

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

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Abstract

We evaluate the effects of a localized and one-year policing experiment that decriminalized cannabis possession in the London borough of Lambeth between 2001 and 2002, on monthly crime rates in Lambeth and in neighboring boroughs. Our main empirical results are that decriminalization in Lambeth leads to a surge in drug related offences, and a collapse in accusation and clear-up rates for drug related crimes in Lambeth. These effects of the police appearing to turn a blind-eye to all illicit drugs activity in Lambeth are quantitatively large and persist well after the policy experiment ends. However, the policy does allow the Lambeth police to reallocate their effort towards other crimes, leading to permanent reductions in nearly all other crime types. We also present evidence that drug consumers and suppliers relocate to Lambeth from neighboring boroughs after decriminalization. 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 the size of the illicit drugs market but does allow the police to reallocate effort towards non-drug related crime. We interpret all the results through a Hotelling-style model that makes precise the behavioral response to decriminalization of the police, suppliers and demanders of drugs.

Keywords: cannabis; crime; decriminalization.

JEL Classification: H75, J18, K42.

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1 Introduction

In nearly every country the market for illicit drugs remains pervasive, despite long running attempts to prohibit or restrict such activities. The policy approach in the US, as manifested in the ‘war on drugs’, emphasizes a punitive approach. In contrast, liberal attitudes of law enforcement towards cannabis possession – *de facto* or *de jure* decriminalization – is internationally common. Australia, Germany, the Netherlands, Portugal, Spain and 12 US states have all implemented some form of decriminalization. Despite different approaches, many countries have a high prevalence among 16 years old, as shown in Figure 1 [Pudney 2010].¹

Given these magnitudes, it is unlikely this market will disappear over time, hence the need to consider some form of market intervention. Moreover, the tough policy in the US has proved to be expensive, estimated to cost tens of billions of dollars annually. It is 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 is reinforced by market characteristics such as the consumption of illicit drugs damaging user’s health; the use of some drugs providing a gateway to using 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].²

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. The decriminalization of cannabis has been an ongoing policy debate in the UK as in many other countries. 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.³

¹The figure is based on the *ESPAD* group of school surveys for Europe, and the *Monitoring the Future* survey in the US. An alternative source of data for cross country comparisons are the WHO World Mental Health Surveys 2001-4 that cover 17 countries, including the US but not the UK. These find that 42% of American adults report having tried cannabis, significantly higher than all other countries except New Zealand [Degenhardt *et al* 2008]. For the UK, there are an estimated 5.5 million cannabis users, and the market is estimated to be worth almost one billion pounds [Pudney *et al* 2006].

²The causal relationship between the consumption of illicit drugs and harm to the user is controversial [Van Os *et al* 2002, 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].

³Clearly 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.

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.⁴

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: the possession of cannabis for personal consumption was decriminalized; 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; 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. Long lived effects can occur if a temporary policy causes long term shifts in *de facto* policing strategies, or if there are permanent changes in the market for illicit drugs, such as fixed costs of entry and exit in criminal networks. However, such changes in the underlying organization of the illicit drugs market might only occur if the policy is perceived to be permanent.

Third, the policy is localized as it is only implemented in Lambeth. Given transportation links

⁴Current evidence links the size of drugs markets to crime rates both at the aggregate level for US counties [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 [Baker 1998, Corman and Mocan 2000, Entorf and Winker 2008]. 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.

and the relatively short distances involved, shown in Figure 2, 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 empirical analysis and help interpret the evidence, we first develop a Hotelling-style model of two borough locations, that sheds light on how the link between decriminalization and crime can be driven by the behavioral responses of three types of agent: police officers, suppliers of drugs, and consumers of drugs. The framework makes precise the effect of a decriminalization policy when unilaterally implemented in one borough on 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. To help structure the empirical analysis, the model emphasize what these effects will be in the borough that unilaterally decriminalizes, and the potential spillover effects on the other borough. Finally, the model helps us compare the effects of such a localized decriminalization to the effects of a nationwide decriminalization policy that is simultaneously implemented in both boroughs.

On the empirical analysis, one contribution of our study is to first 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 eight crime types: drug related, violence against the person, sexual offences, robbery, burglary, theft and handling, fraud and forgery, criminal damage. Finally, for each crime type, we construct separate series by borough-month for the number of recorded offences, as well as two measures more closely related to police behavior: the number of individuals accused for each given crime type, and the number of crimes clear-up by the police.⁵

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, we find no evidence of there being reductions or 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, all else equal.

We conduct a series of thought experiments to shed light on the extent to which the change in drug crime in Lambeth is driven by changes in the market size for cannabis or other illicit drugs. These imply the quantitative results could be explained by: (i) a 25% increase in offences related

⁵To be clear, the number of offences is the number of offences recorded by the police. These offences might be reported by the public or the police, and there can be more than one individual accused for any recorded offence.

to cannabis possession, which perhaps reflects that drug suppliers are apprehended with large quantities of cannabis after the policy is introduced; (ii) or by between a 100 and 500% percent increase in offences related to the possession of other illicit drugs, depending on the assumptions made on whether the police in Lambeth *de facto* already turn a blind-eye to cannabis possession prior to the formal introduction of the LCWS. In any case, the results certainly rule out no change in the underlying market size for cannabis or other illicit drugs in Lambeth post-policy.⁶

Second, examining police behavior in Lambeth, we find significant declines during and after the LCWS's introduction in accusation 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 accusations 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, and this remains a long run phenomena. Of course, such reduced police effort, in terms of lower accusation and clear-up rates, might itself feedback to drive the higher rates of drug related offences previously documented. In short, it is as if with the introduction of the LCWS, the police turn a blind-eye towards the market for illicit drugs as whole and this leads to a large and sustained surge in drug offences in Lambeth, and a long run collapse in the Lambeth police's effectiveness against drug related crime.

Our third set of results therefore explore whether such a blind-eye 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. Doing so we find that in the post-policy period, total crime in Lambeth significantly falls by 6.9%, against a backdrop of London-wide increases in total crime. This is despite the previously documented increases in drug related crime. More precisely, 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 long run falls in crime go against the London-wide trend of increases in each type of crime. Moreover, the Lambeth police become efficient in tackling other types of crime as measured by significant increases in accusation rates. However, we find no evidence that this eventually feeds through into higher clear-up rates for other crime types.

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 towards other crime types as reflected in significant increases in accusations per offence for non-drugs crime.

⁶We 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.

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 London boroughs over time, and describes our empirical method. Section 5 presents the core results on the effects of the LCWS on drug crime and police efficiency in relation to drug crime, within Lambeth. Section 6 presents evidence on the effects of the LCWS on other crime types in Lambeth, on crime in neighboring boroughs, and the effects on crime of the nationwide decriminalization of cannabis. Section 7 concludes with a discussion of whether and how the illicit drugs market for London as a whole might have restructured in response to the LCWS, 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.

2 Theoretical Framework

We develop a Hotelling-style model of the illicit drugs market, accounting for profit maximizing drug suppliers, utility maximizing drug consumers, and crime minimizing behaviors of local police. We do so to help organize and interpret the empirical analysis by making precise the effects of decriminalization on the behavior of drug suppliers, drug consumers, and the police.

The model has three key ingredients. First, consumers are uniformly located on a segment of unit length between two boroughs, there are two drug suppliers each located at one end of the line, and each borough has its own police force. Second, the market for drugs generates drugs related crime, and might also generate other types of crime as suggested by the existing evidence [Grogger and Willis 2000, Pacula and Kilmer 2003]. Third, each police force optimally allocate its effort to minimize a weighted sum of drug related crime and other crimes. The weights reflect the relative importance the police place on drug crime in the borough.

We then investigate the effect of decriminalizing drugs – as defined by a change in the weight placed on drugs related crimes in the police’s objective function – on the following endogenous outcomes: the equilibrium demand and price for drugs, police effort towards drugs and non-drugs crimes, and the numbers of drugs crimes and non-drugs crimes. We calculate such effects when the policy is implemented locally in one borough, and in both boroughs simultaneously.

2.1 Consumers

Consumers are uniformly located on a segment of unit length. They face a transport cost t per unit. Denoting the distance along the segment from A as x , consumers then face total transport costs of tx to travel to borough A . There are two drug suppliers, with one located at each end of the line, taken to be the center of boroughs A and B . The relative price of cannabis in borough $b = A, B$ is π_b and the intensity of policing drug crimes in borough b is P_b^d . Such policing imposes

a total cost on buyers, αP_b^d , where P_b^d can be thought of as the probability of being detected engaging in the illicit drugs market, and α can be thought of as the punishment conditional on having been apprehended. Hence, the utility of a consumer located in each market is,⁷

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

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

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

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 $D_A = x^*$.⁸

2.2 Suppliers

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

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

As is intuitive, equilibrium prices are decreasing in the intensity of drug related policing within the borough ($\partial \pi_b / \partial P_b^d < 0$), equilibrium prices increase as drug related policing in the other borough, b' , increases ($\partial \pi_b / \partial P_{b'}^d > 0$) or consumers' transportation costs rise. The resulting equilibrium market size for illicit drugs in each borough is therefore a function of drugs related policing intensity in *both* borough locations,

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

The interlinkage of drugs markets across boroughs opens up the possibility that localized decriminalization policies that alter policing intensity in one borough, change the behavior of consumers and suppliers in the other borough. Such concerns over drug tourism have been central

⁷Following the discussion in Pudney [2010], we note that a back of the envelope calculation suggests the probability of apprehension, is low. Of the 5.5 million estimated cannabis users in England and Wales, approximately 90,000 were apprehended, implying a 1.6% annual risk of apprehension. On the other hand, the penalty α is large. The mean prison sentence for cannabis related offences was 17 months in England and Wales in 2004.

⁸Note that we assume consumers only have to choose where to purchase drugs from, not whether they consumer them in the first place. Hence 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.

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 localized and nationalized decriminalization policies.⁹

2.3 Crime and Policing

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 as follows,

$$\begin{aligned} C_b^d &= \delta_0 D_b(\alpha, t, P_A^d, P_B^d) - e(P_b^d), \\ C_b^o &= \delta_1 D_b(\alpha, t, P_A^d, P_B^d) - e(P_b^o). \end{aligned} \quad (5)$$

The first term captures the fact that the illicit drugs market can generate drug related and other crime types, and the second term captures the fact that police effort towards a crime type reduces the number of those types of crime all else equal. $e(\cdot)$ captures both the deterrence and incapacitation effects of policing on crime – our data does not allow to attempt to distinguish these effects empirically. We assume: (i) $\delta_0 > 0$, $|\delta_0| \geq |\delta_1|$, implying the drugs market has a larger effect on drugs crime (C_b^d) than on other crimes (C_b^o); (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.¹⁰

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

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

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 in borough b is then implicitly defined through the first order condition,¹¹

$$(1 - \lambda_b)e'(P - P_b^d) - \lambda_b e'(P_b^d) = \frac{\alpha}{6t} [\lambda_b \delta_0 + (1 - \lambda_b) \delta_1]. \quad (7)$$

⁹We have no detailed 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 being considerable dispersion in the price-quality ratio for illicit drugs.

¹⁰If $\delta_1 > 0$ drugs and other crime types are complements, consistent with the literature. However, for some crime types it might be that a larger drugs market crowds out other forms of criminal activity – say because those activities would attract police attention, so that $\delta_1 < 0$. We also assume any spillover effects of the drugs market on crime occur within the same borough. This is a simplifying assumption and the implications are largely unchanged if some fraction of other crimes were committed in the other borough.

¹¹As 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.

2.4 Decriminalization

We model the local decriminalization of drugs in borough b as corresponding to a reduction in λ_b — the relative weight of drug related crimes in the police’s objective function, (6). To see the effects of such a policy within the same borough, we totally differentiate (7) to obtain,

$$\frac{dP_b^d}{d\lambda_b} = -\frac{\frac{\alpha}{6t}(\delta_0 - \delta_1) + [e'(P - P_b^d) + e'(P_b^d)]}{(1 - \lambda_b)e''(P - P_b^d) + \lambda_b e''(P_b^d)} > 0, \quad (8)$$

Hence the decriminalization of drugs ($d\lambda_b < 0$) reduces drugs related policing ($dP_b^d < 0$). Given total police resources P are unchanged, police intensity related to other crime types, P_b^o , necessarily increases. Hence an important mechanism through which decriminalization affects crime is that that enables the police to reallocate their effort from drugs crimes to other crimes. From (5) we can then derive the effects of decriminalization on each crime type within the borough,

$$\frac{\partial C_b^d}{\partial \lambda_b} = -\left[\frac{\alpha}{6t}\delta_0 + e'(P_b^d)\right]\frac{\partial P_b^d}{\partial \lambda_b} < 0, \quad (9)$$

$$\frac{\partial C_b^o}{\partial \lambda_b} = -\left[\frac{\alpha}{6t}\delta_1 - e'(P - P_b^d)\right]\frac{\partial P_b^o}{\partial \lambda_b} > 0. \quad (10)$$

Therefore decriminalization increases drugs related crime. To see the effect on other crimes, note that from (7) it can be shown that $-\left[\frac{\alpha}{6t}\delta_1 - e'(P - P_b^d)\right] > 0$. Hence $\frac{\partial C_b^o}{\partial \lambda_b} > 0$ even if $\delta_1 = 0$ so there are no crime spillovers, decriminalization decreases other crime types through freeing up police resources. The following result summarizes the main within borough effects of a localized decriminalization that we are able to take to the data,

Result 1 (Within Borough): *A localized decriminalization in borough b : (i) reduces police intensity related to drugs crimes; (ii) increases police intensity for other crimes; (iii) increases the number of drugs crimes; (iv) decreases the number of other crime types.*

While we have so far emphasized the within borough effects of decriminalization, the model also makes precise the inter-linkages between the drugs markets in the two boroughs. Hence as is intuitive, the equilibrium changes in policing in the decriminalizing borough described above, will also have effects in the neighboring borough b' . To see the effects on drugs crime in b' we again totally differentiate (7),

$$\frac{\partial C_{b'}^d}{\partial \lambda_b} = \delta_0 \frac{\partial D_b}{\partial P_{b'}^d} \frac{\partial P_{b'}^d}{\partial \lambda_b} > 0, \quad (11)$$

so that decriminalization in borough b reduces drugs crime in b' because of drugs tourism towards borough b .¹² Given spillovers from drugs to non-drugs crime ($\delta_1 > 0$) then other types of crime will also decrease in neighboring borough b' if borough b decriminalizes ($\partial \lambda_b < 0$). To summarize,

Result 2 (Across Borough): *A localized decriminalization in borough b has the following*

¹²Note that the effect on drugs crime in b' works through change in market size in location b rather than change in policing in location b' ($\frac{\partial P_{b'}^d}{\partial \lambda_b} = 0$). If we were to assume the drugs market in borough b generated crime spillovers in borough b' there would be an additional direct policing response in b' to decriminalization in b . Empirically we will explore whether police behaviors change in neighboring boroughs to Lambeth after the LCWS is introduced.

effects in neighboring borough b' : (i) decreases the number of drugs crimes; (ii) decreases the number of other crime types.

It is straightforward to establish the effects of a nationwide decriminalization policy where both λ_A and λ_B are simultaneously reduced by the same amount. There is no overall change in the relative size of each market for illicit drugs. In both boroughs, there will be an increase in drugs related crime, a re-allocation of police resources towards other crimes, and a subsequent decrease in other crimes.

2.5 Discussion

While we have modeled decriminalization as a reduction in λ , this framework suggests decriminalization might be interpreted as reducing the costs to consumers of being apprehended by the police, α . However, given the multiplicative interaction between α and policing intensity in (1), this has qualitatively the same effects as decriminalization reducing λ that in turn reduces police intensity on drugs related crime.

A second issue relates to the distinction between cannabis and non-cannabis related drugs crime. For example, an alternative modeling approach would be to interpret decriminalization as reducing δ_0 so the number of recorded offences is less sensitive to the size of the drugs market because activities related to cannabis possession are no longer recorded as offences. If so, total drugs crime could still rise if the fall in cannabis related offences were more than offset by a rise in non-cannabis related drug offences, as emphasized in the model.

More generally, 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 model highlights that the decriminalization of cannabis allows police to reallocate effort towards other drugs crime. However, this might increase or decrease total drugs crime ($C_b^d + C_b^o$), on which the model provides ambiguous predictions. Intuitively, if the vast majority of drugs crime are cannabis related, then the policy might cause an overall reduction in drugs crime simply because some activities are no longer recorded as offences. On the other hand, if the decriminalization of cannabis effectively leads the police to turn a blind-eye to *all* drugs related crime, then total drugs crime would rise. Finally, if the policy is *de facto* already in place, then there should be little effect on drugs crime overall. These are precisely the types of scenario that we discuss in various thought experiments in Section 5 to help explain the results.

Finally, there are two simple extensions to the model that are worth discussing. First, we have assumed throughout that the location of drug suppliers is fixed at the center of each borough. If suppliers 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 the reduction in police intensity for drugs-related crime in borough b , as emphasized above.

Second, although many of the key intuitions linking decriminalization and the behavior of police, drugs suppliers and drug consumers are brought to the fore in the static model presented,

we can think through extending the model to capture dynamic policing effects. In particular, the ability of police effort to reduce crime might depend both on the current levels of police intensity, as well as that in the last period. If so, the effect of police intensity on drugs crime in (5) in period t would be $e(P_b^{d,t}, P_b^{d,t-1})$. This captures 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 estimate both effects in our empirical analysis.

3 The Lambeth Cannabis Warning Scheme (LCWS)

3.1 Background

The trigger for both the localized decriminalization policy in Lambeth that we specifically focus on, and the more general ongoing debate on the nationwide policing of illicit drugs, can be traced back to the publication of the Runciman Report in 2000. This was a high profile inquiry commissioned by the Police Foundation and authored by Viscountess Runciman. 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 1971.¹³

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 drug classification system to be more closely based on the scientific evidence of relative harm and consequently that cannabis should be reclassified from Class-B to a Class-C drug. The report emphasized three benefits of this approach: (i) making cannabis possession a non-arrestable offence would reduce the number of individuals being criminalized; (ii) this might remove a source of friction between the police and the wider community; (iii) this would free up police time.¹⁴

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

¹³In part, the reason why the report carried such weight was because its chair, Ruth Runciman, had a distinguished reputation having been the former Chair of the UK Mental Health Act Commission, a founder of the Prison Reform Trust in 1981, closely involved with the work of the Citizens Advice Bureau, and she had previously made significant contributions to understanding drug misuse, for which she was awarded the OBE in 1991.

¹⁴The report stated, “Our conclusion is that the present law on cannabis produces more harm than it prevents. It is very expensive of the time and resources of the criminal justice system and especially of the police. The normal sanctions for offences of cannabis possession and cultivation for personal use would be out-of-court disposals, including informal warnings, statutory cautions or a fixed fine on the model of the Scottish fiscal fine. Prosecution would be the exception, and only then would a conviction result in a criminal record”.

of Lambeth, Brian Paddick, conducted a staff consultation exercise on drugs policing strategy in his borough. In the consultation period, officers complained they spent a considerable amount of time dealing with arrests for possession of cannabis 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. 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.^{15,16}

Under Brian Paddick, with the sanctioning of the Commissioner of Police, 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 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 those supplying cannabis. Hence there was no attempt to try and 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.¹⁷

To gauge public opinion during 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.¹⁸ 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. 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 the UK as a whole.

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 restructuring 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

¹⁵Brian Paddick has been no stranger to controversy. He was the United Kingdom’s most senior openly gay police officer. After retiring in 2007, he entered politics, standing as a candidate for London Mayor in 2008.

¹⁶We also note that if the policy was *de facto* in effect before being formally introduced, as might be suggested by the concerns raised by some police officers during Brian Paddick’s consultation exercise in Lambeth, then all else equal, we should observe no change in offences of any crime type during the policy period. A related concern is that police officers might game the policy and so record cautions for the possession of small quantities of cannabis, as clear-ups to meet performance targets. If so, we should observe an increase in clear-ups for drugs related crimes in the policy period. We find no empirical support for this hypothesis, as presented later.

¹⁷If those found in possession of cannabis were juveniles, their parents would have been notified. The police also maintained records of previous earning and retained the power to prosecute.

¹⁸A survey of police was also conducted in the form of a voluntary questionnaire. However, only 51 out of Lambeth’s 860 officers responded so not much can be credibly inferred.

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, almost 13 months after its introduction. 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.

Despite ending the policy, the MPA as a whole, not only the police in Lambeth, refuted many of the claims made against the LCWS. For example, the MPA argued that in the year prior to the pilot scheme, 57% of those arrested for all cannabis offences, corresponding to 454 individuals, were Lambeth residents. In the year of the pilot scheme they argued that this proportion remained at 57%, although during the policy-period this corresponded to 790 arrests. The police also surveyed local head teachers with 7 out of 10 heads of secondary schools responding, and 55 out of 60 primary school heads responding. Secondary heads all said there had been no increase in drugs incidents in their schools, with some saying there had been fewer such incidents. No primary heads reported incidents of pupils being intoxicated.

In part because of these 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.

3.3 Mapping the Policy to the Model

We modelled the decriminalization policy as changing the weight the police force in Lambeth placed on drug crimes relative to other crimes ($d\lambda_b < 0$). The consequent effects on crime and policing discussed in Section 2, are reinforced if the policy also reduced the costs to consumers of being apprehended by the police, α . On the other hand, if in the model we interpret drugs crimes (d) as being those specifically related to cannabis, and other crimes as relating to other illicit drugs, then the framework makes precise that the decriminalization of cannabis could increase drug offences overall if the increase in offences related to other drug types more than offsets the

fall related to cannabis possession offences. In the empirical analysis we use detailed historic data from the pre-policy period to present some thought experiments to shed light on the plausibility of such mechanisms, given the actual observed change in total drugs offences due to the LCWS.

Finally, there is no reason to expect the post-policy crime patterns to return to those that were in place in the pre-policy period. The first obvious reason is because policing strategies did not return to their pre-policy levels.¹⁹ Moreover, there might be dynamic policing effects of the type discussed in Section 2.5 such as learning-by-doing 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.

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. 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, accusations, and clear-ups for each borough-month.²⁰

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). This provides details for each crime, 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, accusations, 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 combine these data sources to create a panel data set for crime rates over almost two decades by borough-month in London.

¹⁹Denoting the pre-policy, policy, and post-policy periods as time periods -1,0 and 1 respectively, the relative weight placed by the police in Lambeth on drugs crimes can then be modelled over time as $\lambda_b^{-1} > \lambda_b^1 > \lambda_b^0$.

²⁰We do not use available data on crime victimization, say from the *British Crime Survey*, because this does not produce reliable estimates at the borough level. Recorded offences – as reported either by the public or the police directly – will typically underestimate the true number of offences.

4.1.2 Police Operations and Other Data

Given that we evaluate the effect of the LCWS policy on crime in Lambeth, it is plausible that other police operations also affect crime. It is therefore important to control for such operations and measure whether the LCWS shifts crime patterns over and above the effects of all other police activities. 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 over which the operation is in place. 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 documents police operations that are known to be borough specific, but on which we have incomplete information regarding 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.²¹

To control for characteristics of the local economy, we use the *Quarterly Labor Force Local Area Data* (QLFSLAD). This provides for each London borough, quarterly information on its 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, accusations, and clear-ups by borough-month.

4.2 Descriptives

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 drugs related offences per 1000 of the adult population. 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 that records drug offences in the pre-policy period. Pre-policy, Lambeth had the 7th highest rate of

²¹Other 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.

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 as shown in Columns 4 and 5. In terms of the framework developed, these results are in line with Result 1 on the within borough effects of decriminalization.

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. 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.

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, we note that, 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.

Figure 3 shows the time series for drug offences in Lambeth and the average for all other London boroughs, where the horizontal axis measures time in terms of the number of months since January 1990. The solid vertical line corresponds to the break in the time series for drug offences from April 1997 to March 1998. Three points are of note. First, the figure makes clear that 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.²²

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.

²²Some of the other jumps in the time series in Figure 3, such as those around $t = 30$, correspond to when the underlying source of data used to construct the series changes slightly. Throughout the empirical analysis we control for a series of dummies to capture these underlying different data regimes.

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,

$$\begin{aligned} \ln C_{bmy} = & \beta_0 P_{my} + \beta_1 [L_b \times P_{my}] + \beta_2 PP_{my} + \beta_3 [L_b \times PP_{my}] \\ & + \gamma X_{bmy} + \lambda_b + \lambda_m + \lambda_y + u_{bmy}, \end{aligned} \quad (12)$$

where C_{bmy} 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 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. These capture the total behavioral response of the police, drug consumers, drug suppliers, and other members of the public to the policy. β_0 and β_2 capture London-wide trends 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 (12) weighting the observations by the borough population.²³

We define time t as the number of months since January 1990: $t = [12 \times (y - 1990)] + m$. For our baseline estimates we assume a Prais-Winsten borough specific AR(1) error structure,

$$u_{bmy} = u_{bt} = e_{bt} + \rho_b e_{bt-1}, \quad (13)$$

so u_{bmy} is borough specific heteroskedastic, and contemporaneously correlated across boroughs. Given the potential for the police forces to coordinate actions, we also estimate (12) allowing the error term to be spatially correlated across boroughs [Chen and Conley 2001].

²³On the controls, we note 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.

5 Baseline Results

5.1 Drug Offences

We estimate (12) to determine the impact of the LCWS on reported offence rates for drug related crimes. Table 2 presents the results. To begin with, Column 1 only controls for the dummies for the policy and post-policy periods respectively (P_{my} , PP_{my}) and interactions of both with the dummy for Lambeth. In line with the descriptive evidence in Table 1 and Figure 3, Lambeth experiences a significant increase of 16% in recorded drug related crimes moving from the pre-policy period to when the LCWS was in place ($\hat{\beta}_1$). Moreover, this increase is sustained in the post-policy period where reported drug crimes are 27% higher than in the pre-policy period in Lambeth ($\hat{\beta}_3$). 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 coefficients on the dummies for the policy and post-policy periods ($\hat{\beta}_0$, $\hat{\beta}_2$) are both positive and significant suggesting drug crimes were rising London-wide over this period. However, Column 2 shows these London-wide time trends are not robust to the inclusion of month, borough and especially year fixed effects. Conditional on such fixed effects we see that although Lambeth experiences significant increases in drug crime in the policy and post-policy periods, this is in sharp contrast to there being no such trends across London on average, with the point estimates on these trends being close to zero and precisely estimated.

Columns 3 to 7 show these results to be robust to: (i) controlling for time varying demographic and economic characteristics of the borough (X_{bmy}); (ii) allowing the error terms to be serially correlated within borough following the process described in (13); (iii) allowing for a borough specific quadratic time trend, namely including $\lambda_b \times t$, $\lambda_b \times t^2$ as additional controls in (12) to address concerns raised earlier of there being natural trends in drug crimes over time that might differ by borough; (iv) controlling for the set of police operations conducted by borough during the sample period, as recorded in Panel A of Table A1; (v) allowing for the error structure to be spatially correlated across boroughs.²⁴

Throughout we find that Lambeth experienced a statistically significant rise in drugs offences during the policy period, a time at which drugs offences did not change in the rest of London on average. After the LCWS was officially ended, rates of drug related criminal offences in Lambeth increased at an even greater rate, with again no corresponding increase London-wide. Importantly, these effects exist over and above any long term borough specific time trends in drug crime.

²⁴More precisely, we assume the error term takes the form, $u_{bmy} = \theta \sum_{c=1}^N w_{bc} u_{cm} + e_{bmy}$, 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 $w_{bc}^s = w_{bc} / \sum_c w_{bc}$. For the result presented in Table 2 we define \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.

To assess the magnitude of these effects, we note that from Column 3 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 yardstick, the LCWS appears to have had qualitatively large effects on drug crimes in Lambeth in the short and long run.²⁵

Columns 7 and 8 check for dynamic policy responses during and after the LCWS's implementation. Column 7 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 3, 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 8 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 as Lambeth reverts back to its old pre-policy crime levels.

From January 2000 onwards, the MPA publishes detailed information on detailed sub-categories of criminal offences, including those for drug offences, at the borough-month level. Table 3 presents estimates of our baseline specification (12) for each drugs related sub-offence type. To begin with, Column 1 confirms the previous results for total drug offences continue to hold over this sample period. Columns 2 to 4 then show that the aggregate rise in total drug offences is primarily driven by increases in the possession of drugs: 10.5% (20.6%) 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, and although other drug offences do increase in Lambeth post-policy, such offences constitute a small share of overall drug offences. 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.²⁶

²⁵An 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 -0.4 to -0.5 . Assuming these estimates apply to drugs crime in London, the long run increase in drug crime of 24% reported in Column 6 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.

²⁶To see why possessions offences relate to the behavior of suppliers as well as consumers, note that the police

For drugs possession offences, the last two columns of Table 3 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.

5.2 Robustness Checks

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 3.

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, defined to be 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 in some London boroughs.

always have the option to record an offence or charge an individual 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.

5.3 Thought Experiments

As highlighted by Result 1, decriminalization reduces police intensity against drugs related crime and so increases drugs crime, all else equal. Mapping the model to the evidence we need to precisely distinguish between cannabis and other drugs. Doing so, we might have expected the policy to reduce cannabis related offences because apprehending individuals in possession of small quantities of cannabis would no longer be recorded as an offence. In this scenario, drugs crime overall can only have increased overall if, either: (i) the policy causes an increase in the quantities of drugs possessed by dealers or consumers; or, (ii) there is a rise in non-cannabis offences that more than offsets any falls in offences for cannabis possession.^{27,28}

On this second channel of more non-cannabis offences actually being committed, this might in part reflect changes in behavior of drug suppliers and the police. For example, decriminalization causes dealers to relocate closer to Lambeth, all else equal. As the LCWS policy does not alter the penalties for supplying cannabis, there is no incentive for dealers to segment the market and only specialize in the supply of cannabis. On changes in police behavior explaining the increase in drug offences, if in the model we interpret drugs crimes (d) as being those specifically related to cannabis, and other crimes as relating to other illicit drugs, then the decriminalization of cannabis allows the police to reallocate their effort towards other drug types. The increase in drug offences overall then relates to the police being better able to detect crimes related to other illicit drug, and this more than offsets any fall related to cannabis possession offences.

To assess these scenarios, we conduct a series of thought experiments to understand what would need to be true for each mechanism to explain the increase in recorded offences for drugs possession measured in Column 2 of Table 3. 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 4 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

²⁷We have been unable to find any official definition of the quantity threshold for cannabis deemed to be for personal consumption, nor on the distribution of quantities confiscated from possessions.

²⁸There are other explanations for the documented increase in drug offences, although these appear to be far less plausible. For example, it might be 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 in the long run even after the policy has been ended, or why reporting behavior specifically related to drugs possession offences rather than drug trafficking offences, say, should change. Similar concerns suggest the observed change in drug related criminal offences is unlikely to be caused by the police merely recording more drug offences. If anything, the police would have incentives to game the policy by claiming higher clear-up rates as this is what their performance targets are based upon. We later directly examine the effects of the policy on clear-up rates related to drug offences and find no evidence of such gaming.

constitute 4% 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 period and assume the shares reported for Lambeth in Table 4 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 4 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 20.6% (Column 2, Table 3). 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, $\hat{\beta}_3$ in specification (12), corresponds to,

$$\hat{\beta}_3 = \frac{(possession_1^{NC} + possession_1^C) - (possession_0^{NC} + possession_0^C)}{possession_0^{NC} + possession_0^C} = .206. \quad (14)$$

We now use the relevant statistics from Table 4 to substitute into (14) under various scenarios.

5.3.1 Scenario 1: Increase 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 (14) and substituting in for the ratio of cannabis to non-cannabis possession offences pre-period from Table 4, we see that,

$$\frac{possession_1^{NC} - possession_0^{NC}}{possession_0^{NC}} = \hat{\beta}_3 + \left(1 + \hat{\beta}_3\right) \frac{possession_0^C}{possession_0^{NC}} = .206 + 1.206 \times (4.01) = 5.04. \quad (15)$$

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 (71.6%) and it is assumed that the policy's introduction causes all such offences of cannabis possession to no longer be recorded as an offence.

5.3.2 Scenario 2: Policy already in place

A second more realistic scenario is one in which the policy was *de facto* already in place. Hence there is no change in recorded cannabis offences ($possession_1^C = possession_0^C$). Under this scenario we find the documented increase in drug possession offences can be explained by changes in the possession of other drugs types if,

$$\frac{possession_1^{NC} - possession_0^{NC}}{possession_0^{NC}} = \hat{\beta}_3 \left(1 + \frac{possession_0^C}{possession_0^{NC}} \right) = .206 \times (1 + 4.01) = 1.03, \quad (16)$$

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.

5.3.3 Scenario 3: Increase 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,

$$\frac{possession_1^C - possession_0^C}{possession_0^C} = \hat{\beta}_3 \left(1 + \frac{possession_0^{NC}}{possession_0^C} \right) = .206 \times \left(1 + \frac{1}{4.01} \right) = .257. \quad (17)$$

Hence a 26% increase in cannabis possessions from below the threshold for personnel use to above would explain our findings.

Clearly, perhaps the most realistic scenario combines elements of all three thought 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* already in place, and there is some increase in cannabis possessions. If so, then in the presence of a gateway effect of cannabis consumption leading to the consumption of other illicit drugs, these estimates provide a lower bound on the increase in the market size for these other drugs. What does appear to be 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.²⁹

5.4 Accusations 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, as shown in (8). To pin down empirically whether the policy effects are related to changes in police behavior we next focus on more direct measures of police efficiency. Following the same empirical methodology embodied in specification (12), we estimate the short and long run effects of the LCWS on the

²⁹This 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.

rate of accusations and clear-up rate for drug offences. As we do not necessarily expect individuals to be immediately accused for offences that occur in any given borough and month, we define the accusation rate as the number of accusations in the borough in period t divided by the number of offences committed in month t and the previous quarter within the borough. The clear-up rate is analogously defined.³⁰

The results are presented in Table 5. Columns 1 to 5 focus on the policy effects on the log of the accusation rate, and Columns 6 to 10 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 in the borough, and allow for the autocorrelated error structure described in (13). Column 1 shows that the accusation rate significantly drops by 29.9% ($\hat{\beta}_1$) during the policy period. In the long run this effect is reduced in absolute magnitude, although accusations per drug offence in Lambeth still fall by almost 15% relative to the pre-policy period ($\hat{\beta}_3$). There is not much evidence of declines in accusation rates for drugs crime in London as a whole over these time periods.

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 to 5 exploit this information on sub-offence type. In line with the earlier evidence on drug offence rates, the decline in accusation rates is predominantly driven by significant declines in accusation 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 accusation rates for drug trafficking, or for other drug offences.

Columns 6 to 10 present an analogous set of specifications on clear-up rates for drugs related crime. These provide a pattern of coefficients consistent with the seeming decline in police intensity on drugs crime implied both by the results for offence rates (Tables 2 and 3) and the results in Columns 1 to 5 on accusation 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 32.9%. As with accusation rates in the long run, the relative fall in clear-up rates pre-policy is smaller – at 17.5% – 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.

If we instead define accusation and clear-up rates as being relative to the adult population (rather than the number of offences), and control directly for offences on the right hand side of (12), the estimates for the policy and post-policy increases in Lambeth are of a similar magnitude and still significant, as shown in Table A3. Hence the results not driven purely by an increase in the number of offences (the denominator) during this time.³¹

³⁰Ideally, 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 shown later, all the presented results are robust to just focusing on the number of accused or clear-ups per 1000 of the adult population, so that we are confident the results are not being driven merely by changes in the denominator of the accusation and clear-up rates as defined.

³¹Finally, we note that the results confirm there is no evidence of the Lambeth police altering recorded clear-ups to meet performance targets.

5.5 Interpretation

Overall, the data shows a surge in drugs offences occurring in Lambeth in response to the announcement of the LCWS being a permanent policing strategy. This announcement occurred seven months into the start of the policy experiment. This surge in drug offences occurs largely for offences for drugs possession, with little effect on other drug crimes, and remains well after the official ending of the LCWS. This surge takes place against a backdrop of no similar increases in drug offences London-wide. As the thought experiments show, this is likely to match with a substantial increase in the market size for both cannabis and other illicit drugs in Lambeth.

In terms of changes in police behavior, we find that police effectiveness – as measured in terms of accusation and clear-up rates – appears to have dropped considerably during the policy period, and remained 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.

As shown in Panels B and C of Table A4 (Column 3), 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 there is 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 six months into the LCWS when the policy is announced to be permanent. Hence it is important to be clear that the changes in offences measured in Tables 2 and 3 reflect responses to changes in police intensity, exactly as mapped out in the model developed in Section 2.

Moreover, mapping the evidence to the model, the findings are consistent with the police turning a blind-eye to *all* illicit drugs in response to the specific decriminalization of cannabis. Hence the decline in police intensity against drugs related crime (P_b^d) and the subsequent increase in drugs related crime in Lambeth (C_b^d) summarized in Result 1. We now pursue this intuition and explore whether decriminalization then allows the police to re-allocate effort towards other crime types during and after the policy, as suggested by the Hotelling-style framework.³²

³²Of course, 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.

6 Extended Results

6.1 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 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 (12) for each crime type and so we continue to condition on borough, month, and year fixed effects, time varying socio-demographic characteristics and police operations in the borough, and allow for the autocorrelated error structure described in (13).

Table 6 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 there was no significant rise in total crime within Lambeth ($\hat{\beta}_1 = 0$). Over the same period, total crime was rising in London on average ($\hat{\beta}_0 = .073$). In the post-policy period, the total crime rate in Lambeth significantly fell by 6.9%, 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 (12) for each non-drugs related offence type. Two 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 ($\delta_1 = 0$), decriminalization can still reduce such crimes by freeing up police resources.³³

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 ($\delta_1 > 0$) so they are complements to drug offences, consistent with evidence from other studies [Baker 1998, Corman and Mocan 2000, Entorf and Winker 2008]. Given the long run rise in drug related offences in Lambeth documented in Tables 2 and 3, such complementarity would also then explain why these crimes in particular fall by smaller percentage amounts than other crimes.³⁴

To pin down whether these long run declines in all crime types other than drugs crimes is due

³³As 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.

³⁴However, this interpretation should be treated with caution. 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 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.

to a reallocation of police effort, in Table 7 we present estimates of the short and long run policy effects on our measures of police efficiency: accusations (Panel A) and clear-ups (Panel B). We see that in aggregate, accusation 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 accusations per offence for robbery, despite the rise in robbery offences in the policy period shown in Table 6. Post-policy, there are significant increases in accusation 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 accusation rates for a broad range of crime types (Table 7, Panel A) – that might itself feedback into lowering offence rates (Table 6). 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.³⁵

6.2 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, and drug suppliers, who have incentives to locate closer to Lambeth. In the presence of such drugs tourism, our previous estimates of the policy and post-policy effects in Lambeth might be biased downwards because of the change in the illicit drugs market in neighboring boroughs also caused by the LCWS.³⁶

To gauge this possibility, we first estimate (12) using only observations from Lambeth and its geographic neighbors. Table 8 shows our baseline estimate (Column 1) and this restricted specification (Column 2). In line with there being spillovers onto contiguous boroughs, the point estimates of the policy and post-policy effects within Lambeth are larger when we compare it to its neighbors only rather than the London-wide average.

To check for spillover effects more precisely, we therefore estimate a specification analogous to (12) but additionally control for interactions between the policy and post policy-periods and

³⁵Panels B and C of Table A4 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 accusation rates for burglaries. In contrast, there is an immediate decline in accusation and clear-up rates for drug crime. As the positive effects of accusation rates for non-drug crimes shown in Panel A of Table 7 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.

³⁶Other studies examining the effects of policing on the spatial displacement of crime include Di Tella and Schargrodsky [2004] and Draca *et al* [2008]. These both exploit police redeployments caused by terrorism.

whether the borough is a geographic neighbor of Lambeth ($N_b = 1$) or not ($N_b = 0$),³⁷

$$\begin{aligned} \ln C_{bmy} = & \beta_0 P_{my} + \beta_1 [L_b \times P_{my}] + \beta_2 PP_{my} + \beta_3 [L_b \times PP_{my}] \\ & + \beta_4 [N_b \times P_{my}] + \beta_5 [N_b \times PP_{my}] + \gamma X_{bmy} + \lambda_b + \lambda_m + \lambda_y + u_{bmy}. \end{aligned} \quad (18)$$

As shown in Column 3 of Table 8, there are significant and negative effects on rates of drug-related crime 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 4 estimates the effect separately for each neighbor and shows the previous effect to be driven by two boroughs in particular: Merton and Southwark. Columns 5 and 6 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 (Column 4) and drugs possession offences (Column 5) 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. In the final Section we explore in more depth the likely effects of the LCWS on the aggregate supply of illicit drugs in London as a whole.

Finally, Columns 7 and 8 check for any response in policing intensity in neighboring boroughs. We find mixed evidence for whether the police respond to decriminalization policies in neighbors. On the one hand there are no significant changes in accusation rates in neighbors for drug or other crime types. On the other hand, clear-up rates significantly fall for drug offences in neighbors in both the policy and post-policy period. More research is needed to better understand the policing objectives and responses across boroughs in response to localized police initiatives.

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 further headway in understanding the costs

³⁷>From Figure 2, 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.

and benefits of coordinated such policies across jurisdictions, when mechanisms such as drugs tourism are shut down.

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 AMCD 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.

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.³⁸

³⁸Concerns 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 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”. The government had accepted the other 20 recommendations on drugs policy from the ACMD. It was this fundamental disagreement over the classification of cannabis that led to the eventual sacking of Professor David Nutt who had been appointed as the chairman of the ACMD in January 2008, having previously been Chair of the Technical Committee of the ACMD for seven years. He was sacked after his public pronouncements summarizing the results in Nutt *et al* [2007] and shown in Figure 1B, that the personal harm from cannabis consumption were lower than for legal drugs such

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 ,

$$\ln C_{bmy} = \beta_0 N_{my} + \gamma X_{bmy} + \lambda_b + \lambda_m + \lambda_y + u_{bmy}, \quad (19)$$

where C_{bmy} is the number of 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. The only 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 (19) 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 10 reports the results.

In Column 1 we find drug offence rates rise significantly under the policy, so that as with the LCWS, any fall in cannabis related possession offences ($\Delta\delta_0 < 0$) appears to be more than offset by rises in other types of drug related offence. However Column 2 shows this result is not robust to including an aggregate quadratic time trend. 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].³⁹

The remaining columns check for evidence of a reallocation of police effort towards other crime types. Column 3 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 6.1%, 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.⁴⁰

as alcohol and tobacco.

³⁹The model also highlights that the relative size of drugs markets across boroughs should not change with a nationwide decriminalization. To check for this we explored whether the national policy had differential effects on crime in Lambeth, and found no such effect.

⁴⁰Given 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 that 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 related 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.

In this final Section we probe further the policy implications of our findings. Broadly speaking, three classes of policy have been attempted: criminalization, decriminalization, and legalization. The relative costs and benefits of each approach hinge on two questions: (i) what are the private costs to consumers of illicit drugs of the policy; (ii) what are the social costs of the policy.

On usage, given the lack of reliable data 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 policies should be coordinated among drugs markets that are linked in terms of potential consumers and suppliers.⁴¹ 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. We provide suggestive evidence on this issue in the first subsection below.

On the social costs of decriminalization, 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].⁴²

⁴¹It remains an open question of whether these effects on usage from a localized policy could be mitigated by simultaneously reducing penalties not only for cannabis possession, as most forms of decriminalization allow, but also for cannabis supply, which is typically only associated with policies of legalization. This is because such an approach might provide incentives to drug suppliers to de-couple the supply of cannabis from other illicit drugs, and effectively segment the market [Pudney 2010]. Moreover, we also note that throughout we have found little robust evidence of police forces coordinating their efforts across jurisdictions. If they were to do so, this might mitigate some of the private and social costs of decriminalization.

⁴²On 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

The total social cost of decriminalization should, over time, be reflected in house prices. To conclude our analysis, in the second subsection below we therefore estimate the association between house prices and the decriminalization of cannabis in Lambeth.

7.1 The Illicit Drugs Market in London

To better understand whether the underlying market in London as a whole is altered by the LCWS, we use the method of synthetic controls developed in Abadie and Gardeazabal [2003], to pick up large changes in drugs crime in other London boroughs apart from Lambeth and its neighbors. This method matches Lambeth with a weighted average of other London boroughs – a synthetic control – based on pre-policy predictors of the outcome of interest: the drugs offence rate. More precisely, for a given set of pre-policy borough characteristics, to construct the synthetic control we simultaneously choose: (i) the relative weight to give to each characteristic; and, (ii) the relative weight to give to each control borough in the synthetic control, in order to minimize the difference in drug offence rates between Lambeth and its synthetic control during the pre-policy period.⁴³

We use the socio-demographic variables X_{bmy} obtained from the *QLFSLAD* to predict the pre-policy drugs crime rate. The resulting implied weights used to construct the synthetic control are then as follows: Camden (.101), Greenwich (.121), Hammersmith and Fulham (.076), Haringey (.290), Islington (.246), Southwark (.052), Wandsworth (.062), and Westminster (.052). All other boroughs have zero weight.

Figure 4A then shows drug offences in Lambeth versus those of its synthetic control. Three points are of note. First, in the pre-policy period to the right of the first vertical dashed line, we see that reassuringly, the drug offence rate in Lambeth is closely matched by its synthetic control. Second, during the policy period, drug offences diverge considerably between Lambeth and its synthetic control. This divergence remains in the post-policy period with no evidence of a convergence in the two time series. Third, among the synthetic controls, there is a jump in drug crime precisely six months into the policy. The timing of this jump exactly matches the jump in Lambeth.

To explore this jump among the synthetic controls in more detail, we note first that the borough with the highest weight among the synthetic controls is Haringey. We then repeat the analysis

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.

⁴³As discussed in more detail in Abadie and Gardeazabal [2003], in our setting we have one treatment unit (Lambeth) and J controls corresponding to all other London boroughs. X_1 is a $(K \times 1)$ vector of pre-treatment characteristics of Lambeth, and X_0 is a $(K \times J)$ matrix of the same characteristics for the control boroughs. The two elements we have to choose are: (i) V , a $(K \times K)$ diagonal matrix with non-negative components, where each element measures the relative importance of each of the pre-treatment characteristics; (ii) $W = (w_1 \dots w_J)'$, a $(J \times 1)$ vector of non-negative weights, where w_j is the weight placed on control borough j in constructing the synthetic control. The vector of weights W^* is chosen to minimize $(X_1 - X_0 W)' V (X_1 - X_0 W)$, subject to: (i) $w_j \geq 0 \forall j$; (ii) $\sum_j w_j = 1$. The complicating factor is that W^* itself depends on V and so V is chosen to minimize $(Z_1 - Z_0 W^*(V))' (Z_1 - Z_0 W^*(V))$, where T is the number of pre-treatment periods, Z_0 is a $(T \times 1)$ vector of outcome variables during this period and Z_1 the corresponding $(T \times J)$ matrix for control boroughs. The actual weights used to construct the synthetic control are then $W^* = W^*(V^*)$. We then compare the series for our outcome of interest in Lambeth, Y_1 , with the outcome for the synthetic control, $Y_0 W^*$.

but drop Haringey from the sample of potential synthetic controls. The result in Figure 3B is striking. All of the previously found jump in drug crime offences among synthetic controls can be attributed to an increase in drug crimes in Haringey.⁴⁴

Following Abadie *et al* [2009], we use placebo tests to assess whether the result could be driven by chance, in the same spirit as Bertrand *et al* [2004]. To do so we apply the synthetic control method to all the other London boroughs and in each case, allow Lambeth to be part of the pool of control boroughs from which the synthetic control is constructed. Figure 5 plots the difference for each borough, between its drug crime rate and that of its synthetic control. We do this both assuming Haringey should be included in the sample of control boroughs (Figure 5A), and excluding Haringey from the pool of controls (Figure 5B). Figure 6 represents the same information by showing the ratio of the square root of the mean squared prediction error (RMSPE) between the post and pre policy periods for each borough. A larger ratio indicates the post policy difference between a borough and its synthetic control diverges further from the same difference in the pre-policy period. Again Figures 6A and 6B show this histogram of ratios across boroughs including and excluding Haringey from the control pool.

We see that when Haringey is included in the control pool drug offences diverge sharply post-policy between Lambeth and its synthetic control (Figure 5A). Measured in terms of the RMPSE, the same is true for Haringey (Figure 6A), although Lambeth and Haringey have the largest RMPSE ratios of all boroughs. The results are sharper when Haringey is dropped from the control pool as Figure 4 suggested ought to be so. In this case, the evidence shows Lambeth to be a clear outlier relative to its synthetic control (Figure 5B). The ratio of RMPSE's for other boroughs and their synthetic control are all considerably lower (Figure 6B).

7.1.1 A Closer Look At Haringey

Taken together the synthetic control results imply that there were large effects of the LCWS in Lambeth, that caused a structural break in its rate of drug offences. Second, these results strongly hint at the LCWS having a similar effect in Haringey. To explore these effects in Haringey, we estimate a specification analogous to (18) but define the dummy variable N_b to be equal to one for Haringey, not the geographic neighbors of Lambeth. Table 9 presents the estimates of the effects of the short and long run effects of the LCWS allowing for differential effects in both Lambeth and Haringey. Four results are of note.

First, the estimated effects of the LCWS in Haringey both during policy period and post-policy period, are both positive and significant. Moreover, the point estimates are both larger than the corresponding estimates in Lambeth (Column 1). Decomposing by drug sub-offence types, we see that the previous result is robust to restricting the sample to when sub-offence data is available (Column 2). More substantively, we see that in Haringey there are significant increases in offences related to drug possession (Column 3) during and after the policy. Relative to Lambeth, the

⁴⁴The fact that the introduction of the LCWS in Lambeth appears causally related to drug offences in Haringey six months later further helps allay concerns of there being reverse causality between anticipated drug offences in Lambeth and the introduction of the LCWS.

effects while the LCWS is in place are larger in Haringey, and smaller in the post-policy period. Column 4 shows that there is a significant increase of 7.3% in drug trafficking offences in Haringey post-policy, and effect that does not occur in Lambeth.⁴⁵

One interpretation of the similar results between Lambeth and Haringey is that the police in Haringey implicitly decided to turn a blind-eye to illicit drugs markets at exactly the same time as Lambeth introduced the LCWS. Although this might not have been an officially announced policy, the effect on drugs crime – with the exception of the significant increase in drug trafficking – was qualitatively similar to those in Lambeth. To pursue this intuition the second set of results in Table 9 then examine the relationship between the LCWS and total crime rates, as well as for each crime type in Haringey.

Columns 5 to 13 show that in the long run, Haringey experiences significant *increases* in crime (Columns 5 and 6). These increases occur for 7 out of the 9 crime types. This is in complete contrast to Lambeth which although it experiences similar increases in drug possession offences, experiences declines in all other crimes. Taken together the results do not suggest that the police in Haringey turned a blind-eye to illicit drugs and were then able to effectively reallocate their resources towards other crime types as in Lambeth.

Final confirmation of this is presented in the third set of results in Table 9 examining the relationship between the LCWS and accusation and clear-up rates in Haringey. Columns 14 to 17 show that, unlike in Lambeth, there appears to be no change in police behavior with regards to either measure, for neither drugs crime nor other types of crime. It therefore appears unlikely that the results reflect any coordinated response of the police in Haringey to what was occurring in Lambeth. In line with the results in Table 8, the evidence here again suggests a lack of police coordination across boroughs.

One interpretation of these results is that Haringey is an important route for illicit drugs supply into London as a whole and this is affected by the LCWS. Such an interpretation lies outside of the bounds of the model developed because there we made the simplifying assumption that all consumers only choose where to purchase drugs from, they do not choose *whether* to consume drugs at all. As is intuitive, extending the model in this direction makes precise that decriminalization, by lowering the price of drugs, can induce new consumers to enter the drugs market. In equilibrium, the aggregate drugs supply has to rise and our findings might indicate one geographic route through which this occurs. This linkage is worth pursuing in further research.⁴⁶

⁴⁵If we repeat this type of analysis for Camden for example – the borough with the second highest weight in Lambeth’s control – we find no such effects of the LCWS.

⁴⁶To be clear, we do not claim that the increased drug trafficking that occurs in Haringey post-policy is intended to be directly supplied to Lambeth. It might for example merely replace drug trafficking that was previously occurring in the geographic neighbors to Haringey. We can only speculate on whether there are specific links between drugs supply between Lambeth and Haringey. Two avenues of research appear worth pursuing along these lines. First, Haringey and Lambeth lie at opposite ends of the Victoria Underground Line. It might therefore be less risky to transport drugs between Haringey and Lambeth, relative to other boroughs. Second, a recent media report in *The Observer* on October 25th 2009, suggested a recent spike in gun crime in Haringey was caused by feuds between drugs gangs there. This has led to the Metropolitan Police’s decision to, for the first time, deploy armed police onto the streets not only in Haringey where the drugs gangs are based, but Brixton – a neighborhood in Lambeth – where gun crime has also risen.

7.2 House Prices

The total social cost of decriminalization should be reflected in house prices [Rosen 1974].⁴⁷ 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 (12). 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, but to begin with we estimate a panel data specification of the form,⁴⁸

$$\begin{aligned} \ln h_{sqy} = & \beta_0 P_{qy} + \beta_1 [L_b \times P_{qy}] + \beta_2 PP_{qy} + \beta_3 [L_b \times PP_{qy}] \\ & + \gamma X_{bqy} + \lambda_s + \lambda_q + \lambda_y + u_{sqy}, \end{aligned} \quad (20)$$

where h_{sq} is the mean house price sale for terraced houses in zip code sector s in quarter q , deflated to 1995 Q1 prices;⁴⁹ P_q , PP_q are dummies for the policy and post-policy quarters 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_{bq} we continue to control for socio-demographic and police operation controls, as in (12). We also allow for a borough specific time trend ($\lambda_b \times t$) 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 11 reports the results. Column 1 shows that in the long run after the LCWS is first introduced, house prices fall by 4.3% 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 and significant house price effect in Lambeth. As expected, we no longer find that the LCWS had an almost immediate effect on house prices during its period of formal operation. 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 6. 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.⁵⁰

⁴⁷Following Rosen [1974], hedonic price functions have been used to estimate households' valuation of non-market amenities, including local crime rates [Gibbons 2004, Linden and Rockoff 2005], school quality [Black 1999], air quality [Chay and Greenstone 2005], and health risks [Davis 2004].

⁴⁸The 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.

⁴⁹House 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.

⁵⁰To get a sense of the magnitudes of these effects we compare our results to Gibbons [2004] who estimates that

As house prices are available by zip-code sector, the remaining specifications examine whether there are heterogeneous effects of decriminalization on house prices within Lambeth, depending on the location of crime. To do so we identify which zip-code sectors within each borough have higher than median crime rates and define these zip codes to be 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 London boroughs over time.⁵¹

Column 3 shows that all of the long run negative effect of decriminalization on house prices within Lambeth occurs in these crime hot-spot zip codes, and the magnitude of the fall, at -13.4%, 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. This pattern of results is repeated in Column 4 when we define crime hot-spot zip codes in terms of drugs crime in particular. In contrast we 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 as maisonettes, rather than terraced houses. Hence it is reassuring to find the triple-difference estimates to be very similar for such properties, as Column 5 shows.

Finally Column 6 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, and that we plan to investigate in future research.

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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 6).

⁵¹The 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.

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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 (.024) | .830 (.100) | 1.133 (.028) | .405*** (.077) | .303*** (.075) |
| London (excluding Lambeth) | .284 (.005) | .395 (.014) | .488 (.010) | .111*** (.016) | .093*** (.020) |
| Neighbours to Lambeth | .422 (.021) | .481 (.047) | .661 (.041) | .059 (.064) | .180** (.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.

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) Unconditional | (2) Fixed Effects | (3) Controls | (4) Prais-Winsten Panel Specific AR(1) | (5) Borough Specific Quadratic Time Trend | (6) Police Operations | (7) Spatial Error Model | (8) 2-Part Policy Period | (9) Dynamic Response |
|---|-------------------|-------------------|-------------------|--|---|--------------------------|----------------------------|-----------------------------|-------------------------|
| Lambeth x Policy Period | .163*** (.055) | .166*** (.047) | .136*** (.047) | .157*** (.038) | .110** (.044) | .135*** (.038) | .145*** (.032) | | .133*** (.036) |
| Policy Period | .091*** (.010) | -.007 (.008) | -.007 (.008) | .008 (.022) | .000 (.021) | .005 (.021) | -.009 (.016) | .008 (.021) | .008 (.021) |
| Lambeth x Post-Policy Period | .272*** (.023) | .273*** (.016) | .247*** (.016) | .259*** (.027) | .202*** (.059) | .241*** (.027) | .222*** (.020) | .249*** (.025) | |
| Post-Policy Period | .144*** (.007) | .006 (.015) | .006 (.015) | .003 (.030) | -.008 (.031) | .002 (.030) | .016 .025 | .009 (.030) | |
| Lambeth x Policy Period [1-6 months] | | | | | | | | .038 (.046) | |
| Lambeth x Policy Period [7-13 months] | | | | | | | | .215*** (.043) | |
| Lambeth x Post-Policy Period [1-12 months] | | | | | | | | | .238*** (.038) |
| Lambeth x Post-Policy Period [13-24 months] | | | | | | | | | .298*** (.038) |
| Lambeth x Post-Policy Period [25-36 months] | | | | | | | | | .231*** (.039) |
| Lambeth x Post-Policy Period [37-48 months] | | | | | | | | | .163*** (.054) |
| Short and Long Run Policy Effects are Equal in Lambeth [p-value] | [.042] | [.027] | [.022] | [.013] | [.051] | [.008] | | | |
| First and Second Half Policy Effects are Equal in Lambeth [p-value] | | | | | | | | [.001] | |
| First and Fourth Year Post Policy Effects are Equal in Lambeth [p-value] | | | | | | | | | [.231] |
| Borough, Month, Year Fixed Effects | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Socio-demographic Controls | No | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Police Operation Controls | No | No | No | No | No | Yes | Yes | Yes | Yes |
| R-squared | .147 | .748 | .752 | .732 | .812 | .737 | .765 | .740 | .742 |
| Observations | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 4681 | 4810 | 4810 |

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, as drug offence data was not available for this period. In Columns 1 to 3, robust standard errors are reported. In Column 4 onwards, 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. In all columns except Column 7, 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. In Column 3 onwards, 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. In Column 6 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 (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). In column 7 a contiguity spatial weighting matrix W was used, where neighbouring boroughs are coded as 1, non-neighbouring boroughs as 0. The borough of Sutton was dropped for the Spatial Error Model, as a balanced panel was required, and 23 months of data are missing for this borough. At the foot of columns 1-6 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 reported at the base of Column 8 is 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. Finally, the p-value reported in Column 9 is 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 Recorded Drugs Sub-Category Offences

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

| | (1) Drugs Total (Restricted Sample) | (2) Possession of Drugs | (3) Drug Trafficking | (4) Other Drug Offences | (5) Possession of Drugs | (6) Possession of Drugs |
|--|--|----------------------------|----------------------|----------------------------|----------------------------|----------------------------|
| Lambeth x Policy Period | .101** (.047) | .105** (.045) | .008 (.019) | -.000 (.003) | | .104** (.041) |
| Policy Period | -.015 (.014) | -.021 (.013) | .003 (.005) | -.001 (.001) | -.017 (.013) | -.014 (.013) |
| Lambeth x Post-Policy Period | .201*** (.039) | .206*** (.038) | .012 (.015) | .005** (.003) | .222*** (.030) | |
| Post-Policy Period | -.018 (.022) | -.023 (.020) | .001 (.008) | -.001 (.001) | -.016 (.020) | |
| Lambeth x Policy Period [1-6 months] | | | | | .012 (.045) | |
| Lambeth x Policy Period [7-13 months] | | | | | .196*** (.045) | |
| Lambeth x Post-Policy Period [1-12 months] | | | | | | .185*** (.043) |
| Lambeth x Post-Policy Period [13-24 months] | | | | | | .262*** (.043) |
| Lambeth x Post-Policy Period [25-36 months] | | | | | | .216*** (.044) |
| Lambeth x Post-Policy Period [37-48 months] | | | | | | .155*** (.056) |
| Share of All Offences in Pre-policy Period | - | .855 | .141 | .012 | .844 | .844 |
| Short and Long Run Policy Effects are Equal in Lambeth [p-value] | [.016] | [.012] | [.793] | [.044] | | |
| First and Second Half Policy Effects are Equal in Lambeth [p-value] | | | | | [.000] | |
| First and Fourth Year Post Policy Effects are Equal in Lambeth [p-value] | | | | | | [.611] |
| Borough, Month, Year Fixed Effects | 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 | .863 | .855 | .456 | .356 | .861 | .860 |
| Observations | 2233 | 2233 | 2233 | 2233 | 2233 | 2233 |

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. 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 share of all offences in the pre-policy period is the average ratio of the drug offence type to all drug offences in the period from January 2000 until June 2001. At the foot of Columns 1-4 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 reported at the base of Column 5 is 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 reported in Column 6 is 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 4: Detailed Drugs Offences, Pre-policy Period

Means and standard errors in parentheses

| | Lambeth | Non-Lambeth Boroughs | Difference [p-value] |
|---|----------------|-------------------------|-------------------------|
| Total drugs offences per 1000 of adult population | .537 (.109) | .393 (.285) | [.084] |
| Share of drugs offences related to class A drugs | .196 (.072) | .125 (.075) | [.001] |
| Share of drugs offences related to class B drugs (includes cannabis) | .776 (.077) | .840 (.086) | [.012] |
| Share of drugs offences related to class C drugs | .007 (.007) | .008 (.019) | [.788] |
| Share of drug offences relating to any cannabis offences | .714 (.086) | .776 (.099) | [.034] |
| Share of cannabis offences relating to having possession of cannabis | .913 (.041) | .918 (.060) | [.771] |
| Share of cannabis offences relating to having possession of cannabis with intent to supply | .045 (.025) | .043 (.042) | [.848] |
| Share of cannabis offences relating to production/being concerned in production of cannabis | .026 (.019) | .020 (.026) | [.442] |
| Share of cannabis offences relating to supply or offer to supply cannabis | .012 (.009) | .017 (.025) | [.513] |
| Share of cannabis offences relating to permitting premises to be used for unlawful purpose (cannabis) | .004 (.008) | .002 (.008) | [.500] |

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.

Table 5: The Effect of The Policy on Accused and Clear Up Rates for Drugs

Prais-Winsten Regression Estimates

| | Log (recorded accused 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) 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 | -.299*** (.059) | -.288*** (.069) | -.396*** (.086) | -.034 (.051) | .094 (.107) | -.329*** (.059) | -.303*** (.080) | -.405*** (.100) | -.031 (.074) | -.004 (.110) |
| Policy Period | -.009 (.021) | -.002 (.022) | .000 (.023) | .006 (.040) | -.132*** (.050) | .018 (.044) | -.053** (.027) | -.057** (.027) | -.020 (.040) | -.121** (.058) |
| Lambeth x Post-Policy Period | -.148*** (.047) | -.139** (.060) | -.192** (.082) | -.054 (.040) | .071 (.082) | -.175*** (.043) | -.136** (.068) | -.178** (.087) | -.037 (.061) | .040 (.090) |
| Post-Policy Period | -.014 (.032) | -.001 (.036) | -.007 (.037) | .061 (.065) | -.327*** (.082) | .054 (.068) | -.089** (.044) | -.095** (.044) | -.018 (.067) | -.293*** (.096) |
| Share of All Accused, or Clear ups, in Pre-policy Period | | - | .840 | .148 | .009 | - | - | .845 | .139 | .011 |
| Short and Long Run Policy Effects are Equal in Lambeth [p-value] | [.009] | [.014] | [.007] | [.670] | [.816] | [.019] | [.022] | [.012] | [.920] | [.674] |
| 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 | .823 | .809 | .846 | .217 | .058 | .430 | - | .605 | - | .045 |
| Observations | 2898 | 2233 | 2233 | 2233 | 2224 | 4495 | 1914 | 1914 | 1914 | 1899 |

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 accused 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 5 is 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. The dependent variable in Columns 6 to 10 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. 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 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 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: 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 | .043 (.027) | .035 (.027) | -.018 (.033) | -.049 (.035) | .138*** (.045) | .010 (.043) | .106*** (.033) | -.039 (.044) | -.019 (.026) |
| Policy Period | .073** (.033) | .073** (.033) | .007 (.035) | .016* (.009) | .017 (.016) | .066** (.028) | .090*** (.032) | -.001 (.028) | .023 (.022) |
| Lambeth x Post-Policy Period | -.069*** (.020) | -.093*** (.021) | -.058** (.025) | -.055** (.026) | -.064* (.035) | -.172*** (.034) | .003 (.026) | -.144*** (.031) | -.048** (.019) |
| Post-Policy Period | .107** (.046) | .109** (.045) | .051 (.049) | .029** (.013) | -.006 (.024) | .068* (.040) | .138*** (.045) | -.008 (.040) | .016 (.031) |
| Share of All Offences in Pre-policy Period | - | - | .160 | .009 | .040 | .119 | .401 | .088 | .160 |
| Short and Long Run Policy Effects are Equal in Lambeth [p-value] | [.000] | [.000] | [.258] | [.873] | [.000] | [.000] | [.004] | [.025] | [.300] |
| 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 | .962 | .637 | .869 | .946 | .987 | .880 | .973 |
| Observations | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 |

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. 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 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 7: The Effect of The Policy on Other Crime Types

Prais-Winsten Regression Estimates

| A. Dependent Variable: Log (recorded accused 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 | -.017*** (.006) | -.003 (.005) | .009 (.011) | .011 (.038) | .029** (.013) | -.016 (.010) | -.005 (.004) | -.002 (.011) | -.008 (.007) |
| Policy Period | -.002 (.006) | -.001 (.005) | .007 (.010) | .027** (.014) | -.011 (.011) | .007 (.006) | -.004 (.004) | .005 (.006) | -.008** (.004) |
| Lambeth x Post-Policy Period | .010** (.005) | .012*** (.004) | .030*** (.008) | .086*** (.026) | .031*** (.010) | .019** (.008) | .002 (.003) | .020** (.008) | .002 (.005) |
| Post-Policy Period | -.021** (.009) | -.018** (.008) | -.006 (.015) | .019 (.021) | -.027 (.017) | -.006 (.010) | -.020*** (.006) | -.001 (.010) | -.025*** (.006) |
| Share of All Accused in Pre-policy Period | - | - | .220 | .015 | .047 | .077 | .332 | .048 | .092 |
| Short and Long Run Policy Effects are Equal in Lambeth [p-value] | [.000] | [.003] | [.059] | [.045] | [.849] | [.001] | [.052] | [.044] | [.113] |
| 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 | .925 | .922 | .864 | .265 | .451 | .626 | .901 | .591 | .687 |
| Observations | 2898 | 2898 | 2898 | 2898 | 2898 | 2898 | 2898 | 2898 | 2898 |

| 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 | -.037** (.015) | -.026* (.015) | -.035 (.024) | -.089 (.059) | -.030 (.021) | -.046* (.027) | -.016 (.013) | -.025 (.025) | -.019 (.014) |
| Policy Period | .005 (.014) | .006 (.013) | .050 (.032) | .090*** (.033) | .011 (.014) | .015 (.014) | .000 (.014) | .028 (.020) | -.003 (.012) |
| Lambeth x Post-Policy Period | -.014 (.012) | -.017 (.013) | -.013 (.019) | -.044 (.042) | -.027* (.016) | -.006 (.020) | -.018 (.011) | -.034* (.019) | -.014 (.011) |
| Post-Policy Period | .001 (.020) | .004 (.019) | .099** (.046) | .167*** (.050) | .025 (.021) | .008 (.020) | -.005 (.020) | .069** (.031) | -.010 (.017) |
| Share of All Clear-Ups in Pre-policy Period | - | - | .290 | .018 | .034 | .080 | .279 | .053 | .098 |
| Short and Long Run Policy Effects are Equal in Lambeth [p-value] | [.044] | [.581] | [.412] | [.502] | [.913] | [.182] | [.905] | [.773] | [.712] |
| 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 | .728 | .699 | .859 | .436 | .432 | .453 | .611 | .651 | .557 |
| Observations | 4495 | 4495 | 4495 | 4494 | 4494 | 4492 | 4491 | 4494 | 4491 |

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 accused 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 accused 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. 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 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 covered 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 8: Crime in Lambeth and its Neighboring Boroughs

Panel Regression Estimates

| | Drugs Offences, Total | | | | Drugs Offences, Detailed | | Accused | | Clear-ups | |
|---|-----------------------|---------------------------|----------------------|--------------------|--------------------------|----------------------|--------------------|--------------------|-------------------|--------------------|
| | (1) Baseline | (2) Relative to Neighbors | (3) Neighbors Effect | (4) Spillovers | (5) Possession of Drugs | (6) Drug Trafficking | (7) Drugs | (8) All Non Drugs | (9) Drugs | (10) All Non Drugs |
| Lambeth x Policy Period | .135*** (.038) | .171*** (.046) | .128*** (.037) | .129*** (.038) | .103** (.046) | .005 (.019) | -.299*** (.060) | -.334*** (.061) | -.004 (.006) | -.027* (.015) |
| Policy Period | .005 (.021) | -.044 (.036) | .011 (.021) | .009 (.021) | -.019 (.013) | .006 (.006) | -.010 (.021) | .018 (.045) | -.000 (.005) | .006 (.013) |
| Lambeth x Post-Policy Period | .241*** (.027) | .271*** (.037) | .238*** (.027) | .239*** (.027) | .206*** (.039) | .012 (.015) | -.151*** (.048) | -.173*** (.044) | .012*** (.004) | -.015 (.012) |
| Post-Policy Period | .002 (.030) | -.037 (.048) | .006 (.030) | .002 (.030) | -.024 (.019) | .002 (.009) | -.013 (.032) | .050 (.068) | -.017** (.008) | .001 (.018) |
| Neighboring Borough x Policy Period | | | -.042** (.021) | | | | .001 (.015) | -.015 (.030) | -.006** (.003) | -.004 (.006) |
| Neighboring Borough x Post-Policy Period | | | -.031* (.017) | | | | -.016 (.010) | -.005 (.021) | -.004* (.002) | -.001 (.004) |
| Croydon x Policy Period | | | | -.051** (.024) | -.016 (.027) | -.010 (.013) | | | | |
| Croydon x Post-Policy Period | | | | -.055*** (.019) | -.053** (.023) | -.007 (.010) | | | | |
| Merton x Policy Period | | | | -.089*** (.023) | -.055*** (.021) | -.018*** (.006) | | | | |
| Merton x Post-Policy Period | | | | -.082*** (.019) | -.059*** (.017) | -.010* (.005) | | | | |
| Southwark x Policy Period | | | | -.083 (.064) | -.099** (.040) | -.051*** (.018) | | | | |
| Southwark x Post-Policy Period | | | | -.035 (.053) | -.012 (.032) | -.006 (.014) | | | | |
| Wandsworth x Policy Period | | | | .018 (.050) | .053* (.028) | .001 (.013) | | | | |
| Wandsworth x Post-Policy Period | | | | -.036 (.042) | .016 (.022) | -.002 (.010) | | | | |
| Westminster x Policy Period | | | | .040 (.084) | -.011 (.063) | .003 (.027) | | | | |
| Westminster x Post-Policy Period | | | | .174* (.090) | .105 (.065) | .010 (.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 | 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 | .737 | .792 | .738 | .741 | .861 | .460 | .823 | .424 | .922 | .721 |
| Observations | 4810 | 906 | 4810 | 4810 | 2233 | 2233 | 2898 | 4491 | 2898 | 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. In Column 2 we use only observations from Lambeth and its neighboring boroughs.

Table 9: Haringey

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

Panel Regression Estimates

| | (1) Drugs Total (Full Sample) | (2) Drugs Total (Restricted Sample) | (3) Possession of Drugs | (4) Drug Trafficking | Other Offences | | | | | | | | | Accused | | Clear-ups | |
|------------------------------------|----------------------------------|---|----------------------------|-------------------------|---------------------------|---------------------------------|---------------------------------------|------------------------|-------------------|--------------------|----------------------------|--------------------------|-------------------------|--------------------|-----------------------|--------------------|-----------------------|
| | | | | | (5) Total (with drugs) | (6) Total (without drugs) | (7) Violence Against the Person | (8) Sexual Offences | (9) Robbery | (10) Burglary | (11) Theft and Handling | (12) Fraud or Forgery | (13) Criminal Damage | (14) Drugs | (15) All Non Drugs | (16) Drugs | (17) All Non Drugs |
| Lambeth x Policy Period | .143*** (.038) | .120** (.050) | .124** (.048) | .008 (.020) | .048* (.027) | .039 (.027) | -.018 (.033) | -.046 (.035) | .137*** (.045) | .014 (.043) | .111*** (.033) | -.038 (.044) | -.016 (.026) | -.296*** (.058) | -.003 (.005) | -.323*** (.060) | -.026* (.014) |
| Lambeth x Post-Policy Period | .247*** (.027) | .207*** (.042) | .206*** (.041) | .015 (.016) | -.062*** (.020) | -.087*** (.021) | -.056** (.025) | -.052** (.026) | -.066* (.035) | -.166*** (.034) | .011 (.026) | -.134*** (.031) | -.044** (.019) | -.150*** (.047) | .012*** (.004) | -.174*** (.043) | -.016 (.012) |
| Haringey x Policy Period | .329*** (.050) | .182*** (.067) | .214*** (.079) | -.001 (.032) | .122*** (.038) | .084** (.038) | -.003 (.031) | .055** (.024) | -.006 (.049) | .056 (.043) | .171*** (.037) | -.050 (.056) | .053 (.036) | .054 (.037) | .012* (.007) | .094 (.065) | -.000 (.021) |
| Haringey x Post-Policy Period | .318*** (.033) | .173*** (.054) | .115* (.067) | .073*** (.025) | .123*** (.030) | .101*** (.031) | .018 (.023) | .030* (.016) | -.064* (.038) | .074** (.031) | .184*** (.028) | .107*** (.036) | .052* (.029) | -.038 (.027) | -0.006 (.006) | -.023 (.045) | -.023 (.018) |
| Policy Period | -.005 (.021) | -.016 (.013) | -.021* (.012) | .004 (.005) | .070** (.033) | .071** (.033) | .007 (.035) | .014* (.009) | .017 (.016) | .065** (.028) | .086*** (.032) | .001 (.028) | .021 (.022) | -.011 (.020) | -.002 (.005) | .012 (.045) | .005 (.012) |
| Post-Policy Period | .002 (.030) | -.015 (.021) | -.017 (.019) | .001 (.008) | .105** (.046) | .106** (.045) | .050 (.049) | .027** (.013) | -.004 (.024) | .066* (.040) | .134*** (.045) | -.013 (.040) | .014 (.031) | -.013 (.032) | -.018** (.008) | .052 (.068) | .002 (.018) |
| Borough, Month, Year Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Socio-demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Police Operation Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| R-squared | .763 | .880 | .874 | .471 | .994 | .994 | .962 | .640 | .869 | .947 | .987 | .881 | .973 | .821 | .922 | .407 | .721 |
| Observations | 4810 | 2233 | 2233 | 2233 | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 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, as drug offence 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).

Table 10: The Nationwide Decriminalization of Cannabis

Prais-Winsten Regression Estimates

| | Offences | | | Accused | | Clear-ups | |
|--|------------------------------|---------------------------|---------------------------------|--------------------|------------------------------|----------------|------------------------------|
| | (1) Drugs, No Time Trends | (2) Drugs, Time Trends | (3) All Non-Drug Crime Types | (4) Drugs | (5) All Other Crime Types | (6) Drugs | (7) All Other Crime Types |
| General Equilibrium Policy Period | .141*** (.029) | -.027 (.032) | .037* (.020) | -.061*** (.020) | .008* (.004) | .004 (.025) | .013 (.011) |
| Borough and Month Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Aggregate Quadratic Time Trend | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Socio-demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Police Operation Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| R-squared | .727 | .836 | .996 | .852 | .913 | .035 | .870 |
| Observations | 3739 | 3739 | 3739 | 3227 | 3227 | 2300 | 2300 |

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. 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. Accused 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 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 11: The Effect of The Policy on House Prices – Terraced Houses**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) Crime Hotspot | (4) Drug Crime Hotspot | (5) Flats and Maisonettes | (6) Neighbors |
|--|--------------------------------|--|----------------------|---------------------------|------------------------------|--------------------|
| Lambeth x Policy Period | .027* (.014) | -.020 (.019) | .018 (.020) | .030 (.038) | .015 (.022) | .008 (.038) |
| Policy Period | -.022*** (.007) | -.022*** (.007) | -.055*** (.012) | -.052*** (.011) | -.037*** (.016) | -.038*** (.013) |
| Lambeth x Post-Policy Period | -.042** (.016) | -.111*** (.035) | .003 (.029) | -.001 (.031) | .087*** (.028) | -.035 (.032) |
| Post-Policy Period | -.022* (.012) | -.024** (.012) | -.084*** (.014) | -.089*** (.016) | -.101*** (.017) | -.064*** (.018) |
| Lambeth x Hotspot | | | -.088* (.047) | -.089* (.047) | .003 (.037) | .647*** (.055) |
| Hotspot | | | -.011 (.015) | .032 (.025) | -.067** (.027) | -.671*** (.048) |
| Lambeth x Policy Period x Hotspot | | | -.045** (.018) | -.062* (.036) | -.033 (.030) | -.062* (.036) |
| Policy Period x Hotspot | | | .036*** (.012) | .033*** (.011) | .032** (.016) | .034*** (.012) |
| Lambeth x Post-Policy Period x Hotspot | | | -.132*** (.018) | -.134*** (.022) | -.202*** (.030) | -.137*** (.023) |
| Post-Policy Period x Hotspot | | | .066*** (.011) | .073*** (.014) | .080*** (.016) | .076*** (.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 | Yes |
| R-squared | .946 | .951 | .951 | .951 | .930 | .952 |
| Observations | 17336 | 17336 | 17336 | 17336 | 20712 | 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. Hotspots were created based on the total offences (excluding drugs and other notifiable), namely if the subdivision 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-1/2008/080221/11/ |
| 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-Castile | 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/ |
| 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 | Altwick | Haringey | 16/09/2002 | 13/10/2002 | Street Crime | http://www.mpa.gov.uk/downloads/publications/annualrep2002-03.pdf | www.haringeyccpg.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-1/2005/050915/07/ |
| 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-agm06/#2002 | http://ken.3cdn.net/d23b2ee136d273b37d_xm6bhcgf.pdf |
| Other Sources - ref URL | Kartel | 11 Boroughs | | 25/02/2008 | | http://www.mpa.gov.uk/committees/mpa/2008/080529-agm06/#2004 | |
| Other Sources - ref URL | Coalmont | Southward, Lambeth, Lewisham | | | Gun Crime | http://www.mpa.gov.uk/committees/x-eod/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 | Halfax 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/#2003 | |
| 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/rparticle.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 | Tullbardine | | 04/2003 | | Crime Around Heathrow | http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf | |
| MPA - Annual Reports | Grafton | | | | Anti-terror | http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf | |
| MPA - Annual Reports | Bright Star | | | | Child Sex Abuse | http://www.mpa.gov.uk/downloads/publications/annualrep2003-04.pdf | |
| MPA - Annual Reports | Amethyst | | | | Drugs | http://www.mpa.gov.uk/downloads/publications/annualrep2004-05.pdf | |
| MPA - Annual Reports | Nemo | | | | Drugs | http://www.mpa.gov.uk/downloads/publications/annualrep2004-05.pdf | |
| MPA - Annual Reports | Vanadium | | | | Bus Crime | http://www.mpa.gov.uk/downloads/publications/annualrep2006-07.pdf | |
| MPA - Annual Reports | Chicago | | | | Bus Vandalism | http://www.mpa.gov.uk/downloads/publications/annualrep2006-07.pdf | |
| MPA - Annual Reports | BusTag | | | | Anti-terror | 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 | Oeramp | | | | 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.haringeyccpg.org.uk/documents/CPG%20police%20report%20Apr%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 | .084* (.044) | .094** (.045) | .110** (.052) | .136*** (.040) | .097** (.038) | .132*** (.035) | | .136*** (.038) |
| Policy Period | .010 (.021) | .015 (.020) | -.034*** (.012) | .005 (.023) | -.007 (.013) | .009 (.018) | | .003 (.021) |
| Lambeth x Post-Policy Period | .128*** (.044) | .192*** (.058) | .229*** (.055) | .246*** (.029) | .200*** (.029) | .228*** (.025) | | .241*** (.027) |
| Post-Policy Period | .015 (.030) | .018 (.030) | -.052** (.022) | .004 (.032) | -.007 (.020) | .023 (.027) | | .001 (.030) |
| Lambeth x Placebo Policy Period | | | | | | | .032 (.042) | |
| Placebo Policy Period | | | | | | | -.016 (.023) | |
| Lambeth x Placebo Post-Policy Period | | | | | | | .214*** (.027) | |
| Placebo Post-Policy Period | | | | | | | -.014 (.031) | |
| Camden x Policy Period | | | | | | | | .086 (.060) |
| Camden x Post-Policy Period | | | | | | | | .060 (.058) |
| Short and Long Run Policy Effects are Equal in Lambeth [p-value] | [.286] | [.035] | [.024] | [.011] | [.007] | [.008] | [.000] | [.009] |
| 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 | .785 | .848 | .896 | .739 | .849 | .833 | .732 | .738 |
| Observations | 4810 | 4810 | 1151 | 4689 | 2867 | 3850 | 4810 | 4810 |

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. 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. 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 (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). 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 Borough x Policy Period and Borough x Post-Policy Period interactions are equal.

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

Prais-Winsten Regression Estimates

| | Log recorded accused 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 | -.292*** (.061) | -.261*** (.070) | -.287*** (.076) | -.016 (.011) | .003 (.002) | -.360*** (.063) | -.302*** (.106) | -.327*** (.113) | -.017 (.016) | .000 (.002) |
| Policy Period | .007 (.013) | .008 (.013) | .006 (.012) | .001 (.004) | -.001* (.000) | .005 (.018) | -.024 (.017) | -.024 (.016) | -.002 (.005) | -.001 (.001) |
| Lambeth x Post-Policy Period | -.257*** (.051) | -.205*** (.063) | -.212*** (.071) | -.025*** (.008) | -.001 (.002) | -.319*** (.048) | -.232** (.094) | -.234** (.102) | -.023* (.013) | -.002 (.002) |
| Post-Policy Period | .019 (.020) | .022 (.021) | .012 (.019) | .008 (.007) | -.001 (.001) | .012 (.027) | -.038 (.028) | -.042 (.026) | -.001 (.008) | -.001 (.001) |
| Share of All Accused, or Clear ups, in Pre-policy Period | | - | .840 | .148 | .009 | - | - | .845 | .139 | .011 |
| Short and Long Run Policy Effects are Equal in Lambeth [p-value] | [.931] | [.697] | [.562] | [.235] | [.112] | [.774] | [.876] | [.717] | [.537] | [.340] |
| 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 | .868 | .846 | .845 | .519 | .499 | .870 | .827 | .832 | .502 | .535 |
| Observations | 2898 | 2233 | 2233 | 2233 | 2233 | 4495 | 1914 | 1914 | 1914 | 1914 |

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 accused 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. 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 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: 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] | .085** (.034) | .087** (.034) | .038 (.046) | .013 (.042) | -.031 (.046) | .201*** (.053) | .044 (.055) | .145*** (.041) | -.004 (.057) | .012 (.035) |
| Lambeth x Policy Period [7-13 months] | .011 (.031) | -.005 (.032) | .215*** (.043) | -.044 (.040) | -.064 (.042) | .119** (.050) | -.017 (.051) | .075* (.039) | -.063 (.054) | -.045 (.033) |
| Policy Period | .072** (.033) | .071** (.032) | .008 (.021) | .006 (.035) | .015* (.008) | .014 (.016) | .065** (.028) | .088*** (.032) | -.002 (.028) | .021 (.022) |
| Lambeth x Post-Policy Period | -.073*** (.020) | -.098*** (.020) | .249*** (.025) | -.061** (.025) | -.056** (.026) | -.074** (.031) | -.177*** (.033) | -.001 (.025) | -.147*** (.030) | -.049*** (.019) |
| Post-Policy Period | .105** (.046) | .106** (.045) | .009 (.030) | .049 (.049) | .028** (.013) | -.008 (.024) | .066* (.039) | .136*** (.045) | -.010 (.040) | .014 (.031) |
| Share of All Offences in Pre-policy Period | - | - | .023 | .160 | .009 | .040 | .119 | .401 | .088 | .160 |
| First and Second Half Policy Effects are Equal in Lambeth [p-value] | [.063] | [.022] | [.001] | [.239] | [.541] | [.179] | [.322] | [.142] | [.393] | [.182] |
| 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 | .740 | .962 | .638 | .874 | .946 | .987 | .880 | .973 |
| Observations | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 | 4810 |

Prais-Winsten Regression Estimates

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

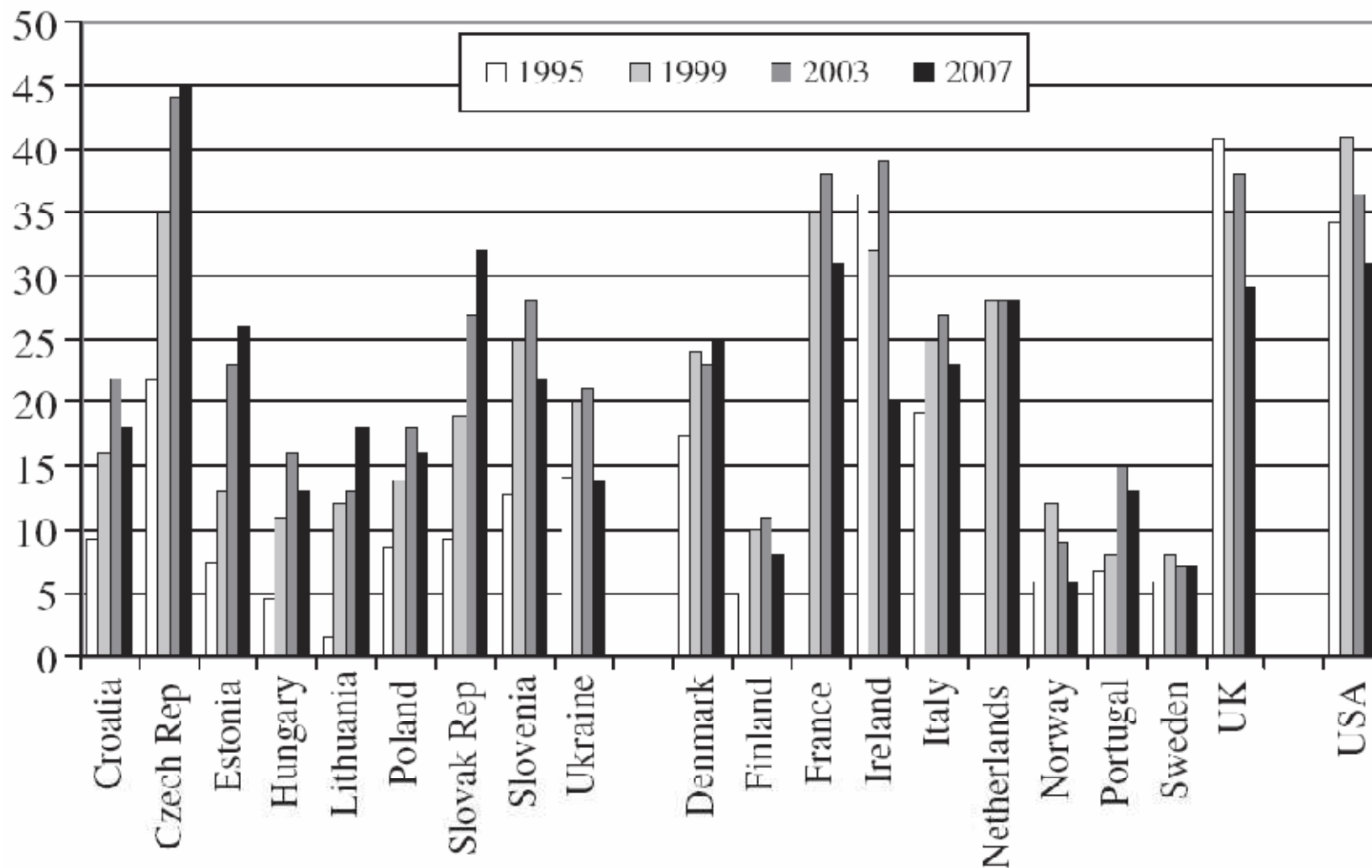
| | (1) Total (with drugs) | (2) Total (without drugs) | (3) Drugs Offences | (4) Violence Against the Person | (5) Sexual Offences | (6) Robbery | (7) Burglary | (8) Theft and Handling | (9) Fraud or Forgery | (10) Criminal Damage |
|---|---------------------------|------------------------------|-----------------------|------------------------------------|------------------------|-------------------|------------------|---------------------------|-------------------------|-------------------------|
| Lambeth x Policy Period [1-6 months] | -.019** (.008) | -.007 (.007) | -.274*** (.074) | -.006 (.014) | .051 (.047) | .016 (.016) | -.015 (.013) | -.005 (.005) | -.011 (.014) | -.010 (.009) |
| Lambeth x Policy Period [7-13 months] | -.016** (.008) | -.000 (.006) | -.322*** (.070) | .020 (.013) | -.013 (.048) | .042** (.017) | -.017 (.013) | -.006 (.005) | .007 (.014) | -.008 (.009) |
| Policy Period | -.002 (.006) | -.001 (.005) | -.010 (.021) | .008 (.010) | .027** (.014) | -.010 (.011) | .007 (.006) | -.004 (.004) | .005 (.006) | -.008** (.004) |
| Lambeth x Post-Policy Period | .010** (.005) | .013*** (.004) | -.151*** (.047) | .030*** (.008) | .085*** (.025) | .032*** (.010) | .019** (.008) | .002 (.003) | .021*** (.003) | .002 (.005) |
| Post-Policy Period | -.021** (.009) | -.018** (.008) | -.016 (.032) | -.006 (.016) | .021 (.022) | -.026 (.017) | -.006 (.010) | -.020*** (.006) | -.000 (.010) | -.025*** (.006) |
| Share of All Accused in Pre-policy Period | - | - | .169 | .220 | .015 | .047 | .077 | .332 | .048 | .092 |
| First and Second Half Policy Effects are Equal in Lambeth [p-value] | [.722] | [.414] | [.566] | [.117] | [.305] | [.228] | [.883] | [.919] | [.323] | [.897] |
| 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 | .925 | .922 | .824 | .863 | .261 | .450 | .626 | .901 | .591 | .687 |
| Observations | 2898 | 2898 | 2898 | 2898 | 2898 | 2898 | 2898 | 2898 | 2898 | 2898 |

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] | -.028 (.019) | -.016 (.019) | -.302*** (.081) | -.035 (.031) | -.064 (.082) | -.031 (.029) | -.032 (.036) | -.011 (.018) | -.029 (.035) | -.013 (.018) |
| Lambeth x Policy Period [7-13 months] | -.045** (.018) | -.033* (.018) | -.357*** (.076) | -.032 (.030) | -.103 (.078) | -.028 (.028) | -.055 (.035) | -.020 (.016) | -.020 (.035) | -.024 (.017) |
| Policy Period | .005 (.014) | .006 (.013) | .016 (.044) | .050 (.032) | .090*** (.033) | .011 (.014) | .015 (.014) | .000 (.014) | .029 (.020) | -.003 (.012) |
| Lambeth x Post-Policy Period | -.015 (.013) | -.018 (.013) | -.174*** (.043) | -.014 (.019) | -.046 (.042) | -.028* (.016) | -.007 (.020) | -.018 (.012) | -.034* (.019) | -.015 (.012) |
| Post-Policy Period | .000 (.020) | .003 (.019) | .050 (.068) | .100** (.046) | .169*** (.050) | .026 (.021) | .008 (.020) | -.005 (.020) | .071** (.031) | -.010 (.017) |
| Share of All Clear-ups in Pre-policy Period | - | - | .148 | .290 | .018 | .034 | .080 | .279 | .053 | .098 |
| First and Second Half Policy Effects are Equal in Lambeth [p-value] | [.462] | [.405] | [.597] | [.926] | [.724] | [.928] | [.613] | [.668] | [.849] | [.582] |
| 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 | .728 | .700 | .443 | .858 | .438 | .431 | .453 | .611 | .651 | .556 |
| Observations | 4495 | 4495 | 4495 | 4495 | 4494 | 4494 | 4492 | 4491 | 4494 | 4491 |

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 accused 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 accused 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. 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 [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: 16/07/2001-08/09/2001) 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 and Lambeth x Post-Policy Period interactions are equal.

Figure 1: Lifetime Cannabis Prevalence, 16-year-old Students



Notes: The source for Figure 1 is Pudney [2010]. It shows the lifetime cannabis prevalence among 16-year-old school students in Europe and the USA using data from the ESPAD school surveys and US Monitoring the Future survey.

Figure 2: Map of the London Boroughs

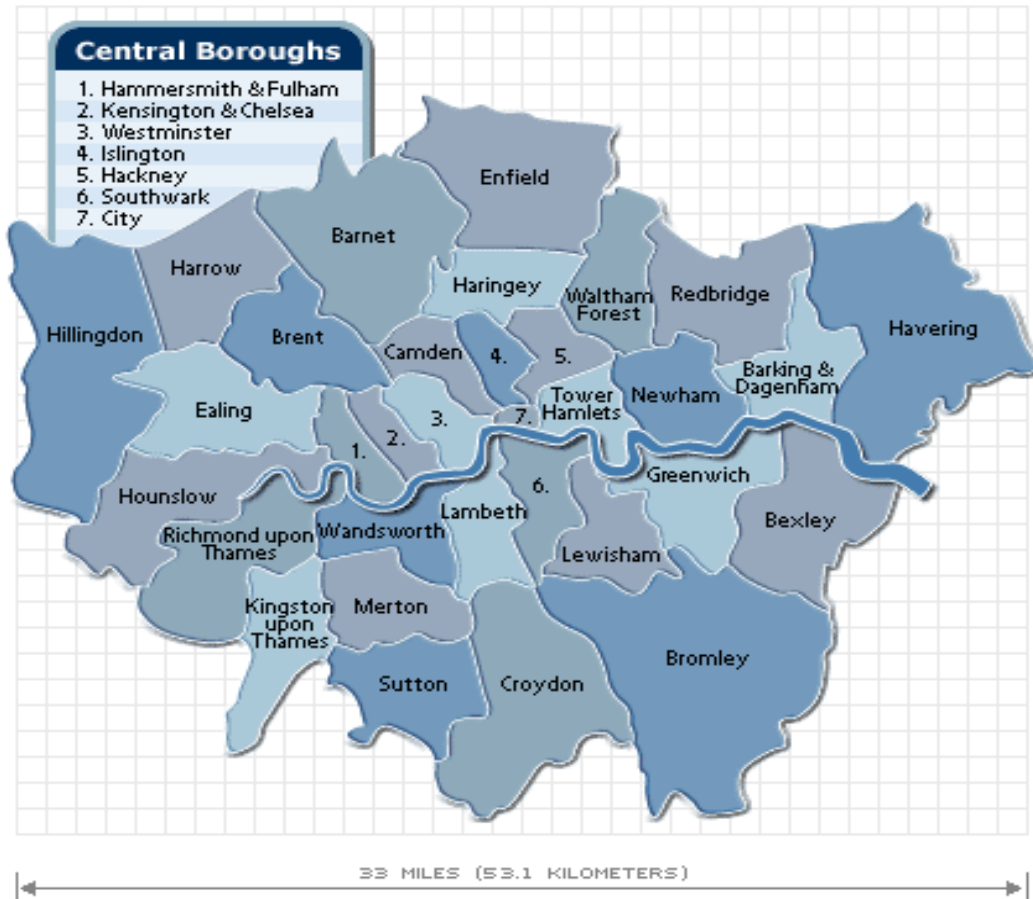
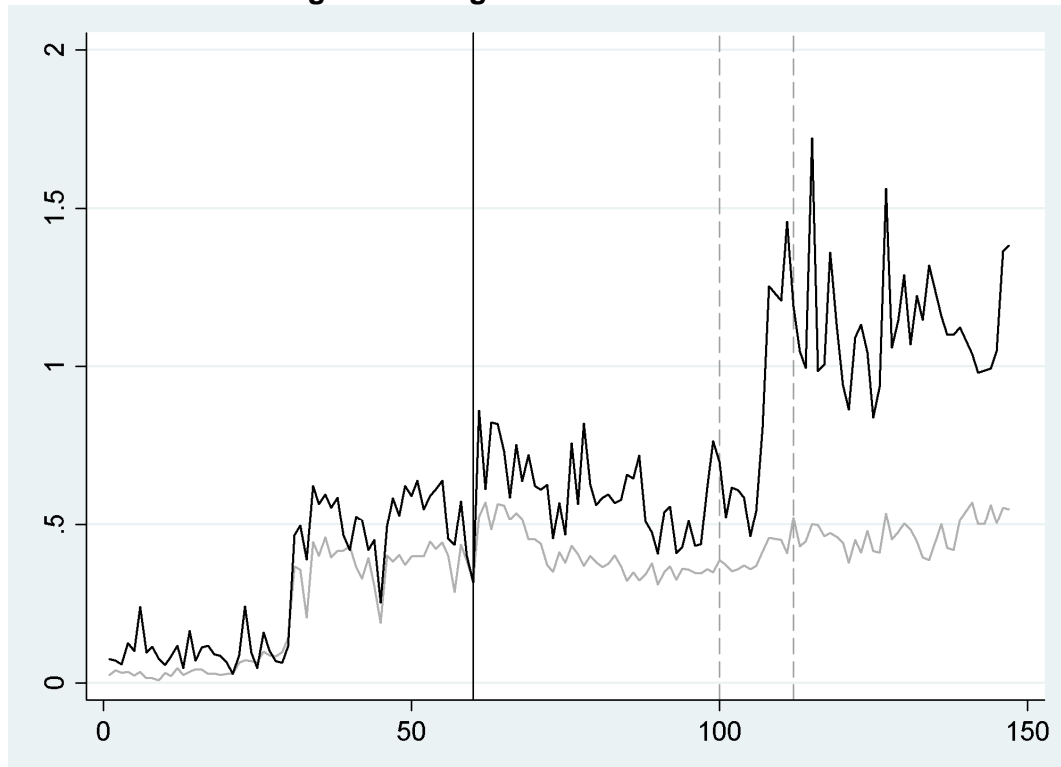
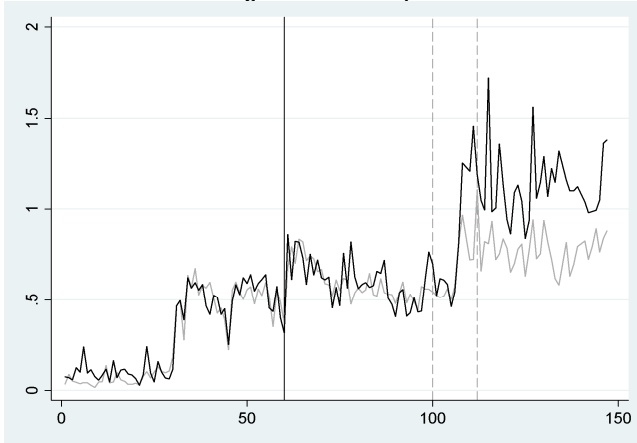


Figure 3: Drug Offences Time Series

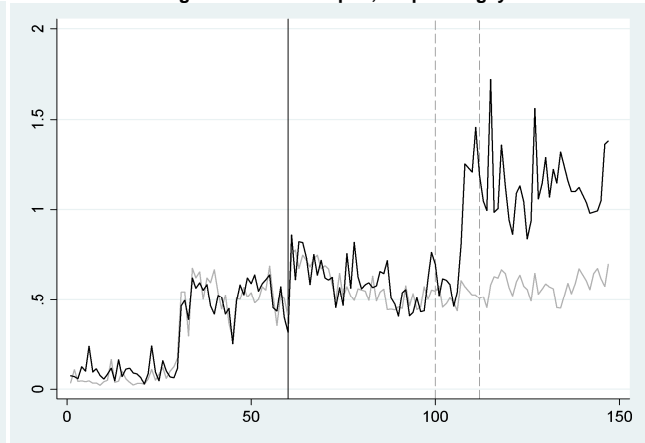


Notes: In Figure 3, the time period runs from April 1992 until June 2005, excluding the period April 1997 to March 1998. The time variable was recoded to skip these 12 months, thus April 1998 directly proceeds March 1997. It is for this reason that the x-axis labels the time variable as "artificial". The first vertical line on the graph is placed at the time at which the data series breaks (March 1997). The second and third 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 monthly drugs offences per capita for the rest of London, excluding Lambeth and also Sutton. The reason for the latter exclusion is that a balanced panel is required for the synthetic control analysis, and Sutton does not have recorded data for any crime types between April 1998 and March 1999.

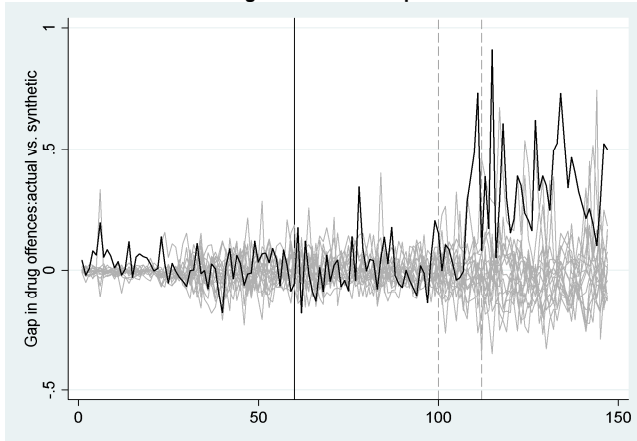
**Figure 4A: Lambeth and Synthetic Controls,
Drug Offences Per Capita**



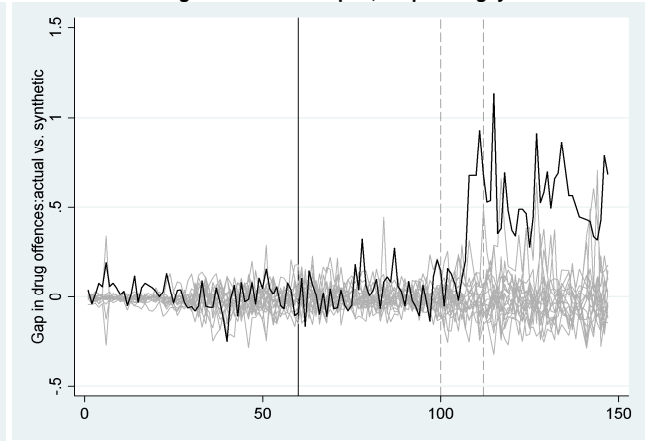
**Figure 4B: Lambeth and Synthetic Controls,
Drug Offences Per Capita, drop Haringey**



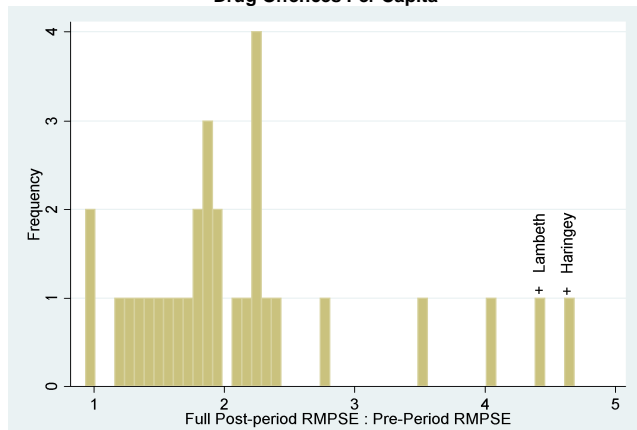
**Figure 5A: Lambeth and Synthetic Controls,
Drug Offences Per Capita**



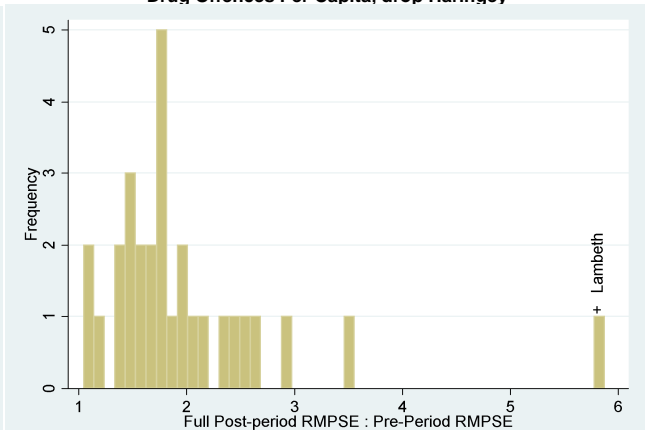
**Figure 5B: Lambeth and Synthetic Controls,
Drug Offences Per Capita, drop Haringey**



**Figure 6A: Lambeth and Synthetic Control RMSPE,
Drug Offences Per Capita**



**Figure 6B: Lambeth and Synthetic Control RMSPE,
Drug Offences Per Capita, drop Haringey**



Notes: The pre-policy period data runs from April 1992 until June 2001, excluding the period April 1997 to March 1998. The full period runs until June 2005. A balanced panel is required for this analysis hence the borough of Sutton was excluded as this borough does not have recorded data for April 1998-March 1999. A synthetic counterfactual - in the form of a weighted average of all other Metropolitan boroughs - was constructed for each borough (bar Sutton). The weights were calculated in order to best fit the pre-policy time series for drugs offences per 1000 borough capita over the full pre-policy period. For the first specification, the full pre-policy series of the following borough-month-year level variables are used to predict the pre-policy drugs offence series - 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, the male unemployment rate and the following offence types per capita: Violence Against The Person, Sexual Offences, Robbery, Burglary, Theft & Handling, Fraud & Forgery, Criminal Damage. In addition, and following Abadie et al. (2009), three pre-policy months' drugs offences were also used as predictors - 05/2000, 10/2000 and 03/2001. The right-hand side specification is identical to this previous specification, but excludes the borough of Haringey throughout. In graphs A and B, the first vertical line corresponds to the time at which the data series breaks (March 1997). The second and third represent the start and end of the Lambeth policy (July 2001 and July 2002 respectively).