throughout the empirical analysis and to show any results to be robust to alternative methods of allowing for time trends in criminal offences.

A clearer pattern emerges in Figure 1A that shows the time series for drug offences per capita in Lambeth and the average for all other London boroughs excluding Lambeth's neighbors. The horizontal axis measures time in terms of the number of months since April 1992. The faint dashed vertical lines corresponds to the breaks in the constructed time series for drug offences. These appear to have similar effects on the recording of drug offences in Lambeth and other boroughs.[13]

The policy period is then indicated by the arrow at the right of the figure. Three points are of note. First, there is a long run upwards trend in rates of drug offences in Lambeth and London more generally. However in the period immediately before the initiation of the LCWS, it appears as if the rate of drug offences is constant or slightly *downward* trending in Lambeth and London as a whole. Second, there is a large and permanent increase in drug offence rates in Lambeth during the policy period. There is no evidence 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.

Third, the within policy effect in Lambeth begins 6 months after the policy starts – precisely the time when the policy is announced to represent a permanent change in policing. This suggests any effects on recorded offences are unlikely to be driven by changes in reporting behavior of local residents, as we might expect residents to change behavior immediately after the LCWS is first introduced, and to revert back to their original reporting behavior once the LCWS was ended.

Figure 1B shows the corresponding time series for all non-drug related offences (C_b^o). Two points are of note. First, prior to the LCWS's introduction, we observe upward trends in these crimes in Lambeth and across London. Second, a few months into the policy period, criminal offences begin declining in Lambeth and this downward trend continues after the ending of the LCWS. In contrast for the rest of London, non-drug related offences remain relatively constant for the second half of the sample period.

4.3 Empirical Method

The remainder of the paper seeks to understand whether there is a causal impact of the policy on crime, conditional on all other determinants of crime. To do so, for borough b in month m in year y we estimate the following panel data specification,

$$\begin{aligned} \ln C_{bmy}^d &= \beta_0 P_{my} + \beta_1 \left[L_b \times P_{my}\right] + \beta_2 PP_{my} + \beta_3 \left[L_b \times PP_{my}\right] \\ &\quad + \gamma X_{bmy} + \lambda_b + \lambda_m + \lambda_y + u_{bmy}, \end{aligned} \tag{9}$$

where C_{bmy}^d is the number of drug offences per thousand of the 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

[13] Throughout the empirical analysis we control for a series of dummies to capture these underlying different data regimes.

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 drug crime rates in Lambeth during and after the LCWS policy period, relative to other London boroughs. Given the potential for localized spillover effects, highlighted by Result 2, we exclude geographic neighbors to Lambeth throughout. β_0 and β_2 capture London-wide trends, among non-neighbors to Lambeth, in drug crime during and post-policy.

We control for the following borough specific time varying variables: the share of the adult population that is ethnic minority, that is aged between 20 to 26, aged 25 to 34, aged 35 to 49, aged above 50, and the male unemployment rate. We construct a new database of police operations in London (at borough-month-year level) and control for the borough-month specific policing initiatives described in Panel A of Table A1. The fixed effects capture remaining permanent differences across boroughs, seasonality in crime, and London-wide policing and drugs policies. Finally, we control for dummies for each data regime used to construct the time series, and estimate (9) weighting the observations by the borough population.[14] Finally, defining time t as the number of months since January 1990: $t = [12 \times (y - 1990)] + m$, we assume a Prais-Winsten borough specific AR(1) error structure,

$$u_{bmy} = u_{bt} = \rho_b u_{bt-1} + e_{bt}, \tag{10}$$

where e_{bt} is a classical error term. u_{bmy} is borough specific heteroskedastic, and contemporaneously correlated across boroughs.

5 Baseline Results

5.1 Drug Offences

Column 1 of Table 2 presents the estimate of (9). In line with the descriptive evidence in Table 1 and Figure 1, Lambeth experiences a significant increase of 15% in recorded drug related crimes moving from the pre-policy period to when the LCWS was in place ($\widehat{\beta}_1$). Moreover, this increase is sustained in the post-policy period where reported drug crimes are 26% higher than in the pre-policy period in Lambeth ($\widehat{\beta}_3$). The foot of the table shows the p-value on the null hypothesis that the short and long run policy effects within Lambeth are equal. This null is rejected at the 5% significance level, suggesting the LCWS might have had long run effects on crime in Lambeth even after it was formally ended. The sign of these changes imply the direct mechanical effect of decriminalization, $\Delta\delta_{d(Lambeth)} < 0$, is more than offset by the other channels through which decriminalization affects drugs crime.

[14] On the controls, there exists evidence of causal relationships on crime of education [Lochner and Moretti 2004] and wages, particularly at the low end of the wage distribution [Machin and Meghir 2004]. Three data regime dummies are included, for the periods of March 1992 to March 1997 when the NDAD data is used to construct crime rates, for April 1997 to March 2000 when the MPA supplied data on crime is used, and for April 2000 onwards when the web based data from the MPA is used. By including dummies for each data regime we assume that any coding changes have homogenous effects on crime counts in each borough.

The coefficients on the policy and post-policy period dummies ($\widehat{\beta}_0$, $\widehat{\beta}_2$) are both not significantly different from zero suggesting no time trend in drug crimes over either the policy or post-policy periods, conditional on all other controls. These point estimates are almost equal to zero.

Columns 2 and 3 show these results to be robust to: (i) allowing for a borough specific quadratic time trend, namely including $\lambda_b \times t$, $\lambda_b \times t^2$ as additional controls in (9) to address concerns raised earlier of there being natural trends in drug crimes over time that might differ by borough; (ii) controlling for the set of police operations conducted by borough during the sample period, as recorded in Panel A of Table A1.[15]

To assess the magnitude of these effects, we note that the elasticity of drugs offences with respect to unemployment rate is .013, and effect that is significant at 1% level. Hence the policy (post-policy) period represents an equivalent to a 12% (20%) increase in the unemployment rate, relative to a mean unemployment rate of 8.8% with a standard deviation of 4.9%. By any metric, the LCWS has quantitatively large effects on drug crimes in Lambeth in the short and long run.[16]

Columns 4 and 5 check for dynamic policy responses during and after the LCWS's implementation. Column 4 splits the policy period into the first six months, when the policy was announced to be a temporary policing experiment, and the last seven months, during which it had been announced to be a permanent change in policing strategy. In line with the descriptive evidence in Figure 1, all of the within policy effect on drugs offences occurs after the policy is announced to be permanent. This lag between the introduction of the LCWS and changes in drugs crime allays concerns over there being reverse causality between drug offences and the introduction of the policy. This suggests that some part of the policy response picked up relates to changes in behavior of drug suppliers in response to signals of the permanence of the policy.

Column 5 breaks down the post-policy response into a separate effect measured for each year post-policy. This shows the post-policy effect to be long-lasting. The foot of the table shows that we cannot reject the null that the post-policy effect of the LCWS on drugs crimes in Lambeth is the same in the first and fourth year post-policy, although the point estimate on the post-policy is smaller after four years. This allays concerns that drug crime in Lambeth was naturally diverging from the London-wide average over time, or that the long run effects of the policy diminish over time an Lambeth reverts back to it old pre-policy crime levels.

[15] Given the potential for the police forces to coordinate actions, we also estimate (9) allowing the error term to be spatially correlated across boroughs [Chen and Conley 2001]. More precisely, we assume the error term takes the form, $u_{bmy} = \theta \sum_{c=1}^{N} w_{bc} u_{cmy} + e_{bmy}$, where θ is the spatially autoregressive parameter and w_{bc} is an element of the spatial weighting matrix $\mathbf{W}$, is an $N \times N$ matrix, where N is the number of boroughs. As $\mathbf{W}$ describes the spatial relationship between boroughs, $diag(\mathbf{W}) = 0$, and $\mathbf{W}$ is row-standardized so the standardized elements are $w_{bc}^s = w_{bc} / \sum_c w_{bc}$. Defining $\mathbf{W}$ as a contiguity weighting matrix where $w_{bc} = 1$ for neighboring boroughs, 0 for all other boroughs, the spatially autoregressive coefficient was found to be positive ($\hat{\theta} = 0.29$) and significant. The results are robust to alternative weighting matrices, including inverse distance and inverse distance squared weights, where distance is measured from the spatial centroid of each borough.

[16] An alternative yardstick to compare these effects to is the elasticity of crime rates with respect to the number of police officers [Levitt 1997, Di Tella and Schargrodsky 2004, Draca *et al* 2008]. Levitt [2004] overviews this literature and suggests a median estimate of around $-.4$ to $-.5$. Assuming these estimates apply to drugs crime in London, the long run increase in drug crime of 26% reported in Column 1 suggests reducing police numbers by around half would have similar effects. Such magnitudes hint at possibly large reallocations of police effort towards other crime types, as documented in the next Section.

From January 2000 onwards, the MPA has published information on detailed sub-categories of criminal offences at the borough-month level. Columns 6 and 7 show that over this time frame, the aggregate rise in total drug offences is primarily driven by increases in the possession of drugs: 9.7% (20%) during the policy (post-policy) period. We think it reasonable that offences for drug possession are committed both by consumers and suppliers of illicit drugs, either because they are in possession of drugs other than cannabis, or in possession of large quantities of cannabis. Within Lambeth, changes in recorded drug trafficking offences are not statistically distinguishable from zero. The results also confirm no change, on average, in drug offences by sub-category in other London boroughs during and after the LCWS's operation.[17]

For drugs possession offences, the last two columns again estimate the dynamic policy responses. As for aggregate offences, we find that drug possessions only increase significantly in the last seven months of the policy when the LCWS was announced to be permanently in place. The post-policy effect on drug possessions also mirrors the earlier results in that the long run effects appear to be stable over time, and it is not that the rate of such offences appears to be naturally diverging between Lambeth and other London boroughs.

In Appendix Table A2 we present a series of robustness checks on these baseline results related to: (i) alternative specifications for within borough time trends; (ii) issues related to the measurement of crime [Pepper *et al* 2009]; (iii) falsification exercises.

Finally we note that an alternative explanation for the documented increase in drug offences is that the public are simply more likely to report drug offences during the policy, or especially when the policy is announced to be permanent. However, it remains unclear why such a change in the public's behavior should persist after the policy has ended, or why reporting behavior specifically related to drugs possession offences rather than drug trafficking offences, say, should change. Indeed, in the next sections we show changes in police behavior and crime rates for other crime types that are unlikely to all be driven by changes in reporting behavior.

5.2 Arrests and Clear-Up Rates for Drug Crimes

The model highlights that any increase in the market size for illicit drugs following decriminalization is partly due to reduced police intensity towards drugs related crime, $P_b^d(.)$. As (7) shows, this occur through the mechanical, incarceration and deterrence channels. To check for this, we explore the policy effects on direct measures of police behavior. Following the same empirical methodology in (9), we estimate the short and long run effects of the LCWS on the arrest and clear-up rates for drug offences. As individuals are not necessarily immediately arrested for offences that occur in any given borough and month, 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

[17] To see why possessions offences relate to the behavior of suppliers as well as consumers, note that the police always have the option to record an offence or charge an individuals with possession with intent to supply. However it might be substantially more difficult to prove an intent to supply, so in practice drug suppliers are charged with a lesser offence of possession. As discussed in more detail below, we note that historically over 90% of cannabis related offences are classified as being for possession, and only 4% relate to possession with intent to supply.

within the borough. The clear-up rate is analogously defined.[18]

Table 3 presents the results. Columns 1 to 4 focus on the policy effects on the log of the arrest rate, and Columns 5 to 8 have the log of the clear-up rate as their dependent variable. As before we condition on borough, month, and year fixed effects, time varying socio-demographic characteristics and police operations, and allow for the autocorrelated error structure in (10). Column 1 shows that the arrest rate significantly drops by 30.4% ($\widehat{\beta}_1$) during the policy period. In the long run this effect is reduced in absolute magnitude, although arrests per drug offence in Lambeth still fall by over 15% relative to the pre-policy period ($\widehat{\beta}_3$). There is not much evidence of declines in arrest rates for drugs crime in London as a whole.

Column 2 shows this result to be robust to restricting the sample to January 2000 onwards, when crime data by sub-offence is available. Columns 3 and 4 exploit this information on sub-offence type. In line with the earlier evidence on drug offence rates, the decline in arrest rates is predominantly driven by significant declines in arrest rates related to drugs possession. Relative to the pre-policy period, there is no significant change during or post-policy in Lambeth in terms of arrest rates for drug trafficking, or for other drug offences.

Columns 5 to 8 present analogous specifications on clear-up rates for drugs crime. The pattern of coefficients is consistent with the seeming decline in police intensity on drugs crime implied both by the results for offence rates (Table 2) and the results in Columns 1 to 4 on arrest rates for drugs crime. In particular, we find that relative to the pre-policy period, clear-up rates significantly decline in Lambeth when the LCWS is in place by 33.6%. As with arrest rates in the long run, the relative fall in clear-up rates post-policy is smaller – at 17.8% – but remains significantly lower than in the pre-period. The remaining Columns confirm that this is largely driven by declines in clear-up rates specifically related to crime related to drugs possession.[19]

5.3 Does Decriminalization Allow the Police to Re-allocate Effort Towards Other Crime Types?

We now exploit data on the full range of crime types to explore whether the police in Lambeth reallocate their effort away from drugs crime to other types of crime when the LCWS is initiated, as suggested by theory. As before we examine both the short run effects on other crime types while the LCWS is actually in place, as well as potential long run effects post-policy. To do so we estimate specifications analogous to (9) for each crime type and so we continue to condition on borough, month, and year fixed effects, time varying socio-demographic characteristics and police

[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. However, we do not have information on the stock of unsolved crimes, nor is it clear whether officially announced clear-up rates merely refer to the number of clear-ups, and are not measured relative to some stock of unsolved crimes. As Table A3 shows, the results are robust to just focusing on the number of arrests or clear-ups per 1000 of the adult population.

[19] Police incentives are based on clear-up rates. Hence the documented declines in clear-up rates suggest the police were not gaming the system by changing how they recorded offences to focus only on those that were easy to clear-up.

operations in the borough, and allow for the autocorrelated error structure described in (10).[20]

Table 4 presents the results. To begin with, Column 1 examines the policy effect on total recorded criminal offences, including drug offences. We find that during the policy period total crime was rising in London on average ($\widehat{\beta}_0 = .074$), and there was no significant differential rise in total crime within Lambeth ($\widehat{\beta}_1 = 0$). In the post-policy period, the total crime rate in Lambeth significantly fell by 8.4%, again against a backdrop of London wide increases in aggregate crimes. In Column 2 when drugs crime is excluded from the total, then as expected given the earlier results, we find even smaller changes in total crime within Lambeth during the period, and larger declines in crime rates post-policy.

The remaining columns estimate (9) for each non-drugs related offence type. Three points are of note. First, we observe robust evidence that there are falls post-policy in recorded offences for nearly all crime types. These long runs falls in crime rates in Lambeth go against the London wide trend of increases in each type of crime. As the model suggests, even in the absence of direct spillovers between the drugs market and other specific types of crime ($\delta_o = 0$), decriminalization can still reduce such crimes by freeing up police resources.[21]

Second, during the policy period there are rises in robbery and theft and handling. These short run rises might reflect something about the technology of crime. As emphasized in the model, the market for illicit drugs might have larger spillover effects onto these crime types ($\delta_o > 0$) so they are complements to drug offences, consistent with other evidence [Corman and Mocan 2000]. Given the long run rise in drug related offences in Lambeth documented in Table 2, such complementarity would also then explain why these crimes in particular fall by smaller percentage amounts than other crimes.[22]

[20] Before embarking on such an exercise, it is worth considering whether it is plausible a sufficiently large number of police man-hours could have been freed up by these policy changes. We note that 1390 cautions were given for possession of cannabis during the policy period. The PRS consultancy group, which evaluated the pilot scheme at the 6 month point, estimated that for every warning given, 3 police hours were saved by avoiding custody procedures and interviewing time. This amounts to 4170 police hours saved per annum, the equivalent of 2.75 officers. The MPA stated that during the policy period, the size of the Lambeth police force was 860 officers, Thus, based on these numbers, the policy changed the effective police force by .3%. However, there are two caveats to this that both lead this to be an underestimate of the total time available to be reallocated. First, the MPA noted that the 3 hours per warning 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 5 hours dealing with cannabis arrest and 2 more hours operational time at police stations [Wood 2004]. Second and more significantly, the figures refer to the amount of time save *conditional* on apprehending an individual for an offence that would previously have been recorded. The first order effect on police man-hours would presumably be not to search for such individuals in the first place.

[21] As shown in Panel A of Table A4, if we repeat the analysis splitting the policy effect into two parts for the first 6 months and last 7 months when the LCWS is in operation, we observe that drugs offences are the only crime type to rise in the second half of the policy period relative to the first half. *All* other crime types in Lambeth fall in the second part relative to the first, consistent with the long run substitution patterns found.

[22] However, this interpretation should be treated with caution. As shown in Panel A of Table A5, if we repeat the analysis splitting the policy effect into two parts for the first 6 months and last 7 months when the LCWS is in operation, we observe significant rises in robberies and thefts in the first half of the policy period, despite there being no significant increase in drugs offences in this period. We can only speculate that the effects in the first six months of the policy period might capture new consumers immediately begin traveling to Lambeth after the LCWS is introduced, and are victims of this type of crime.

Third, in the post-policy period, among the smallest percentage falls occur for crimes related to violence against the person, robbery, and theft and handling. Perhaps such crimes are those on which the greatest externalities from the illicit drugs market occur ($\delta_o > 0$).

To pin down whether these long run declines in all crime types other than drugs crimes is due to a reallocation of police effort, Table 5 estimates the short and long run policy effects on our measures of police efficiency: arrests (Panel A) and clear-ups (Panel B). We see that in aggregate, arrest rates fell during the policy period but rose in the post-policy period. Considering specific crime types, during the policy we observe significant increases in the number of arrests per offence for robbery, despite the rise in robbery offences in the policy period shown in Table 4. Post-policy, there are significant increases in arrest rates for a broad range of crime types. However, Panel B shows little evidence that these improved accusation rates feed through into differentially higher clear-up rates in Lambeth relative to the rest of London.

Taking together these results with those on offences, the evidence suggests a significant reallocation of policing intensity after the introduction of the LCWS, away from drugs crimes and towards all other crimes. This reallocation appears to persist long after the LCWS is officially ended, and is reflected in marked increases in arrest rates for a broad range of crime types (Table 5, Panel A) – that might itself feedback into lowering offence rates (Table 4). However, this reallocation does not ultimately result in higher clear-up rates over the period of study, so the police are not more likely to solve any given crime even if they appear to successfully devote more effort towards finding and accusing potential offenders.[23]

5.4 Interpretation

The evidence shows that when the Lambeth police announce their local decriminalization policy to be permanent, a surge in offences for drugs possession occurs, and remains well after the official ending of the LCWS. The increase in Lambeth takes place against a backdrop of no similar increases in drug offences London-wide. Police effectiveness against drugs crime – as measured in terms of arrest and clear-up rates – drops considerably during the policy period, and remains at lower levels thereafter. Again this change in Lambeth occurs against a backdrop of no similar London-wide declines in police effectiveness against drugs crime. This collapse in police effectiveness against drug crimes might itself feedback into higher rates of drugs offences. Theory suggests there are multiple channels through which such a policy causes such changes in drugs crime and police behavior: the mechanical, incarceration and deterrence effects.[24]

[23] Panels B and C of Table A5 show that within the policy period, there is no immediate increase in numbers accused for other crimes, and in the second half of the policy period, there is only a significant increase in arrest rates for burglaries. In contrast, there is an immediate decline in arrest and clear-up rates for drug crime. As the positive effects of arrest rates for non-drug crimes shown in Panel A of Table 5 take time to materialize, this is suggestive of dynamic effects of policing so that as the police reallocate their effort from drugs crimes to other crimes, it takes time for them to learn how to find and accuse individuals for other crime types.

[24] If we repeat the analysis splitting the policy effect into two parts for the first 6 months and last 7 months when the LCWS, then as shown in Panels B and C of Table A5 (Column 3), we observe an immediate significant decline in accusation and clear-up rates for drugs crime in Lambeth. This immediate change in police intensity on drugs crime might then, because of a lack of deterrence, create the conditions for the surge in drug offences that occurs

However, theory also suggests that a potential benefit of the decriminalization policy might be to allow the police in Lambeth to reallocate their effort away from drugs crime to other types of crime. The evidence suggests this is indeed the case. In the post policy period, crime rates for other offences significantly fall, and especially for those crime types that are least associated with the illicit drugs market. In addition, police effectiveness against other crimes, as measured by the number of arrests per crime, significantly rises in Lambeth post-policy relative to other London boroughs. This is precisely in line with the police reallocating their effort under the LCWS.

Two further points need to be discussed in relation to interpreting the evidence: (i) whether the results can be explained by changes in reporting behavior of members of the public; (ii) implications for the drugs markets specifically for cannabis and non-cannabis drugs.

5.4.1 Reporting Behavior

Most recorded offences are initially reported by members of the public. The LCWS policy might signal to individuals that the police are taking drugs crime more seriously and then cause them to be more likely to report such offences *per se*. In the model developed this can be interpreted as an increase in $\delta_{d(A)}$ so that a stronger relationship exists between the true size of the market for illicit drugs and recorded drugs related crime, and that the police place greater weight on drugs related crime in their objective function, λ_A. This implies a set of comparative statics that move in the *opposite* direction to the mechanical and deterrence effects in Results 1a and 1c.

On the one hand, this interpretation explains the rise in recorded drugs crime in Table 2. However, this change in behavior would have to apply to drugs crimes specifically related to drug possession and not drug trafficking, and would also have to persist well after the policy had ended. Furthermore, the hypothesis that the policy causes $\delta_{d(A)}$ or λ_A to increase has the clear prediction that the Lambeth police reallocate their effort away from other crimes towards drug related crime. This is flatly contradicted by: (i) the results in Table 3 showing a significant reduction in police effectiveness against drugs crime, both in terms of arrest rates and clear-up rates; (ii) the results in Tables 4 and 5 on the falling incidence of other crimes, and rising police effectiveness against other crimes in terms of arrest rates.

To summarize, while undoubtedly there might be some change in reporting behavior of the public in response to announcements related to the LCWS, the balance of evidence does not suggest that such channels are the driving force behind the complete set of results we document.

5.4.2 Cannabis and Non-Cannabis Drugs Markets

While the theory and evidence presented emphasize that decriminalization allows a reallocation of police effort away from drugs related crime to other crime types, it might also be the case that the policy allows a reallocation of police effort from cannabis related to crime to other non-cannabis related drugs crime. To examine the consequent effects on the illicit drugs market requires detailed information on drug offences *by drug type*. This is unavailable for the years around the LCWS's

six months into the LCWS when the policy is announced to be permanent.

operation.[25] However, we can use detailed historic information from the mid 1990s on crime by drug type to present a series of thought experiments. These help shed light on what would have to be the underlying changes in the illicit drugs market, by drug type, to explain our baseline result that the policy caused a long run increase of 20% in drugs possession offences (Table 2, Column 6). The methodology underlying these thought experiments is described in the Appendix.

Two crucial pieces of evidence are needed to conduct these thought experiments. First, the experiments vary in what they assume on whether some police officers behaved as if the LCWS was *de facto* already in place before its formal introduction.[26] Second, we need some estimate of how drugs offences break down by drug type. On this point, we can use historic data from the individual crime level data from NDAD in 1996/7 described in Section 4.1.1. This shows that 71% of drug offences relate to cannabis in Lambeth. Among these cannabis related offences, over 91% of offences relate to cannabis possession.

Using this information, we outline three thought experiments. In the first scenario, assume the increase in total drugs possession offences recorded in Table 2 is entirely driven by an increase in offences related to non-cannabis possessions *and* that recorded offences of cannabis possessions post-policy fall to zero, so the LCWS was not *de facto* in place before its formal introduction. If so, then only a five-fold increase in non-cannabis possessions could explain the long run effects. Intuitively, the increase in possession of other drug types has to increase so dramatically because the majority of drug offences relates to cannabis possession in Lambeth and it is assumed that the policy's introduction causes all such offences of cannabis possession to no longer be recorded as an offence, so the direct negative mechanical effect, $\Delta\delta_{d(Lambeth)} < 0$, is large.

Our second scenario assumes the policy was *de facto* already in place so that all the effects essentially are explained only through the incarceration effect as drug consumers respond to the policy announcement. Hence there is no change in recorded cannabis offences over time by the police. To explain the magnitude of the results in Table 2, we would still require a near doubling in non-cannabis possessions. The final scenario supposes the increase in drug possession offences is entirely driven by an increase in offences related to cannabis possession, say because of increases in quantities possessed. Therefore there is no change in *non-cannabis* possessions. If so, a 26% increase in cannabis possessions from below the threshold for personnel use to above the threshold would explain the earlier results.

Clearly, perhaps the most realistic scenario combines elements of all three experiments, so there is some increase in non-cannabis related possessions, pre-policy some proportion of police officers behave as if the policy is *de facto* in place, and there is some increase in cannabis possessions. What is certainly ruled out by these thought experiments however, is that there is no underlying change in the equilibrium supply and consumption of both cannabis and other illicit drugs in Lambeth post-policy.

[25] We are unable to use direct data on illicit drug usage, say from the *British Crime Survey*, because, while this data is nationally representative, the samples are too small to produce reliable estimates at the borough level.

[26] May *et al* (2002) provide a detailed discussion of police strategy towards cannabis in England and also suggest police officers use discretion in recording cannabis possession offences.

6 Extended Results

6.1 Spillovers To Neighboring Boroughs

Given the localized nature of the LCWS policing experiment, it is reasonable to expect spillover effects on neighboring boroughs. The model developed highlights that such a localized decriminalization should alter the behavior of drug consumers who become more likely to travel to Lambeth to purchase cannabis. Suppliers might also have incentives to locate closer to Lambeth.[27]

To check for spillover effects, we estimate a specification analogous to (9) but additionally control for interactions between the policy and post policy-periods and whether the borough is a geographic neighbor of Lambeth ($N_b = 1$) or not ($N_b = 0$),[28]

$$\begin{aligned} \ln C^d_{bmy} &= \beta_0 P_{my} + \beta_1 \left[L_b \times P_{my}\right] + \beta_2 PP_{my} + \beta_3 \left[L_b \times PP_{my}\right] \\ &\quad + \beta_4 \left[N_b \times P_{my}\right] + \beta_5 \left[N_b \times PP_{my}\right] + \gamma X_{bmy} + \lambda_b + \lambda_m + \lambda_y + u_{bmy}. \end{aligned} \tag{11}$$

As shown in Column 1 of Table 6, there are significant declines in drug-related crime rates in Lambeth's neighbors during the period in which the LCWS is in place in Lambeth. Post-policy the effect on neighbors remains negative and significant albeit at the 10% significance level. Column 2 estimates the effect separately for each neighbor and shows the previous effect to be driven by two boroughs in particular: Merton and Southwark. Columns 3 and 4 exploit the information on more detailed drug offence types available from January 2000 onwards. These show how these boroughs experienced a decrease in both offences related to drugs possession and drugs trafficking. In line with the model, it appears as if the behavior of both consumers and suppliers of drugs is altered in boroughs neighboring Lambeth.

While these results are indicative of some drugs tourism into Lambeth, the negative effects in neighbors cannot fully explain the increase in Lambeth. For example, at the foot of the table we report p-values on the hypothesis that the sum of coefficients in neighbors is equal and opposite to that in Lambeth. This is rejected for aggregate drugs offences and drugs possession offences during the policy and post-policy period. Overall the magnitude of the estimated effects imply that at most, the declines in drugs crime neighbors to Lambeth might explain up to half of the increases in Lambeth.

The remaining columns check for any response in policing intensity in neighboring boroughs. We find mixed evidence for whether the police in neighboring boroughs respond to decriminalization in Lambeth. On the one hand there are no significant changes in accusation rates in neighbors for drugs crime but there are for other crime types. Clear-up rates do not significantly change for either offence type in neighbors during or post policy. More research is needed to better understand the policing objectives and responses across boroughs to localized police initiatives.

[27] Other studies examining the effects of policing on the spatial displacement of crime include Di Tella and Schargrodsky [2004] and Draca *et al* [2008]. Both exploit police redeployments caused by terrorism.

[28] We define the geographic neighbors of Lambeth to be Croydon, Merton, Southwark, Wandsworth, and Westminster. All these boroughs have direct transport links to Lambeth, although not necessarily links via the underground network. The results are robust to excluding Westminster as a neighbor as it lies north of the River Thames.

6.2 Hedonic Evaluation of the LCWS

While crime is undoubtedly a major social cost of the illicit drugs market, there might be other social costs factor in such as effects on educational outcomes, road accidents, hospital admissions, and alcohol consumption [Miron and Zweibel 1995].[29] The total social cost of decriminalization should be reflected in house prices [Rosen 1974]. We therefore estimate the association between house prices and the decriminalization of cannabis in Lambeth. More precisely, we exploit information at the zip code level on house prices to estimate a specification analogous to (9). The unit of observation is zip code sector s in quarter q in year y, where we note that zip code sectors are within borough. This later allows us to explore whether and how the effects of decriminalization affect house prices within Lambeth. To begin with we estimate a panel data specification of the form,[30]

$$\begin{aligned} \ln h_{sqy} &= \beta_0 P_{qy} + \beta_1 \left[L_b \times P_{qy}\right] + \beta_2 PP_{qy} + \beta_3 \left[L_b \times PP_{qy}\right] \\ &\quad + \gamma X_{bqy} + \lambda_s + \lambda_q + \lambda_y + u_{sqy}, \end{aligned} \tag{12}$$

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;[31] 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-vary explanatory variables are lagged by one quarter. In X_{bqy} we continue to control for socio-demographic and police operation controls, as in (9). We also allow for a borough specific time trend ($\lambda_b \times qy$) to capture common house price movements, and control for fixed effects for zip code, quarter, and year. 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.

Table 8 reports the results. Column 1 shows that in the long run after the LCWS is first introduced, house prices fall by 6.1% more in Lambeth relative the London wide average. In Column 2 when we control for borough specific linear time trends, we continue to find a negative

[29] On hospital admissions, 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] present evidence they are substitutes, while Pacula [1998] finds them to be complements.

[30] The house price data cover 25 of the 32 boroughs used for the crime analysis. The boroughs that are not covered are Barking and Dagenham, Bexley, Harrow, Havering, Hillingdon, Kingston-upon-Thames and Sutton. There are a total of 509 distinct zip codes in the final sample, with an average of 25.3 zip codes per borough. There are 33 distinct zip codes in Lambeth.

[31] 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/. House price information is available for terraced houses, detached, semi-detached, and flats/maisonettes. Finally 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.

and significant house price effect in Lambeth. The negative effects on house prices in the long run exist despite the overall falls in total crime experienced in Lambeth post-policy, as documented in Table 4. Hence Lambeth residents either place disproportionate weight on drugs related crime relative to all other crimes, and there might exist other social costs – not captured in crime data – that make the neighborhood less desirable.[32]

As house price data is available by zip-code, the remaining specifications examine whether there are heterogeneous effects of decriminalization on house prices within Lambeth, depending on the location of drugs crime. To do so we identify which zip-code sectors within each borough have higher than median drugs crime rates and define these zip codes to be drugs crime 'hot-spots'. We then explore whether house prices vary differentially within borough between hot-spots and non-hot-spots, and whether this difference in Lambeth differs from other boroughs over time.[33]

Column 3 shows that all of the long run negative effect of decriminalization on house prices within Lambeth occurs in these drug crime hot-spot zip codes, and the magnitude of the fall, at -13.9%, is significantly larger than in previous all-Lambeth estimates. Indeed, there is no significant effect of decriminalization on house prices on non hot-spot zip codes within Lambeth. We also see that hot-spot areas in other boroughs appear to have positive and significant house price rises, consistent with there being convergence in house prices across neighborhoods within the borough. In high crime neighborhoods, a greater proportion of property sales might relate to flats and maisonettes, rather than terraced houses. Hence it is reassuring to find the triple-difference estimates to be very similar for such properties, as Column 4 shows.[34]

Finally, in Column 5 we explore how house prices for terraced houses change in neighboring boroughs. In contrast to Lambeth, we find that house prices in hot-spot zip codes in neighbors to Lambeth do not decline relatively more than other areas in neighboring boroughs. This might reflect that the drugs tourism documented earlier originates from drugs crime hot-spots in neighboring boroughs.

Overall these data suggest that for local residents, the total social effects of decriminalization, such as alcohol use and visible anti-social behaviors, reduce the willingness to pay within these neighborhoods and increase within borough inequality in house prices between high and low crime zip codes. These are important channels through which the effects of decriminalization might operate in the long run.

[32] To get a sense of the magnitudes of these effects we compare our results to Gibbons [2004] who estimates that for London, that a standard deviation increase in property crime leads to a 10% drop in house prices. Hence our results imply the sensitivity of house prices to drugs related crime must be at least as large to generate house price falls overall in Lambeth, which recall experiences falls in property crime but rises in drugs crime (Table 4).

[33] The major caveat to the definition of hot-spot that we use is that it is based on data published in 2008/9. Only for these most recent years does the MPA publish detailed crime statistics by ward, which are areas within a borough and can be readily matched to zip code sectors. Hence in using this *ex post* definition of hot-spots we are implicitly assuming the location of crime within a borough does not vary over time. We cannot use the individual level data from NDAD to define hot-spots pre-policy because crime locations are not precisely matched to zip codes in that data.

[34] May *et al* (2007a) provide detailed descriptive evidence on drug dealing in Brixton – an important commercial centre within Lambeth. They describe the geography of drugs crime in Brixton, how it affects other crimes.

6.3 Nationwide Decriminalization

Analyzing the effects of decriminalization policies on social outcomes such as crime, provides a rare opportunity to compare the effects of such policies when implemented locally – as in the LCWS in Lambeth between 2001 and 2002 – and nationally, as cannabis was declassified in the UK as a whole between 2004 and 2009. The model and evidence presented suggest localized decriminalization induces drug tourism and so has spillover effects onto other boroughs. Comparing the results to a nationwide decriminalization policy allows us to make father headway in understanding the costs and benefits of coordinated such policies across jurisdictions, when mechanisms such as drugs tourism are shut down, as highlighted in Result 3.

6.3.1 Background

While it is impossible to establish a direct relation between the local and national decriminalization policies we study, it is interesting to note that the seeds of the nationwide decriminalization policy were sown in October 2001 – during the initial six month phase of the LCWS – when the then Home Secretary, David Blunkett, asked the Advisory Council on the Misuse of Drugs (ACMD) to review the classification of cannabis. In March 2002 the ACMD recommended cannabis be declassified to Class-C, because the existing classification was, "disproportionate in relation both to its inherent toxicity, and that of other substances...within Class-B". In March 2002 the Parliamentary Home Affairs Select Committee supported such a declassification and cannabis was formally declassified from a Class-B drug to a Class-C drug in the UK on January 29th 2004.[35]

The nationwide policy mirrored the LCWS policy experiment in several ways. First, the Association of Chief Police Officers advised officers to give street warnings for most possession cases. The police maintained the power of arrest for possession and this was advised to be used under aggravating circumstances. The maximum penalty for possession declined from 5 to 2 years with declassification. Second, the policy was intended to represent a permanent change in policing strategies. Third, a key reason for the change cited by the Home Office was that it would free up police resources to tackle higher priority Class-A drug crimes. Fourth, as with the LCWS, the nationwide decriminalization policy did not try to segment the market for cannabis from that for other illicit drugs by for example, incentivizing suppliers to switch from supplying illicit drugs in general, to cannabis in particular. Indeed, the penalty for the *supply* of Class-C drugs increased at this time to coincide with those for Class B drugs, to a maximum of 14 years.

As with the LCWS, the nationwide decriminalization policy would eventually be reversed, on January 26th 2009, due to political pressures, and contrary to the advice of the ACMD. This only allows us to compare the within policy effects of local and national decriminalization policies.[36]

[35] Warburton *et al* (2005) and May *et al* (2007b) discuss the background to this nationwide policy in more detail. They provide descriptive evidence on the limited effects of the policy on drugs activities, and how it affected the behavior and perceptions of the police and users.

[36] Concerns began to grow over nationwide declassification driven by potential links between cannabis use and mental health, and changes in the composition of psychoactive ingredients in cannabis supply. In March 2005, the Home Secretary asked the ACMD to review the classification of cannabis in light of research linking cannabis to mental health problems. There was also concern regarding cannabis varieties with high levels of the active

6.3.2 Results

Given the policy is simultaneously introduced in all boroughs, we can only identify its effect on crime exploiting differences in within borough variations in crime rates over time. More precisely, we estimate the following panel data specification for borough b in month m in year y,

$$\ln C^d_{bmy} = \beta_0 N_{my} + \gamma X_{bmy} + \lambda_b + \lambda_m + \lambda_y + \tau_o t + \tau_1 t^2 + u_{bmy}, \tag{13}$$

where C^d_{bmy} is the number of drug offences per thousand of the population aged 16 and over, and N_{my} is a dummy for when the policy begins. All other controls as as before and we also control for an aggregate quadratic time trend. The only other modification is that the QLSLAD data from which the demographic controls are derived, is only available until the first quarter of 2006. We therefore extrapolate this data so that (13) can be estimated for the entire period during which the nationwide policy is in place. We set the sample to run from January 1999 until December 2008 to have an approximately equal number of observations pre-policy and under the policy, that begins in January 2004. Table 9 reports the results.

Column 1 estimates (13) and shows no significant change in drug offence rates under the policy. Similarly controlling for an aggregate linear or cubic time trends, or allowing for borough specific trends, there is no evidence that recorded drug offences rose significantly after decriminalization. Taken together, the evidence of national decriminalization is far less compelling that such a policy increases drug usage, in line with evidence from other studies on nationwide decriminalization both in the UK [Pudney 2010] and other countries [MacCoun and Reuter 2001].

The remaining columns check for evidence of a reallocation of police effort towards other crime types. Column 2 shows that, conditional on an aggregate quadratic time trend, there remains evidence of a slight increase in non-drug offences. The most striking result is however that, as in the LCWS, there is a significant fall in accusation rates for drug offences of 4.9%, and a significant increase in accusation rates for other crime types. In line with the model, nationwide decriminalization does appear to cause the police to reduce policing intensity towards drugs crimes and to reallocate their effort towards other crimes. However, as with the LCWS, we do not observe significant changes in clear-up rates.[37]

psychoactive ingredient tetrahydrocannabinol (THC). The AMCD responded in December 2005, recommending the drug remained classified as Class-C. They noted that based on the quality of research on drugs and mental health, it was difficult to establish causality between cannabis use and mental health issues such as schizophrenia or depression. In July 2007 the Home Secretary requested another AMCD review of the classification of cannabis due to "real public concern about the potential mental health effects of cannabis use, in particular the use of stronger forms of the drug, commonly known as skunk". In April 2008 the ACMD responded, again recommending cannabis stay classified as Class-C drug. In May 2008, the Home Secretary announced the government was to reclassify cannabis as Class-B. A Home Office circular stated, "in reaching its decision the Government has also taken into account wider issues such as public perceptions and the needs and consequences for policing priorities".

[37] Given the starkly different implied effects of local and national policies on drugs usage, our analysis highlights that when conducting cost-benefit analysis of the likely effects of decriminalization at any given jurisdictional level, such as in Miron [2008], it is important to only utilize data on policy responses from similarly scaled policies.

7 Discussion

This paper evaluates the effects of a localized and one-year policing experiment that decriminalized cannabis possession in the London borough of Lambeth, on crime rates in Lambeth and neighboring boroughs. We find that decriminalization in Lambeth leads to a surge in drug related offences, and a collapse in accusation and clear-up rates for such offences in Lambeth. These effects are quantitatively large and persist well after the policy experiment ends. However, the policy allows the Lambeth police to reallocate their effort towards other crimes, leading to permanent reductions in nearly all other crime types.

On drugs tourism, the evidence suggests drug consumers and suppliers relocate to Lambeth from neighboring boroughs after decriminalization. Finally, comparing the localized decriminalization to nationwide decriminalization, we find the latter does not lead to a growth in the size of the illicit drugs market but does allow the police to reallocate effort towards non-drug crime. We interpret each set of results through a Hotelling-style model we develop that makes precise the behavioral response to decriminalization of the police, suppliers and demanders of drugs.

Given the lack of reliable data on actual drug usage by borough, we have conducted thought experiments to infer the effects of decriminalization on the size of the illicit drugs market. These suggest the effects on usage might be far larger with localized rather than nationalized decriminalization, for both cannabis and other illicit drugs. Hence such polices should be coordinated among drugs markets that are linked in terms of potential consumers and suppliers. Thinking about the illicit drugs market in London as a whole, an important remaining issue is to understand is whether local decriminalization only changes the *location* of drugs markets, but causes no overall change in the market size in London, or, whether local decriminalization increases the number of consumers as well as where they consume illicit drugs. The Hotelling-model we develop is silent on this issue because consumers only choose where to consume, not whether to consume or not. Such an extension forms part of our future research agenda.

The second important direction for future research follows up on the house price results that suggest the total social effects of decriminalization, such as alcohol use and visible anti-social behaviors, reduce the willingness to pay within these neighborhoods and increase within borough inequality in house prices between high and low crime zip codes. The importance of such channels needs to be investigated in future research.

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A Appendix (Not for Publication)

A.1 Proofs

Proof of Result 1a: The indirect mechanical effect operating through changes in drugs related police intensity in borough A is determined by totally differentiating (7),

$$\frac{dP_A^d}{d\delta_{d(A)}} = -\frac{\alpha_A \lambda_A}{6t(1-\lambda_A)e''(P-P_A^d) + \lambda_A e''(P_A^d)} > 0. \tag{14}$$

Given total police resources P are unchanged, police intensity related to other crime types, $P_A^o = P - P_A^d$, necessarily increases. The mechanical effect of decriminalization on drugs crime in borough A is,

$$\begin{aligned} \frac{dC_A^d}{d\delta_{d(A)}} &= D_A(.) + \delta_d \frac{\partial D_A}{\partial P_A^d}\frac{\partial P_A^d}{\partial \delta_{d(A)}} - e'(P_A^d)\frac{\partial P_A^d}{\partial \delta_{d(A)}} \\ &= D_A(.) - \left[\delta_{d(A)}\frac{\alpha_A}{6t} + e'(P_A^d)\right]\frac{\partial P_A^d}{\partial \delta_d}. \end{aligned} \tag{15}$$

The first term captures the positive direct effect, the second term captures the offsetting indirect effect. To see the effect on other crimes,

$$\begin{aligned} \frac{dC_A^o}{d\delta_{d(A)}} &= \delta_o \left[\frac{\partial D_A}{\partial P_A^d}\frac{\partial P_A^d}{\partial \delta_{d(A)}}\right] + e'(P-P_A^d)\frac{\partial P_A^d}{\partial \delta_{d(A)}} \\ &= \left[-\delta_o \frac{\alpha_A}{6t} + e'(P-P_A^d)\right]\frac{\partial P_A^d}{\partial \delta_{d(A)}}, \end{aligned} \tag{16}$$

which is of ambiguous sign. If $\delta_o = 0$ so there are no spillover effects of the drugs market to other crimes, then $\frac{dC_A^o}{d\delta_{d(A)}} > 0$ and the mechanical effect of decriminalization causes other crimes to fall.■

Proof of Result 1b: Totally differentiating (7) we see that a weakened incarceration effect in borough A due to decriminalization, changes drugs related policing intensity as follows,

$$\frac{dP_A^d}{d\alpha_A} = -\frac{\lambda_b \delta_{d(A)} + (1-\lambda_b)\delta_o}{6t(1-\lambda_A)e''(P-P_A^d) + \lambda_A e''(P_A^d)} > 0. \tag{17}$$

To calculate the overall incarceration effect of decriminalization, note that α_A has a direct effect on the equilibrium size of the drugs market in borough A as well as the above effect through police intensity,

$$\begin{aligned} \frac{dC_A^d}{d\alpha_A} &= \delta_{d(A)}\left[\frac{\partial D_A}{\partial \alpha_A} + \frac{\partial D_A}{\partial P_A^d}\frac{\partial P_A^d}{\partial \alpha_A}\right] - e'(P_A^d)\frac{\partial P_A^d}{\partial \alpha_A} \\ &= -\delta_{d(A)}\left[\frac{P_A^d}{6t} + \frac{\alpha_A}{6t}\frac{\partial P_A^d}{\partial \alpha_A}\right] - e'(P_A^d)\frac{\partial P_A^d}{\partial \alpha_A} < 0. \end{aligned} \tag{18}$$

Hence the incarceration effect leads to an unambiguous increase in drugs related crime in borough

A. To see the effect on other crimes,

$$\frac{dC_A^o}{d\alpha_A} = -\delta_o \left[\frac{P_A^d}{6t} + \frac{\alpha_A}{6t} \frac{\partial P_A^d}{\partial \alpha_A} \right] + e'(P - P_A^d) \frac{\partial P_A^d}{\partial \alpha_A}, \tag{19}$$

which is of ambiguous sign because the first term is negative and the second term is positive. In the extreme case of $\delta_o = 0$, $\frac{dC_A^o}{d\alpha_A} > 0$ and the incarceration effect, like the mechanical effect, causes other crimes to fall.■

Proof of Result 1c: Totally differentiating (7) we see that a weakened deterrence effect in borough A changes drugs related policing intensity in the same direction as the incarceration effect,

$$\frac{dP_A^d}{d\lambda_A} = -\frac{\alpha_A \left(\delta_{d(A)} - \delta_o \right) + [e'(P - P_A^d) + e'(P_A^d)]}{6t(1 - \lambda_A)e''(P - P_A^d) + \lambda_A e''(P_A^d)} > 0. \tag{20}$$

This channel only affects drugs crimes through the change in police intensity so,

$$\begin{aligned} \frac{dC_A^d}{d\lambda_A} &= \delta_{d(A)} \frac{\partial D_A}{\partial P_A^d} \frac{\partial P_A^d}{\partial \lambda_A} - e'(P_A^d) \frac{\partial P_A^d}{\partial \lambda_A} \\ &= \left[-\delta_{d(A)} \frac{\alpha_A}{6t} - e'(P_A^d) \right] \frac{\partial P_A^d}{\partial \lambda_A} < 0. \end{aligned}$$

Hence the deterrence effect leads to an unambiguous increase in drugs related crime in borough A. To see the effect on other crimes,

$$\frac{dC_A^o}{d\lambda_A} = \delta_o \left[\frac{\partial D_A}{\partial P_A^d} \frac{\partial P_A^d}{\partial \lambda_A} \right] + e'(P - P_A^d) \frac{\partial P_A^d}{\partial \lambda_A} \tag{21}$$

$$= \left[-\delta_o \frac{\alpha_A}{6t} + e'(P - P_A^d) \right] \frac{\partial P_A^d}{\partial \lambda_A}, \tag{22}$$

which is of ambiguous sign. In the extreme case where $\delta_o = 0$, $\frac{dC_A^o}{d\lambda_A} > 0$ and like the other effects, the deterrence effect causes other crimes to fall.■

Proof of Result 2: To see the effects of a local decriminalization in borough A on crime in borough B, we note first that from the first order condition (7), drugs related police intensity in B is independent of the key parameters in borough A: α_A, λ_A. Hence we see that (7) for borough B also does not depend on $\delta_{d(A)}$. Hence considering first the mechanical effect, the spillover effect on drugs crime in borough B is,

$$\frac{dC_B^d}{d\delta_{d(A)}} = \delta_{d(B)} \frac{\partial D_B}{\partial P_B^d} \frac{\partial P_B^d}{\partial \delta_{d(A)}} = 0, \tag{23}$$

because the final term is zero. In contrast, the incarceration and deterrence effects in borough A

do cause drugs tourism and have spillover effects on drugs crime in neighboring borough B,

$$\begin{aligned}\frac{dC_B^d}{d\alpha_A} &= \delta_{d(B)}\left[\frac{\partial D_B}{\partial \alpha_A}+\frac{\partial D_B}{\partial P_A^d}\frac{\partial P_A^d}{\partial \alpha_A}\right]=\delta_{d(B)}\frac{P_A^d}{6t}>0, \\ \frac{dC_B^d}{d\lambda_A} &= \delta_{d(B)}\left[\frac{\partial D_B}{\partial P_A^d}\frac{\partial P_A^d}{\partial \lambda_A}\right]=\delta_{d(B)}\left[\frac{\alpha_A}{6t}\frac{\partial P_A^d}{\partial \lambda_A}\right]>0.\end{aligned} \tag{24}$$

Hence drugs crimes are reduced in the neighboring borough, through changes in the equilibrium market sizes for drugs across boroughs. Similarly, there can be spillover effects onto other crimes in the neighboring borough, through the incarceration and deterrence effects,

$$\begin{aligned}\frac{dC_B^o}{d\alpha_A} &= \delta_o\left[\frac{\partial D_B}{\partial \alpha_A}+\frac{\partial D_B}{\partial P_A^d}\frac{\partial P_A^d}{\partial \alpha_A}\right]>0, \\ \frac{dC_B^o}{d\lambda_A} &= \delta_o\left[\frac{\partial D_B}{\partial P_A^d}\frac{\partial P_A^d}{\partial \lambda_A}\right]>0.\blacksquare\end{aligned} \tag{25}$$

Proof of Result 3: A nationwide decriminalization policy sets $\delta_{d(b)}$, α_b, and λ_b to be the same across boroughs. With such symmetry, the segment of consumers of unit length are shared equally across the drugs suppliers located in each borough so $D_b = \frac{1}{2}$. Hence only the mechanical effect of decriminalization has any impact on drugs crimes as $C_b^d = \frac{\delta_d}{2}$. However, through the first order condition (7) we see that even with a nationwide decriminalization, $\frac{dP_b^d}{d\delta_d} > 0$, $\frac{dP_b^d}{d\alpha_b} > 0$, and $\frac{dP_b^d}{d\lambda_b} > 0$ so drugs policing intensity falls, police effort is reallocated towards other crimes, and other crimes unambiguously fall through each of the mechanical, incarceration and deterrence channels.■

A.2 Robustness Checks on Baseline Results

Table A2 presents a series of robustness checks on these baseline results. We address three classes of concern. First, we show the results to be robust to controlling for linear or cubic within borough time trends (Columns 1 and 2). As might be expected, the policy effects are less precisely estimated with such specifications, although they remain significantly different from zero. Moreover, there remains little evidence of London-wide changes in drug related offences during the policy and post-policy periods in these specifications. Column 3 restricts 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 second class of concerns relate to the measurement of crime [Pepper *et al* 2009]. Column 4 drops borough-months in which zero drugs crimes are reported, in case these reflect measurement error rather than literally no such crimes taking place. Column 5 uses data from April 1998, the

date from which we have an unbroken data series for drugs offences. Column 6 uses data from October 1994. Data prior to this time was recorded using a different set of Home Office sub-offence type codes. The baseline results are robust to each such alteration of the sample.

The final two columns present falsification checks. In Column 7 we define a placebo policy that is switched on a year prior to the actual LCWS. Reassuringly, we find such a placebo policy to have no correlation to drug crime rates during the placebo policy period. As expected, the interaction of the post-policy placebo with the Lambeth dummy is positive and significant because it picks up some of the true post-policy effects of the LCWS. The second falsification exercise examines the effect of the LCWS on another London borough – Camden. Camden has the second highest crime rates for all crimes, and for drugs crime in particular. *A priori*, this borough also shares some of the socioeconomic features of Lambeth. Reassuringly, we find no effect of the LCWS on Camden, suggesting the previously documented effects are not just picking up changes in drugs crime that would have occurred in the absence of the LCWS is some London boroughs.

A.3 Thought Experiments on the Illicit Drugs Market

We conduct a series of thought experiments to understand what would need to be true for the market for cannabis and non-cannabis drugs in order to explain our baseline estimate of the policy causing a significant long run increase of 20% in offences related to drugs possession (Table 2, Column 6). We proceed as follows. We first use the individual crime data available from NDAD prior to March 1997. These individual crime records include detailed information on the drugs related offence by: (i) drug type, e.g. cannabis, cocaine etc., (ii) the nature of the offence, e.g. possession, possession with intent to supply, trafficking, etc.; (iii) the borough and date on which the offence is recorded.

Table A4 presents some of these statistics for Lambeth and all other London boroughs, using the last year of available data from April 1996 to March 1997. On offences by drug type, 71% of drug offences relate to cannabis in Lambeth in 1996/7, slightly lower than in other boroughs. Among cannabis related offences, over 91% of these offences relate to the possession of cannabis. The next largest category of cannabis offences relate to possession with the intent to supply, which constitute 4.5% of all cannabis offences in Lambeth in 1996/7. Hence while Lambeth differs from all other boroughs in terms of drug offence rates, as has been noted earlier, there are somewhat smaller differences in terms of the composition of drug offences by drug type. Finally, among cannabis offences, there are no significant differences between Lambeth and other boroughs in terms of the type of cannabis related offences committed.

As a second step, we construct the same statistics for the first year in the NDAD for which this is feasible – April 1994 to March 1995. We find no robust evidence of time trends in the composition of drug offences by drug type, nor in the composition of cannabis offences by offence type. We therefore extrapolate from the NDAD sample, assuming the shares reported for Lambeth in Table A4 remain unchanged on the eve of the introduction of the LCWS in June 2001.

As a final step, we then use the relevant statistics from Table A4 for Lambeth to understand

what, under each scenario discussed at the start of this subsection, would have to be these case to explain the observed long run effect of the LCWS increasing recorded offences for the possession of drugs by 20% (Column 6, Table 2). More precisely, let $possession_0^C$ denote the total number of offences for the possession of cannabis pre-policy, and let $possession_1^C$ denote the total number of offences for possession of cannabis drugs post-policy. Let $possession_0^{NC}$ and $possession_1^{NC}$ denote the corresponding numbers of offences for non-cannabis possession. Assuming no population changes within Lambeth over time, the estimate of the long run policy effect in Lambeth, $\widehat{\beta}_3$ in specification (9), corresponds to,

$$\widehat{\beta}_3 = \frac{\left(possession_1^{NC} + possession_1^C\right) - \left(possession_0^{NC} + possession_0^C\right)}{possession_0^{NC} + possession_0^C} = .2. \tag{26}$$

We now use the relevant statistics from Table A4 to substitute into (26) under various scenarios.

A.3.1 Scenario 1: Increase Only in Non-cannabis Related Crimes

Assume first that the observed increase in drug possession offences is entirely driven by an increase in offences related to non-cannabis possessions *and* that recorded offences of cannabis possessions post-policy fall to zero ($possession_1^C = 0$). If so, then re-arranging (26) and substituting in for the ratio of cannabis to non-cannabis possession offences pre-period from Table A4, we see that,

$$\frac{possession_1^{NC} - possession_0^{NC}}{possession_0^{NC}} = \widehat{\beta}_3 + \left(1 + \widehat{\beta}_3\right) \frac{possession_0^C}{possession_0^{NC}} = .2 + 1.2 \times (4.01) = 5.01. \tag{27}$$

Hence under this scenario, only a five-fold increase in non-cannabis possessions could explain the long run policy response. Intuitively, the increase in possession of other drug types has to increase so dramatically because the majority of drug offences relates to cannabis possession in Lambeth (65.2%) and it is assumed that the policy's introduction causes all such offences of cannabis possession to no longer be recorded as an offence.

A.3.2 Scenario 2: Policy *De Facto* Already in Place

A second scenario is one in which the policy was *de facto* already in place so that all the effects essentially are explained only through the incarceration effect as drug consumers respond to the policy announcement. Under this scenario, there is no change in recorded cannabis offences by the police ($possession_1^C = possession_0^C$). We then find the documented increase in drug possession offences can be explained by changes in the possession of other drugs types if,

$$\frac{possession_1^{NC} - possession_0^{NC}}{possession_0^{NC}} = \widehat{\beta}_3 \left(1 + \frac{possession_0^C}{possession_0^{NC}}\right) = .2 \times (1 + 4.01) = 1, \tag{28}$$

Namely, we still require a near doubling in non-cannabis possessions, even if policing strategies related to cannabis possession were *de facto* implemented before the policy in Lambeth.

A.3.3 Scenario 3: Increase Only in Cannabis Related Crime

In the final scenario we suppose that all the increase in drug possession offences is driven by an increase in offences related to cannabis possessions, say because of increases in quantities possessed. Therefore there is no change in non-cannabis possessions ($possession_1^{NC} = possession_0^{NC}$) so the required change in cannabis possessions to explain the results is,

$$\frac{possession_1^C - possession_0^C}{possession_0^C} = \widehat{\beta}_3 \left(1 + \frac{possession_0^{NC}}{possession_0^C}\right) = .2 \times (1 + \frac{1}{4.01}) = .25. \qquad (29)$$

Hence a 25% increase in cannabis possessions from below the threshold for personnel use to above would explain our findings.[38]

[38] This conclusion is unlikely to be changed by making marginal adjustments to the formulae above to account for population changes in Lambeth over time, or to use the NDAD data between 1994 and 1997 to extrapolate the relevant statistics to 2001.

Table 1: Recorded Drugs Offences Across London Boroughs

Means and standard errors in parentheses of total recorded drugs offences per 1000 of adult population, by borough and period

	Total recorded drugs offences, per 1000 of adult population			Change in drugs offences for this borough/counterfactual compared to ITSELF	
	(1) Pre-Policy Period	(2) Policy Period	(3) Post-Policy Period	(4) Pre-policy vs. policy period	(5) Policy vs. post-policy period
Lambeth	.425	.830	1.133	.405***	.303***
	(.024)	(.100)	(.028)	(.077)	(.075)
London (excluding Lambeth)	.284	.395	.488	.111***	.093***
	(.005)	(.014)	(.010)	(.016)	(.020)
Neighbours to Lambeth	.422	.481	.661	.059	.180**
	(.021)	(.047)	(.041)	(.064)	(.079)

Notes: The pre-policy period runs from January 1990 until June 2001. The policy period covers July 2001 until July 2002. The post-policy period extends from August 2002 until January 2006. For the columns detailing offence means, we report borough-period means and standard errors in parentheses. Neighbours to Lambeth are Croydon, Merton, Southwark, Wandsworth and Westminster.

Table 2: The Effect of The Policy on Recorded Drugs Offences

Dependent Variable: Log (total recorded drugs offences, per 1000 of adult population)
Panel Regression Estimates

	(1) Prais-Winsten Panel Specific AR(1)	(2) Borough Specific Quadratic Time Trend	(3) Police Operations	(4) 2-Part Policy Period	(5) Dynamic Response	(6) Possession of Drugs	(7) Drug Trafficking	(8) Possession of Drugs	(9) Possession of Drugs
Lambeth x Policy Period	.152***	.106**	.124***		.121***	.097**	.002		.095**
	(.038)	(.043)	(.038)		(.035)	(.046)	(.019)		(.042)
Policy Period	.008	.001	.007	.012	.011	-.016	.003	-.012	-.009
	(.021)	(.020)	(.020)	(.020)	(.020)	(.013)	(.006)	(.013)	(.013)
Lambeth x Post-Policy Period	.258***	.205***	.233***	.241***		.200***	.010	.216***	
	(.027)	(.058)	(.027)	(.025)		(.039)	(.015)	(.031)	
Post-Policy Period	-.003	-.012	.001	.009		-.014	-.003	-.005	
	(.029)	(.029)	(.029)	(.029)		(.020)	(.010)	(.020)	
Lambeth x Policy Period [1-6 months]				.028				.004	
				(.046)				(.046)	
Lambeth x Policy Period [7-13 months]				.205***				.188***	
				(.043)				(.046)	
Lambeth x Post-Policy Period [1-12 months]					.227***				.173***
					(.037)				(.044)
Lambeth x Post-Policy Period [13-24 months]					.293***				.255***
					(.038)				(.044)
Lambeth x Post-Policy Period [25-36 months]					.224***				.216***
					(.038)				(.045)
Lambeth x Post-Policy Period [37-48 months]					.155***				.156***
					(.053)				(.057)
Share of All Offences in Pre-policy Period						.841	.144	.841	.841
Short and Long Run Policy Effects are Equal in Lambeth [p-value]	[.010]	[.033]	[.007]			[.011]	[.639]		
First and Second Half Policy Effects are Equal in Lambeth [p-value]				[.001]				[.000]	
First and Fourth Year Post Policy Effects are Equal in Lambeth [p-value]					[.241]				[.770]
Borough, Month, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.750	.822	.758	.761	.765	.856	.444	.862	.862
Observations	4055	4055	4055	4055	4055	1883	1883	1883	1883

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. In Columns 1 to 5 the sample period runs from April 1992 until January 2006. There is a 12 month gap from April 1997 to March 1998, as drug offence data was not available for this period. In Columns 6 to 9, data on sub-categories of drug offence are only available from January 2000 onwards. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark, Wandsworth and Westminster). 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 (excluding neighbouring boroughs) 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 socio-demographic control variables, measured in logs at the borough-month-year level, are 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, and aged above 50 and the male unemployment rate. We also control for a series of dummies covering the time periods from which the underlying time series for offences are recovered from. 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). At the foot of the columns we report the p-value on the null hypothesis that the coefficients on the Lambeth x Policy Period and Lambeth x Post-Policy Period interactions are equal; the p-value on the null hypothesis that the coefficients on the Lambeth x Policy Period [1-6 months] Lambeth x Policy Period [7-13 months] interactions are equal; the p-value on the null hypothesis that the coefficients on the Lambeth x Post-Policy Period [1-12 months] and Lambeth x Post-Policy Period [37-48 months] interactions are equal.

Table 3: The Effect of The Policy on Arrest and Clear Up Rates for Drugs

Prais-Winsten Regression Estimates

	Log (recorded arrests of a given drug category, per offence)				Log (recorded clear ups of a given drug category, per offence)			
	(1) Drugs Total (Full Sample)	(2) Drugs Total (Restricted Sample)	(3) Possession of Drugs	(4) Drug Trafficking	(5) Drugs Total (Full Sample)	(6) Drugs Total (Restricted Sample)	(7) Possession of Drugs	(8) Drug Trafficking
Lambeth x Policy Period	-.304***	-.293***	-.403***	-.044	-.336***	-.309***	-.412***	-.048
	(.060)	(.070)	(.088)	(.051)	(.060)	(.082)	(.100)	(.077)
Policy Period	-.008	-.002	.002	-.001	.023	-.055**	-.057**	-.023
	(.021)	(.023)	(.024)	(.040)	(.046)	(.027)	(.028)	(.044)
Lambeth x Post-Policy Period	-.152***	-.142**	-.198**	-.039	-.178***	-.139**	-.182**	-.041
	(.048)	(.062)	(.083)	(.040)	(.043)	(.069)	(.088)	(.063)
Post-Policy Period	-.011	.004	.003	.038	.058	-.086*	-.089*	-.042
	(.033)	(.036)	(.039)	(.064)	(.071)	(.045)	(.045)	(.073)
Share of All Arrests, or Clear ups, in Pre-policy Period		-	.839	.149	-	-	.841	.142
Short and Long Run Policy Effects are Equal in Lambeth [p-value]	[.010]	[.015]	[.009]	[.928]	[.018]	[.022]	[.012]	[.914]
Borough, Month, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.809	.801	.844	.121	.428	.103	.638	-.503
Observations	2443	1883	1883	1883	3790	1614	1614	1614

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. Data on sub-categories of drug offence are only available from January 2000 onwards. The full sample for the arrests per offence series runs from April 1998 to January 2006. For clear-ups per offence, the series runs from April 1992 until March 2005. There is a break in this series due to the offence series (the denominator) not being available from April 1997 to March 1998. The dependent variable in Columns 1 to 4 is the log of the number of recorded arrests divided by the average of offences in the current month and the previous three months in the same borough. The dependent variable in Columns 5 to 8 is the log of the number of recorded clear ups divided by the average of offences in the current month and the previous three months. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark, Wandsworth and Westminster). 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 (excluding neighbouring boroughs) 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 share of all accused (clear-ups) in the pre-policy period is the average ratio of the drug arrests (clear-ups) type to all drug arrests (clear-ups) in the period from January 2000 until June 2001. The socio-demographic control variables, measured in logs at the borough-month-year level, are the total population aged 16 and above, 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, and aged above 50 and the male unemployment rate. We also control for a series of dummies covering the time periods from which the underlying time series for offences are recovered from. 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). At the foot of each column we report the p-value on the null hypothesis that the coefficients on the Lambeth x Policy Period and Lambeth x Post-Policy Period interactions are equal.

Table 4: The Effect of The Policy on Other Crime Types

Dependent Variable: Log (recorded offences of a given type, per 1000 of adult population)
Prais-Winsten Regression Estimates

	(1) Total (with drugs)	(2) Total (without drugs)	(3) Violence Against the Person	(4) Sexual Offences	(5) Robbery	(6) Burglary	(7) Theft and Handling	(8) Fraud or Forgery	(9) Criminal Damage
Lambeth x Policy Period	.028	.021	-.021	-.049	.134***	.003	.092***	-.050	-.026
	(.027)	(.028)	(.036)	(.036)	(.045)	(.045)	(.033)	(.046)	(.026)
Policy Period	.074**	.073**	.010	.012	.018	.061**	.090***	.000	.020
	(.033)	(.032)	(.034)	(.008)	(.016)	(.028)	(.033)	(.029)	(.022)
Lambeth x Post-Policy Period	-.084***	-.107***	-.052*	-.056**	-.064*	-.192***	-.010	-.142***	-.063***
	(.021)	(.022)	(.027)	(.027)	(.035)	(.035)	(.026)	(.033)	(.019)
Post-Policy Period	.107**	.108**	.046	.024**	-.006	.064	.141***	-.007	.017
	(.046)	(.045)	(.048)	(.012)	(.023)	(.040)	(.046)	(.041)	(.030)
Share of All Offences in Pre-policy Period	-	-	.161	.009	.040	.122	.397	.086	.163
Short and Long Run Policy Effects are Equal in Lambeth [p-value]	[.000]	[.000]	[.411]	[.853]	[.000]	[.000]	[.004]	[.061]	[.200]
Borough, Month, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.994	.994	.964	.659	.877	.948	.988	.874	.973
Observations	4055	4055	4055	4055	4055	4055	4055	4055	4055

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. The sample period considered here is the same as that used in the drugs offence specification, running from April 1992 until January 2006 with a 12 month gap from April 1997 to March 1998. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark, Wandsworth and Westminster). 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 (excluding neighbouring boroughs) 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 socio-demographic control variables, measured in logs at the borough-month-year level, are 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, and aged above 50 and the male unemployment rate. We also control for a series of dummies covering the time periods from which the underlying time series for offences are recovered from. 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). The share of all offences in the pre-policy period is the average ratio of the offence type to total offences in the period from January 2000 until June 2001. At the foot of each column we report the p-value on the null hypothesis that the coefficients on the Lambeth x Policy Period and Lambeth x Post-Policy Period interactions are equal.

Table 5: The Effect of The Policy on Other Crime Types

Prais-Winsten Regression Estimates

A. Dependent Variable: Log (recorded arrests of a given crime category, per offence)

	(1) Total (with drugs)	(2) Total (without drugs)	(3) Violence Against the Person	(4) Sexual Offences	(5) Robbery	(6) Burglary	(7) Theft and Handling	(8) Fraud or Forgery	(9) Criminal Damage
Lambeth x Policy Period	-.020***	-.005	.004	.007	.026*	-.014	-.006	-.005	-.011
	(.007)	(.006)	(.011)	(.039)	(.014)	(.010)	(.004)	(.012)	(.007)
Policy Period	-.001	-.001	.006	.025*	-.008	.002	-.002	.006	-.007
	(.006)	(.005)	(.011)	(.014)	(.012)	(.006)	(.004)	(.007)	(.004)
Lambeth x Post-Policy Period	.008*	.011***	.027***	.075***	.029***	.022***	-.000	.023***	.001
	(.005)	(.004)	(.008)	(.027)	(.011)	(.008)	(.003)	(.008)	(.005)
Post-Policy Period	-.020**	-.017**	-.008	.026	-.023	-.011	-.018***	-.003	-.023***
	(.009)	(.008)	(.016)	(.022)	(.019)	(.010)	(.006)	(.010)	(.007)
Share of All Arrests in Pre-policy Period	-	-	.222	.015	.047	.079	.330	.047	.092
Short and Long Run Policy Effects are Equal in Lambeth [p-value]	[.000]	[.004]	[.047]	[.082]	[.798]	[.000]	[.147]	[.019]	[.109]
Borough, Month, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.920	.916	.860	.199	.421	.605	.894	.572	.649
Observations	2443	2443	2443	2443	2443	2443	2443	2443	2443

B. Dependent Variable: Log (clear ups of a given crime category, per offence)

	(1) Total (with drugs)	(2) Total (without drugs)	(3) Violence Against the Person	(4) Sexual Offences	(5) Robbery	(6) Burglary	(7) Theft and Handling	(8) Fraud or Forgery	(9) Criminal Damage
Lambeth x Policy Period	-.040**	-.028*	-.037	-.092	-.028	-.041	-.019	-.028	-.019
	(.016)	(.015)	(.024)	(.060)	(.021)	(.028)	(.013)	(.027)	(.014)
Policy Period	.008	.008	.054*	.083**	.015	.013	.001	.026	-.002
	(.015)	(.014)	(.032)	(.034)	(.016)	(.015)	(.015)	(.022)	(.013)
Lambeth x Post-Policy Period	-.015	-.017	-.008	-.058	-.025	-.001	-.019	-.031	-.011
	(.013)	(.013)	(.019)	(.043)	(.016)	(.021)	(.012)	(.020)	(.012)
Post-Policy Period	.003	.006	.100**	.175***	.036	.008	-.004	.069**	-.008
	(.022)	(.021)	(.047)	(.051)	(.023)	(.022)	(.022)	(.033)	(.019)
Share of All Clear-Ups in Pre-policy Period	-	-	.294	.018	.034	.082	.275	.051	.099
Short and Long Run Policy Effects are Equal in Lambeth [p-value]	[.149]	[.515]	[.277]	[.610]	[.911]	[.196]	[.964]	[.919]	[.622]
Borough, Month, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.694	.662	.858	.415	.409	.433	.577	.640	.524
Observations	3790	3790	3790	3789	3789	3787	3786	3789	3786

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. The dependent variable in Panel A is the log of the number of recorded arrests divided by the average of offences in the current month and the previous three months in the same borough. The sample period considered in this panel is the same as that used in the drugs arrests per offence specification and runs from April 1998 to January 2006.The dependent variable in Panel B is the log of the number of recorded clear ups divided by the average of offences in the current month and the previous three months. The sample period considered in this panel is the same as that used in the drugs clear-ups per offence specification and runs from April 1992 until March 2005, with a break due to missing drugs offence data from April 1997 to March 1998. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark, Wandsworth and Westminster). 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 (excluding neighbouring boroughs) 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 share of all accused (clear-ups) in the pre-policy period is the average ratio of the accused (clear-ups) type to total accused (clear-ups) in the period from January 2000 until June 2001. The socio-demographic control variables, measured in logs at the borough-month-year level, are the total population aged 16 and above, 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, and aged above 50 and the male unemployment rate. We also control for a series of dummies covering the time periods from which the underlying time series for offences are recovered from. 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). At the foot of each column we report the p-value on the null hypothesis that the coefficients on the Lambeth x Policy Period and Lambeth x Post-Policy Period interactions are equal.

Table 6: Drug Related Crime in Lambeth and its Neighboring Boroughs

Panel Regression Estimates

	Offences				Arrests		Clear-ups	
	(1)Drugs Total	(2) Drugs Total by Neighbor	(3) Possession of Drugs	(4) Drug Trafficking	(5) Drugs	(6) All Non Drugs	(7) Drugs	(8) All Non Drugs
Lambeth x Policy Period	.127***	.128***	.103**	.005	-.298***	-.004	-.334***	-.027*
	(.037)	(.038)	(.046)	(.019)	(.060)	(.006)	(.061)	(.015)
Policy Period	.012	.010	-.019	.006	-.009	-.000	.018	.006
	(.021)	(.021)	(.013)	(.006)	(.021)	(.005)	(.045)	(.013)
Lambeth x Post-Policy Period	.237***	.237***	.205***	.012	-.151***	.012***	-.173***	-.015
	(.027)	(.027)	(.039)	(.015)	(.048)	(.004)	(.044)	(.012)
Post-Policy Period	.007	.003	-.024	.001	-.013	-.017**	.050	.001
	(.030)	(.030)	(.019)	(.009)	(.032)	(.008)	(.068)	(.018)
Neighboring Borough x Policy Period	-.043**				.000	-.006**	-.015	-.004
	(.021)				(.015)	(.003)	(.030)	(.006)
Neighboring Borough x Post-Policy Period	-.031*				-.016	-.004*	-.005	-.001
	(.017)				(.010)	(.002)	(.021)	(.004)
Croydon x Policy Period		-.051**	-.017	-.010				
		(.024)	(.027)	(.013)				
Croydon x Post-Policy Period		-.056***	-.053**	-.007				
		(.019)	(.024)	(.010)				
Merton x Policy Period		-.089***	-.056***	-.018***				
		(.023)	(.020)	(.006)				
Merton x Post-Policy Period		-.083***	-.059***	-.011**				
		(.019)	(.017)	(.005)				
Southwark x Policy Period		-.084	-.099**	-.051***				
		(.064)	(.040)	(.018)				
Southwark x Post-Policy Period		-.036	-.012	-.006				
		(.052)	(.032)	(.014)				
Wandsworth x Policy Period		.017	.053*	.001				
		(.050)	(.028)	(.013)				
Wandsworth x Post-Policy Period		-.037	.016	-.003				
		(.042)	(.022)	(.010)				
Westminster x Policy Period		.040	-.013	.003				
		(.084)	(.063)	(.027)				
Westminster x Post-Policy Period		.173*	.105	.010				
		(.090)	(.064)	(.024)				
t-test: Sum of Neighbors Effects = Lambeth Effect, Policy Period [p-value]		[.015]	[.008]	[.076]				
t-test: Sum of Neighbors Effects = Lambeth Effect, Post-Policy Period [p-value]		[.015]	[.009]	[.443]				
Borough, Month, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.738	.742	.862	.462	.815	.922	.424	.721
Observations	4810	4810	2233	2233	2898	2898	4491	4491

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. The sample period runs from April 1992 until January 2006. There is a 12 month gap from April 1997 to March 1998 for drug offences as this data was not available for this period. 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 socio-demographic control variables, measured in logs at the borough-month-year level, are 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, and aged above 50 and the male unemployment rate. We also control for a series of dummies covering the time periods from which the underlying time series for offences are recovered from. 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). The boroughs defined to be neighboring Lambeth are Croydon, Merton, Southwark, Wandsworth, and Westminster.

Table 7: The Nationwide Decriminalization of Cannabis

Prais-Winsten Regression Estimates

	Offences		Arrests		Clear-ups	
	(1) Drugs, Time Trends	(2) All Non-Drug Crime Types	(3) Drugs	(4) All Other Crime Types	(5) Drugs	(6) All Other Crime Types
General Equilibrium Policy Period	-.038	.036*	-.049**	.008*	.016	.018
	(.031)	(.021)	(.020)	(.004)	(.026)	(.011)
Borough and Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Aggregate Quadratic Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.834	.995	.845	.908	.234	.862
Observations	3154	3154	2722	2722	1940	1940

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. The sample period runs from January 1999 until December 2008. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark, Wandsworth and Westminster). 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. Offence variables are the log of the number of recorded offences per 1000 adult borough population. Arrest rates are the log of the number of recorded accused divided by the average of offences in the current month and the previous three months in the same borough. Clear-up rates are calculated as the log of the number of recorded clear ups divided by the average of offences in the current month and the previous three months. Observations are weighted by the share of the total (excluding neighbouring boroughs) London population that month-year in the borough. Cannabis was legally declassified to Class C status on 29 January 2004, and then reclassified to Class B status on 26 January 2009. The general equilibrium policy period dummy variable is equal to one from January 2004 until December 2008, and zero otherwise. The socio-demographic control variables, measured in logs at the borough-month-year level, are 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, and aged above 50 and the male unemployment rate. These variables, in addition to the total population aged 16 and above are only available until January 2006. From February 2006 onwards, we extrapolated these series using a borough specific linear time-trend, where the trend was based on the full sample period. It should be noted that adult population is used as the denominator in the offence data series, as well as in constructing the regression weights. In Column 2 onwards we include a quadratic time trend. 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), Operation Solstice (01/12/2003-08/12/2003) and Operation Theseus (7/7/2005-17/08/2005). Lambeth was part of Safer Streets Phase 1 and 2, and Blunt 1. Further operations (part 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). Not part of Operation Windmill was Operation Alnwick (Haringey: 16/09/2002-13/10/2002).

Table 8: The Effect of The Policy on House Prices

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

	(1) PC Sector Clustered SEs	(2) Borough Specific Linear Time Trend	(3) Drug Crime Hotspot	(4) Flats and Maisonettes	(5) Drug Crime Hotspot, with Neighbors
Lambeth x Policy Period	.015	-.038*	.014	.031	.008
	(.014)	(.021)	(.039)	(.029)	(.038)
Policy Period	-.015*	-.014*	-.043***	-.048***	-.038***
	(.008)	(.008)	(.013)	(.017)	(.013)
Lambeth x Post-Policy Period	-.061***	-.138***	-.021	.070**	-.035
	(.018)	(.037)	(.033)	(.030)	(.032)
Post-Policy Period	-.011	-.009	-.077***	-.111***	-.064***
	(.013)	(.013)	(.018)	(.019)	(.018)
Lambeth x Hotspot			.141***	.308***	.647***
			(.035)	(.046)	(.055)
Hotspot			-.590***	.084***	-.671***
			(.027)	(.021)	(.048)
Lambeth x Policy Period x Hotspot			-.062*	-.056*	-.062*
			(.036)	(.033)	(.036)
Policy Period x Hotspot			.033***	.037**	.034***
			(.012)	(.016)	(.012)
Lambeth x Post-Policy Period x Hotspot			-.139***	-.180***	-.137***
			(.023)	(.030)	(.023)
Post-Policy Period x Hotspot			.076***	.078***	.076***
			(.016)	(.017)	(.016)
Neighbor x Policy Period					-.064***
					(.023)
Neighbor x Post-Policy Period					-.164***
					(.032)
Neighbor x Hotspot					.692***
					(.036)
Neighbor x Policy Period x Hotspot					-.009
					(.024)
Neighbor x Post-Policy Period x Hotspot					-.009
					(.029)
Zip code-sector, Quarter, Year FEs	Yes	Yes	Yes	Yes	Yes
R-squared	.947	.952	.953	.934	.952
Observations	14018	14018	14018	16431	17336

Notes: *** 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. Lambeth's neighbouring boroughs were excluded from the analysis in Columns 1 to 4. Hotspots were created based on the total drug offences, namely if the ward was equal to or above the median within the borough. 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 socio-demographic control variables, lagged by one quarter and measured in logs at the borough-quarter-year level, are 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, and aged above 50 and the male unemployment rate. 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), Operation Solstice (01/12/2003-08/12/2003) and Operation Theseus (7/7/2005-17/08/2005). Lambeth was part of Safer Streets Phase 1 and 2, and Blunt 1. Further operations (part 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). Not part of Operation Windmill was Operation Alnwick (Haringey: 16/09/2002-13/10/2002). All of these police operation markers are also lagged by one quarter.

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	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	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	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	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	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	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	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	Blunt	All London	12/2005	Unknown	Knife Crime	http://www.mpa.gov.uk/committees/mpa/2005/050526/10/	
Planning, Performance & Review Committee reports archive	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	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	

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

Table A2: Robustness Checks on the Effect of The Policy on Recorded Drugs Offences

Dependent Variable: Log (total recorded drugs offences, per 1000 of adult population)
Panel Regression Estimates

	(1) Borough Specific Linear Time Trend	(2) Borough Specific Cubic Time Trend	(3) Short Time-Frame	(4) Positive Values	(5) Continuous Time Series	(6) Consistent Home Office Drugs Sub-Offence Codes	(7) Placebo Policy Timing	(8) Camden
Lambeth x Policy Period	.079*	.090**	.110**	.126***	.084**	.115***		
	(.043)	(.046)	(.052)	(.040)	(.039)	(.035)		
Policy Period	.011	.017	-.034***	.007	-.004	.012		.012
	(.020)	(.020)	(.012)	(.021)	(.014)	(.018)		(.021)
Lambeth x Post-Policy Period	.130***	.191***	.229***	.238***	.186***	.216***		
	(.043)	(.058)	(.055)	(.029)	(.029)	(.025)		
Post-Policy Period	.010	.015	-.053**	.001	-.004	.023		.011
	(.028)	(.029)	(.021)	(.030)	(.021)	(.026)		(.029)
Lambeth x Placebo Policy Period							.026	
							(.042)	
Placebo Policy Period							-.011	
							(.022)	
Lambeth x Placebo Post-Policy Period							.204***	
							(.027)	
Placebo Post-Policy Period							-.005	
							(.030)	
Camden x Policy Period								.075
								(.061)
Camden x Post-Policy Period								.049
								(.059)
Short and Long Run Policy Effects are Equal in Lambeth [p-value]	[.212]	[.031]	[.031]	[.009]	[.008]	[.008]	[.000]	[.681]
Borough, Month, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.798	.849	.887	.760	.852	.827	.753	.729
Observations	4055	4055	971	3953	2417	3245	4055	4055

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. Unless otherwise specified, the sample period runs from April 1992 until January 2006. There is a 12 month gap from April 1997 to March 1998, as drug offence data was not available for this period. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark, Wandsworth and Westminster). 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 (excluding neighbouring boroughs) 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 socio-demographic control variables, measured in logs at the borough-month-year level, are 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, and aged above 50 and the male unemployment rate. We also control for a series of dummies covering the time periods from which the underlying time series for offences are recovered. 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). Column 3 restricts the data to observations within a 12 month window around the policy, that is July 2000 to July 2003. The sample falls in Column 4 because we drop observations in which there are no recorded drugs offences in the borough-month. Column 5 only uses data from April 1998, the date from which we have an unbroken data series for drugs offences. Column 6 only uses data from October 1994. Data prior to this time was recorded using a different set of Home Office sub-offence type codes. In Column 7 the placebo policy period is defined to be one year prior to the actual policy. At the foot of each column we report the p-value on the null hypothesis that the coefficients on the Lambeth x Policy Period and Lambeth x Post-Policy Period interactions are equal.

Table A3: The Effect of The Policy on Arrest and Clear Up Rates for Drugs

Prais-Winsten Regression Estimates

	Log recorded arrests of a given drug category, per 1000 adult population					Log recorded clear ups of a given drug category, per 1000 adult population				
	(1) Drugs Total (Full Sample)	(2) Drugs Total (Restricted Sample)	(3) Possession of Drugs	(4) Drug Trafficking	(5) Other Drug Offences	(6) Drugs Total (Full Sample)	(7) Drugs Total (Restricted Sample)	(8) Possession of Drugs	(9) Drug Trafficking	(10) Other Drug Offences
Lambeth x Policy Period	-.259***	-.238***	-.263***	-.016	.002	-.323***	-.268***	-.293***	-.016	-.000
	(.061)	(.070)	(.076)	(.011)	(.002)	(.064)	(.103)	(.110)	(.017)	(.002)
Policy Period	.005	.005	.005	-.004	-.001	.004	-.024	-.022	-.006	-.000
	(.013)	(.013)	(.012)	(.005)	(.001)	(.017)	(.016)	(.014)	(.006)	(.001)
Lambeth x Post-Policy Period	-.259***	-.213***	-.222***	-.023***	-.001	-.354***	-.249***	-.253**	-.023*	-.002
	(.051)	(.064)	(.071)	(.009)	(.002)	(.049)	(.091)	(.098)	(.014)	(.002)
Post-Policy Period	.009	.012	.006	-.000	-.002**	.003	-.039	-.038	-.009	-.002
	(.020)	(.021)	(.019)	(.007)	(.001)	(.025)	(.026)	(.024)	(.009)	(.001)
Share of All Accused, or Clear ups, in Pre-policy Period	-	-	.839	.149	.010	-	-	.841	.142	.011
Short and Long Run Policy Effects are Equal in Lambeth [p-value]	[.995]	[.687]	[.548]	[.467]	[.105]	[.664]	[.834]	[.689]	[.658]	[.327]
Borough, Month, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.858	.837	.836	.505	.530	.871	.803	.816	.482	.567
Observations	2443	1883	1883	1883	1883	3790	1614	1614	1614	1614

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. Data on sub-categories of drug offence are only available from January 2000 onwards. The dependent variable in Columns 1 to 5 is the log of the number of recorded arrests per 1000 total population aged 16 and above in the same borough. The dependent variable in Columns 6 to 10 is the log of the number of recorded clear ups per 1000 total population aged 16 and above. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark, Wandsworth and Westminster). 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 (excluding neighbouring boroughs) 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 share of all accused (clear-ups) in the pre-policy period is the average ratio of the drug accused (clear-ups) type to all drug accused (clear-ups) in the period from January 2000 until June 2001. The socio-demographic control variables, measured in logs at the borough-month-year level, are 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, and aged above 50 and the male unemployment rate. Additionally, we control for the average of offences in the current month and the previous three months. We also control for a series of dummies covering the time periods from which the underlying time series for offences are recovered. 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). At the foot of each column we report the p-value on the null hypothesis that the coefficients on the Lambeth x Policy Period and Lambeth x Post-Policy Period interactions are equal.

Table A4: Detailed Drugs Offences, Pre-policy Period

Means and standard errors in parentheses

	Lambeth	Control Boroughs	Difference [p-value]
Total drugs offences per 1000 of adult population	.537 (.031)	.362 (.014)	[.084]
Share of drugs offences related to class A drugs	.196 (.021)	.124 (.004)	[.002]
Share of drugs offences related to class B drugs (includes cannabis)	.776 (.022)	.841 (.005)	[.015]
Share of drugs offences related to class C drugs	.007 (.002)	.008 (.001)	[.793]
Share of drug offences relating to any cannabis offences	.714 (.025)	.779 (.006)	[.029]
Share of cannabis offences relating to having possession of cannabis	.913 (.012)	.915 (.004)	[.900]
Share of cannabis offences relating to having possession of cannabis with intent to supply	.045 (.007)	.045 (.003)	[.992]
Share of cannabis offences relating to production/being concerned in production of cannabis	.026 (.006)	.021 (.002)	[.519]
Share of cannabis offences relating to supply or offer to supply cannabis	.012 (.003)	.017 (.001)	[.534]
Share of cannabis offences relating to permitting premises to be used for unlawful purpose (cannabis)	.004 (.002)	.002 (.000)	[.478]

Notes: The data source used for this are in the individual crimes database held at the NDAD which are then aggregated to the borough-month level. The figures refer to the last twelve months in this database from April 1996 until March 1997. The p-value in the last column relates to a t-test of differences between Lambeth and non-Lambeth boroughs over this twelve month period. Control boroughs are all London boroughs excluding Lambeth and its neighbouring boroughs (Croydon, Merton, Southwark, Wandsworth and Westminster).

Table A5: The Within Policy Period Effects of the LCWS on Crime

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

Prais-Winsten Regression Estimates

	(1) Total (with drugs)	(2) Total (without drugs)	(3) Drugs Offences	(4) Violence Against the Person	(5) Sexual Offences	(6) Robbery	(7) Burglary	(8) Theft and Handling	(9) Fraud or Forgery	(10) Criminal Damage
Lambeth x Policy Period [1-6 months]	.072**	.075**	.028	.008	-.029	.204***	.038	.138***	-.014	.009
	(.034)	(.034)	(.046)	(.045)	(.047)	(.053)	(.056)	(.041)	(.058)	(.035)
Lambeth x Policy Period [7-13 months]	-.006	-.021	.205***	-.045	-.066	.114**	-.025	.056	-.075	-.057*
	(.032)	(.032)	(.043)	(.043)	(.043)	(.050)	(.052)	(.038)	(.055)	(.033)
Policy Period	.072**	.070**	.012	.008	.011	.015	.059**	.088***	-.002	.018
	(.032)	(.032)	(.020)	(.034)	(.008)	(.016)	(.028)	(.033)	(.029)	(.022)
Lambeth x Post-Policy Period	-.089***	-.113***	.241***	-.056**	-.058**	-.076**	-.197***	-.016	-.145***	-.065***
	(.020)	(.020)	(.025)	(.028)	(.027)	(.031)	(.035)	(.025)	(.032)	(.019)
Post-Policy Period	.104**	.104**	.009	.044	.022*	-.009	.061	.137***	-.009	.014
	(.045)	(.045)	(.029)	(.048)	(.011)	(.023)	(.039)	(.046)	(.041)	(.030)
Share of All Offences in Pre-policy Period	-	-	.023	.161	.009	.040	.122	.397	.086	.163
First and Second Half Policy Effects are Equal in Lambeth [p-value]	[.049]	[.017]	[.001]	[.298]	[.497]	[.142]	[.320]	[.081]	[.386]	[.123]
Borough, Month, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.994	.994	.761	.964	.659	.883	.948	.988	.874	.973
Observations	4055	4055	4055	4055	4055	4055	4055	4055	4055	4055

Prais-Winsten Regression Estimates

B. Dependent Variable: Log (recorded arrests of a given drug category, per offence)

	(1) Total (with drugs)	(2) Total (without drugs)	(3) Drugs Offences	(4) Violence Against the Person	(5) Sexual Offences	(6) Robbery	(7) Burglary	(8) Theft and Handling	(9) Fraud or Forgery	(10) Criminal Damage
Lambeth x Policy Period [1-6 months]	-.022***	-.008	-.279***	-.008	.056	.015	-.013	-.006	-.013	-.012
	(.008)	(.007)	(.074)	(.014)	(.048)	(.018)	(.013)	(.005)	(.015)	(.009)
Lambeth x Policy Period [7-13 months]	-.017**	-.001	-.325***	.016	-.009	.040**	-.015	-.006	.005	-.009
	(.008)	(.007)	(.070)	(.013)	(.048)	(.018)	(.013)	(.005)	(.014)	(.009)
Policy Period	-.001	-.001	-.009	.006	.024*	-.007	.002	-.002	.006	-.007
	(.006)	(.005)	(.021)	(.011)	(.014)	(.012)	(.006)	(.004)	(.007)	(.004)
Lambeth x Post-Policy Period	.008*	.011***	-.155***	.028***	.074***	.029***	.022***	-.000	.024***	.001
	(.005)	(.004)	(.046)	(.008)	(.025)	(.010)	(.008)	(.003)	(.008)	(.005)
Post-Policy Period	-.020**	-.017**	-.012	-.007	.030	-.021	-.011	-.018***	-.002	-.023***
	(.009)	(.008)	(.033)	(.016)	(.022)	(.019)	(.010)	(.006)	(.010)	(.007)
Share of All Accused in Pre-policy Period	-	-	.168	.222	.015	.047	.079	.330	.047	.092
First and Second Half Policy Effects are Equal in Lambeth [p-value]	[.656]	[.391]	[.584]	[.159]	[.306]	[.274]	[.903]	[.991]	[.342]	[.845]
Borough, Month, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.920	.915	.809	.860	.190	.421	.605	.894	.571	.649
Observations	2443	2443	2443	2443	2443	2443	2443	2443	2443	2443

Prais-Winsten Regression Estimates

C. Dependent Variable: Log (clear ups of a given drug category, per offence)

	(1) Total (with drugs)	(2) Total (without drugs)	(3) Drugs Offences	(4) Violence Against the Person	(5) Sexual Offences	(6) Robbery	(7) Burglary	(8) Theft and Handling	(9) Fraud or Forgery	(10) Criminal Damage
Lambeth x Policy Period [1-6 months]	-.031	-.018	-.310***	-.037	-.064	-.031	-.029	-.012	-.026	-.013
	(.020)	(.019)	(.082)	(.031)	(.083)	(.029)	(.037)	(.019)	(.036)	(.019)
Lambeth x Policy Period [7-13 months]	-.046**	-.035*	-.362***	-.030	-.098	-.022	-.050	-.023	-.025	-.022
	(.019)	(.018)	(.077)	(.030)	(.079)	(.028)	(.035)	(.016)	(.037)	(.017)
Policy Period	.007	.007	.021	.054*	.083**	.015	.013	.001	.027	-.002
	(.015)	(.014)	(.046)	(.032)	(.034)	(.016)	(.015)	(.016)	(.022)	(.013)
Lambeth x Post-Policy Period	-.016	-.019	-.178***	-.008	-.058	-.026*	-.002	-.019	-.032	-.012
	(.013)	(.014)	(.043)	(.019)	(.043)	(.016)	(.021)	(.012)	(.020)	(.012)
Post-Policy Period	.003	.005	.056	.101**	.179***	.038	.008	-.004	.071**	-.008
	(.022)	(.021)	(.071)	(.047)	(.052)	(.024)	(.022)	(.022)	(.033)	(.019)
Share of All Clear-ups in Pre-policy Period	-	-	.146	.294	.018	.034	.082	.275	.051	.099
First and Second Half Policy Effects are Equal in Lambeth [p-value]	[.489]	[.418]	[.617]	[.865]	[.760]	[.810]	[.647]	[.635]	[.977]	[.704]
Borough, Month, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Operation Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.694	.662	.429	.858	.418	.408	.433	.577	.640	.524
Observations	3790	3790	3790	3790	3789	3789	3787	3786	3789	3786

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. All observations are at the borough-month-year level. The sample period considered in Panel A is the same as that used in the drugs offence specification, running from April 1992 until January 2006 with a 12 month gap from April 1997 to March 1998.The dependent variable in Panel B is the log of the number of recorded arrests divided by the average of offences in the current month and the previous three months in the same borough. The sample period considered in this panel is the same as that used in the drugs arrests per offence specification and runs from April 1998 to January 2006.The dependent variable in Panel C is the log of the number of recorded clear ups divided by the average of offences in the current month and the previous three months. The sample period considered in this panel is the same as that used in the drugs clear-ups per offence specification and runs from April 1992 until March 2005, with a break due to missing drugs offence data from April 1997 to March 1998. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark, Wandsworth and Westminster). 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 (excluding neighbouring boroughs) London population that month-year in the borough. The policy period [1-6 months] dummy variable is equal to one from July 2001 until December 2001. The policy period [7-13 months] dummy variable is equal to one from January 2002 until July 2002, and zero otherwise. The post-policy period dummy variable is equal to one from July 2002 onwards, and zero otherwise. The socio-demographic control variables, measured in logs at the borough-month-year level, are 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, and aged above 50 and the male unemployment rate. We also control for a series of dummies covering the time periods from which the underlying time series for offences are recovered from. 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). The share of all offences (accused/clear-ups) in the pre-policy period is the average ratio of the offence (accused/clear-ups) type to total offences (accused/clear-ups) in the period from January 2000 until June 2001. At the foot of each column we report the p-value on the null hypothesis that the coefficients on the Lambeth x Policy Period [1-6 months] and Lambeth x Policy Period [7-13 months] interactions are equal.

Figure 1A: Drug Offences Time Series

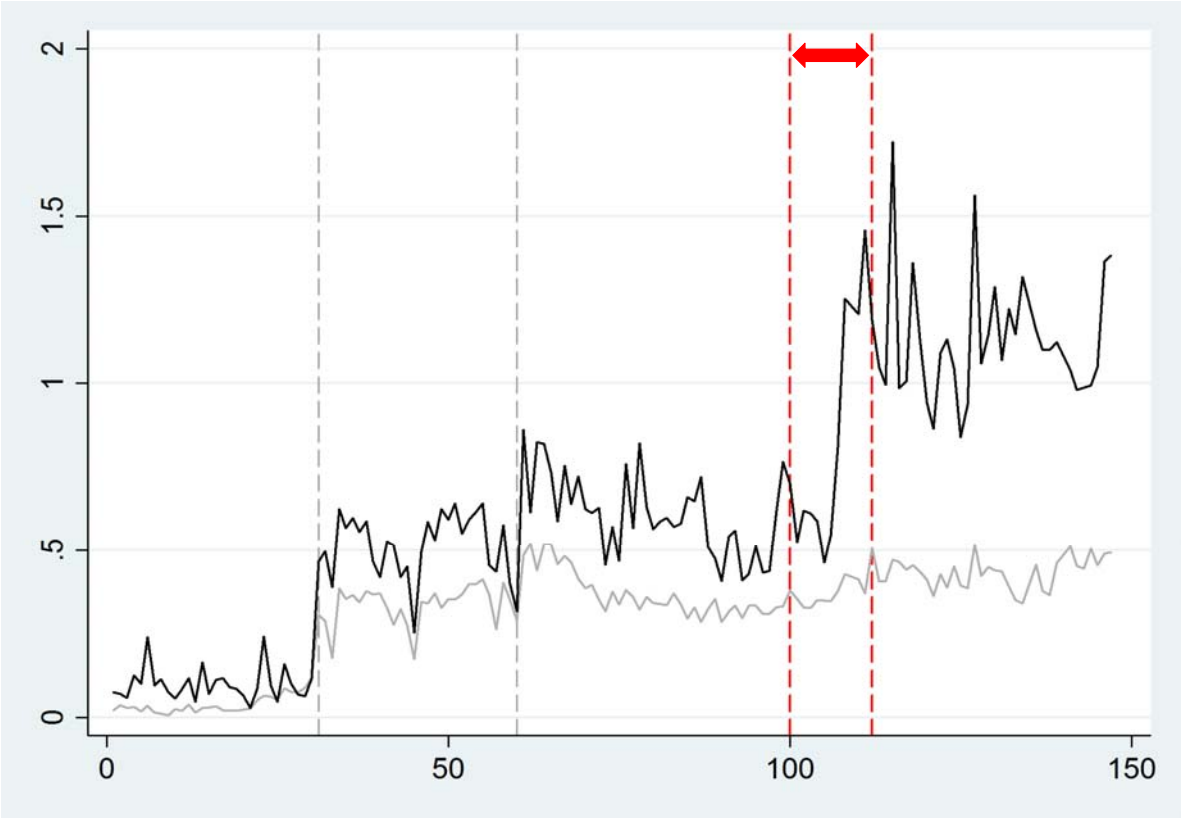
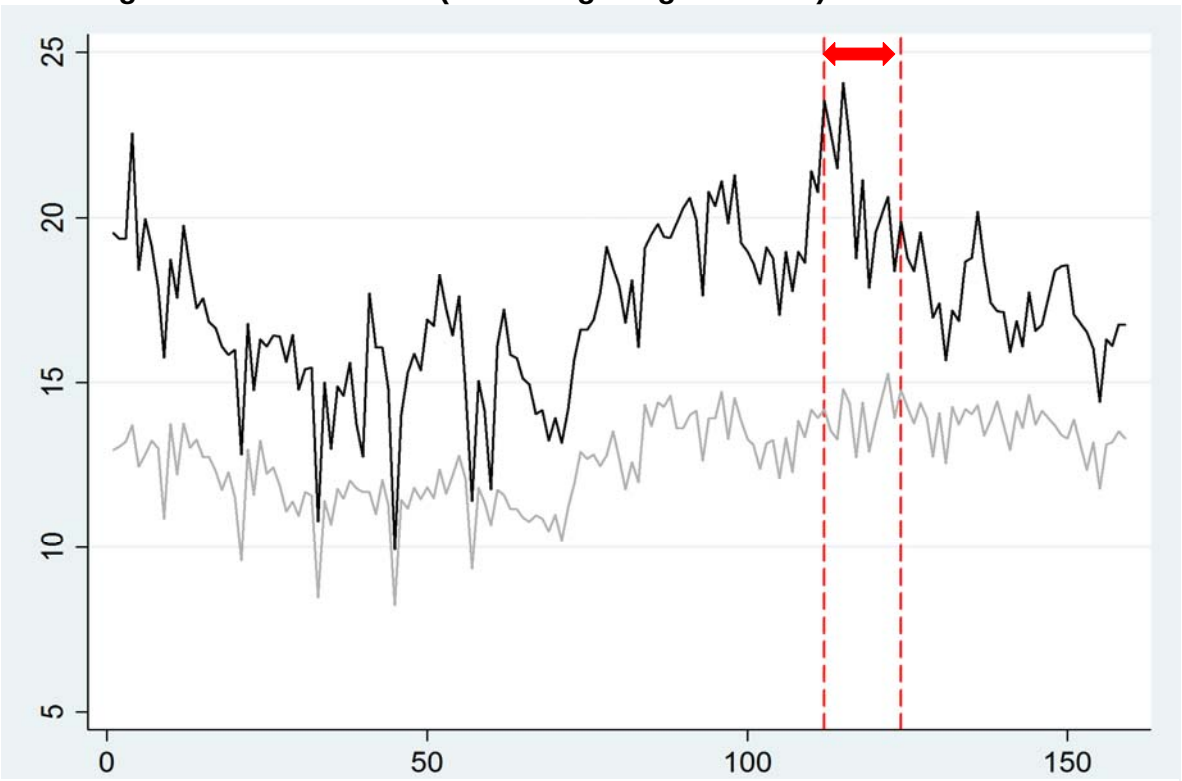


Figure 1B: All Offences (Excluding Drug Offences) Time Series



Notes: For Figure 1A the time period runs from April 1992 until June 2005, excluding the period April 1997 to March 1998. The time variable was recoded to skip these 12 months, thus April 1998 directly proceeds March 1997. The second vertical line on the graph is placed at the time at which the data series breaks (March 1997). The first grey dashed vertical line marks a change in the Home Office coding for drug offences in October 1994. The third and forth vertical lines represent the start and end of the Lambeth policy (July 2001 and July 2002 respectively). The black time series represents the drug offences per capita for Lambeth. The grey series represents the mean drug offences per capita for the rest of London, excluding Lambeth and Lambeth's neighbouring boroughs (Croydon, Merton, Southwark, Wandsworth and Westminster). For Figure 1B the time period runs from April 1992 until June 2005. The two vertical lines represent the start and end of the Lambeth policy (July 2001 and July 2002 respectively). The black time series represents the total offences (excluding drugs) per capita for Lambeth. The grey series represents the mean total offences (excluding drugs) per capita for the rest of London, excluding Lambeth and Lambeth's neighbouring boroughs (Croydon, Merton, Southwark, Wandsworth and Westminster). There are no breaks in this second times series.