Policing Cannabis and Drug Related Hospital Admissions: Evidence from Administrative Records*

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Abstract

We evaluate the impact of a policing experiment that depenalized the possession of small quantities of cannabis in the London borough of Lambeth, on hospital admissions related to illicit drug use. To do so, we exploit administrative records on individual hospital admissions classified by ICD-10 diagnosis codes. These records allow the construction of a quarterly panel data set for London boroughs running from 1997 to 2009 to estimate the short and long run impacts of the depenalization policy unilaterally introduced in Lambeth between 2001 and 2002. We find the depenalization of cannabis had significant longer term impacts on hospital admissions related to the use of hard drugs, raising hospital admission rates for men by between 40 and 100% of their pre-policy baseline levels. The impacts are concentrated among men in younger age cohorts. The dynamic impacts across cohorts vary in profile with some cohorts experiencing hospitalization rates remaining above pre-intervention levels three to four years after the depenalization policy is introduced. We combine these estimated impacts on hospitalization rates with estimates on how the policy impacted the severity of hospital admissions to provide a lower bound estimate of the public health cost of the depenalization policy.

Keywords: alcohol, cannabis, Class-A drugs, depenalization, hospital admissions.
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1 Introduction

Illicit drug use generates substantial economic costs including those related to crime, ill-health, and diminished labor productivity. In 2002, the Office for National Drug Control Policy estimated that illicit drugs cost the US economy $181 billion [ONDCS 2004]. For the UK, Gordon *et al.* [2006] estimated the cost of drug-related crime and health service use to be £15.4 billion in 2003/4. It is these social costs, coupled with the risks posed to drug users themselves, that have led governments throughout the world to try and regulate illicit drug markets. All such policies aim to curb both drug use and its negative consequences, but there is ongoing debate amongst policy-makers as to relative weight that should be given to policies related to prevention, enforcement, and treatment [Grossman *et al.* 2002].

The current trend in policy circles is to suggest regimes built *solely* around strong enforcement and punitive punishment might be both costly and ineffective. For example, after forty-years of the US 'war on drugs', the Obama administration has adopted a strategy that focuses more on prevention and treatment, and less on incarceration [ONDCS 2011], although the two primary enforcement and policy agencies of the Drug Enforcement Agency and the Office for National Drug Control Policy remain more focused on traditional supply-side approaches. Other countries such as the Netherlands, Australia and Portugal, have long adopted more liberal approaches that have depenalized or decriminalized the possession of some illicit drugs, most commonly cannabis, with many countries in Latin America currently debating similar moves.[1] While such policies might well help free up resources from the criminal justice system and stop large numbers of individuals being criminalized [Adda *et al.* 2013], these more liberalized policies also carry their own risks. If such policies signal the health and legal risks from consumption have been reduced, then this should reduce prices [Becker and Murphy 1988]. This can potentially increase the number of users as well as increasing use among existing users, all of which could have deleterious consequences for user's health. The use of certain drugs might also provide a causal 'gateway' to more harmful and addictive substances [van Ours 2003, Melberg *et al.* 2010].

This paper considers the impact of a localized policing experiment that reduced the enforcement of punishments against the use of one illicit drug - cannabis - on a major cost associated with the consumption of illegal drugs: the use of health services by consumers of illicit drugs. The policing experiment we study took place unilaterally in the London Borough of Lambeth and ran from July 2001 to July 2002, during which time all other London boroughs had no change in policing policy towards cannabis or any other illicit drug. The experiment - known as the Lambeth Cannabis Warning Scheme (LCWS) - meant that the possession of small quantities of cannabis was temporarily depenalized, so that this was no longer a prosecutable offence.[2] We evaluate the short and long run consequences of this policy

[1] A recent policy announcement by the US Attorney General Eric Holder in August 2013, signaled that a "fundamentally new approach" would be tried in which federal prosecutors will no longer seek mandatory sentences for some non-violent drug offenders. Uruguay now appears set to be the first country to legalize the sale and production of cannabis.

[2] Donohue *et al.* [2011] categorize illicit drug policies into three type: (1) legalization - a system in which possession and sale are lawful but subject to regulation and taxation; (ii) criminalization - a system of proscriptions on possession and sale backed by criminal punishment, potentially including incarceration; (iii) depenalization - a hybrid system, in which sale

on healthcare usage as measured by detailed and comprehensive administrative records on drug-related admissions to all London hospitals. Such hospital admissions represent 60% of drug-related healthcare costs [Gordon *et al.* 2006]. To do so we use a difference-in-difference research design that compares pre and post-policy changes in hospitalization rates between Lambeth and other London boroughs. Our analysis aims to shed light on the broad question of whether policing strategies towards the market for cannabis impact upon public health, through changes in the use of illicit drugs and subsequent health of drug users.

Our primary data comes from a novel source that has not been much used by economists: the Inpatient Hospital Episode Statistics (HES). These administrative records document every admission to a public hospital in England, with detailed ICD-10 codes for classifying the primary and secondary cause of each individual hospital admission.[3] This is the most comprehensive health related data available for England, in which it is possible to track the admissions history of the same individual over time. We aggregate the individual HES records to construct a panel data set of hospital admissions rates by London borough and quarter. We do so for various cohorts defined along the lines of gender, age at the time of the implementation of the depenalization policy, and previous hospital admissions history. As such these administrative records allow us to provide detailed evidence on the aggregate impact of the depenalization policy on hospitalization rates, and to provide novel evidence on how these health impacts vary across cohorts. To reiterate, these administrative records cover the most serious health events. Patients with less serious conditions receive treatment elsewhere, including outpatient appointments, accident and emergency departments, or primary care services. If such health events are also impacted by drugs policing strategies, our estimates based solely on inpatient records provide a strict *lower* bound impact of the depenalization of cannabis on public health.

The balanced panel data we construct covers all 32 London boroughs between April 1997 and December 2009. This data series starts four years before the initiation of the depenalization policy in the borough of Lambeth, allowing us to estimate policy impacts accounting for underlying trends in hospital admissions. The series runs to seven years after the policy ended, allowing us to assess the long term impacts of a short-lived formal change in policing strategy related to cannabis.

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and possession are proscribed, but the prohibition on possession is backed only by such sanctions as fines or mandatory substance abuse treatment, not incarceration. The LCWS policing experiment we evaluate is a policy of depenalization. The practical way in which it was implemented is very much in line with policy changes in other countries that have changed enforcement strategies in illicit drug markets and as such we expect our results to have external validity to those settings, including for the current debate on the potential decriminalization of cannabis in California [Kilmer *et al.* 2010]. As discussed in Chu [2012], medical marijuana legislation represents a major change in US policy in recent years, where 17 states have now passed laws that allow individuals with specific symptoms to use marijuana for medical purposes.

[3]Private healthcare constitutes less than 10% of the healthcare market in England, with most admissions for elective procedures. Focusing on admissions to public hospitals is therefore unlikely to produce a biased evaluation of the policing policy on drug-related hospitalizations. The HES contains an inpatient and an outpatient data set. We only use the inpatient data. The inpatient data includes all those admitted to hospital (under the order of a doctor) who are expected to stay at least one night, and contains ICD-10 diagnosis classifications. The outpatient data covers those in which a patient is seen but does not require a hospital bed for recovery purposes (except for a short recovery after a specific procedure). We do not use the HES outpatients data because it is only reliable from 2006/7 onwards (and so not before the LCWS is initiated) and does not have information on diagnosis codes.

Given the detailed ICD-10 codes available for each admission, the administrative records allow us to specifically measure admission rates for drug-related hospitalizations for *each* type of illicit drug: although the depenalization policy would most likely impact cannabis consumption more directly than other illicit drugs, this has to be weighed against the fact that hospitalizations related to cannabis usage are extremely rare and so policy impacts are statistically difficult to measure along this margin. Our main outcome variable therefore focuses on hospital admissions related to hard drugs, known as 'Class-A' drugs in England. This includes all hospital admissions where the principal diagnosis relates to cocaine, crack, crystal-meth, heroin, LSD, MDMA or methadone.[4] The administrative records also contain information on the length of hospital stays (in days) associated with each patient admission, and we use this to explore whether the depenalization policy impacted the *severity* of hospital admissions (not just their incidence), where the primary diagnosis relates to hospitalizations for Class-A drug use. Ultimately, we then combine the estimated policy impacts on hospitalization rates and the severity of hospital admissions for Class-A drug use, to provide a conservative estimate of the public health costs of the depenalization policy that arises solely through the increased demand on hospital bed services.

We present four main results. First, relative to other London boroughs, the depenalization policy had significant long term impacts on hospital admissions in Lambeth related to the use of Class-A drugs, with the impacts being concentrated among men. Exploring the heterogeneous impacts across male cohorts, we find the direct impacts on Lambeth residents to be larger among cohorts that were younger at the start of the policy. The magnitudes of the impacts are large: the increases in hospitalization rates correspond to rises of between 40 and 100% of their pre-policy baseline levels in Lambeth, for those aged 15-24 and aged 25-34 on the eve of the policy. To underpin the credibility of the difference-in-difference research design, we also probe the data to: (i) check for pre-existing divergent trends in hospitalization rates between Lambeth and other London boroughs; (ii) evaluate the robustness of the results to alternative control boroughs to compare Lambeth to; (iii) examine whether differential changes over time in health care provision between Lambeth and other locations, or other policies impacting hospitalizations for Class-A drug use, could confound the results, and; (iv) shed light on whether individuals changed borough of residence in response to the policy.

Second, the dynamic impacts across cohorts vary in profile with some cohorts experiencing hospitalization rates remaining above pre-intervention levels three to four years after the depenalization of cannabis was first introduced.

Third, we explore the impacts of the policy on hospitalizations related to alcohol use among Lambeth residents. There is a body of work examining the relationship between cannabis and alcohol use: this has generated mixed results with some research finding evidence of the two being complements [Pacula 1988,

[4]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. Class-A drugs include cocaine, crack, crystal-meth, heroin, LSD, MDMA and methadone. Much of the ongoing policy debate on the decriminalization or depenalization of cannabis, reclassifying it from Class-B to Class-C, stems from the fact that legal drugs such as alcohol and tobacco, are thought to have higher levels of dependency and cause more physical harm to users than some illicit drugs including cannabis [Nutt *et al.* 2007].

Williams *et al.* 2004], and other studies suggesting the two are substitutes [DiNardo and Lemieux 2001, Crost and Guerrero 2012]. We add to this debate using a novel policy experiment and administrative data. Our results suggests that for the youngest age cohort, if depenalization causes the price of cannabis to fall, then alcohol and cannabis might well be substitutes. However for older age cohorts, we find no evidence that the policy leads to increased admissions related to alcohol use, or the combined use of alcohol and Class-A drugs.

Finally, the severity of hospital admissions, as measured by the length of stay in hospital, significantly increases for admissions related to Class-A drug use. We then combine this impact with our baseline estimated impacts on hospitalization rates by age cohort, to calculate the annual cost of the policy. We find the increased hospitalization rates and length of stays conditional on admission to be around £80,000 per annum, and this more than offsets the downward time trend in hospital bed-day costs that exists in the rest of London in the post-policy period.

Taken together, our four classes of results suggest policing strategies towards the market for cannabis have significant, nuanced and long lasting impacts on public health.

Our analysis contributes to understanding the relationship between drug policies and public health, an area that has received relatively little attention despite the sizable social costs involved. This partly relates to well known difficulties in evaluating policies in illicit drug markets: multiple policies are often simultaneously targeted towards high supply locations; even when unilateral policy experiments or changes occur they often fail to cause abrupt or quantitatively large demand or supply shocks, and data is rarely detailed enough to pin down interventions in specific drug markets on other drug-related outcomes [DiNardo 1993, Caulkins 2000, Chu 2012]. Our analysis, that combines a focused policy and administrative records, makes some progress on these fronts.

To place our analysis into a wider context, it is useful to compare our findings with two earlier prominent studies linking illicit drug enforcement policies and health outcomes: Model [1993] uses data from the mid-1970s to estimate the impact on hospital emergency room admissions of cannabis decriminalization, across 12 US states. She finds that policy changes led to an increase in cannabis-related admissions and a decrease in the number of mentions of other drug related emergency room admissions, suggesting a net substitution towards cannabis. Our administrative records also allow us to also check for such broad patterns of substitution or complementarity between illicit drugs. Our results suggest that the depenalization of cannabis led to longer term increases in the use of Class-A drugs, as measured by hospital inpatient admissions rather than emergency room admissions as in Model [1993].[5]

More recent evidence comes from Dobkin and Nicosia [2009], who assess the impact of an intervention that disrupted the supply of methamphetamine in the US by targeting precursors to methamphetamine.

[5] An important distinction between our data and that used in Model [1993] is that the HES data has a patient-episode as its unit of observation, rather than 'drug mentions' of which Model [1993] report up to six per patient-episode. Moreover, the data used in Model [1993] are not administrative records, but were collected by the Drug Abuse Warning Network from emergency rooms in 24 major SMSAs. As Model [1993] discusses, some data inconsistencies arise because the emergency rooms in the sample change over time.

They document how this led to a sharp price increase and decline in quality for methamphetamine. Hospital admissions mentioning methamphetamine fell by 50% during the intervention, whilst admissions into drug treatment fell by 35%. Dobkin and Nicosia [2009] find no evidence that users substituted away from methamphetamine towards other drugs. Finally, Dobkin and Nicosia [2009] find the policy of disrupting methamphetamine supply was effective only for a relatively short period: the price of methamphetamine returned to its pre-intervention level within four months and within 18 months hospital admissions rates had returned to their baseline levels. In contrast, the cannabis depenalization policy we document has an impact on hospitalization rates that, for many cohorts, lasts for up to four years after the policy was initiated and despite the fact that the policy itself was only formally in place for one year.[6]

The paper is organized as follows. Section 2 describes the LCWS and the existing evidence on its impact on crime. Section 3 details our administrative data, discusses the plausibility of a link between policing-induced changes in the cannabis market and the consumption of Class-A drugs, and describes our empirical method. Section 4 presents our baseline results which estimate the impact of the LCWS by cohort and the associated robustness checks to underpin the credibility of the research design. Section 5 presents extended results related to dynamic effects, spillovers in alcohol-related admissions, and the severity of admissions. Section 6 estimates the public health costs of the policy. Section 7 discusses the broader policy implications of our findings, and the potential for opening up a research agenda on the relationship between police behavior and public health.

2 The Lambeth Cannabis Warning Scheme (LCWS)

The Lambeth Cannabis Warning Scheme (LCWS) was unilaterally introduced into the London borough of Lambeth on 4th July 2001 by the borough's police force. The scheme was initially launched as a pilot intended to last six months, and represented a change in policing policy towards the market for cannabis. Under the scheme, those found in possession of small quantities of cannabis for their personal use in Lambeth: (i) had their drugs confiscated; (ii) were given a warning rather than being arrested.[7] The main reason behind the policy change was to reduce the number of individuals being criminalized for consuming cannabis, and to free up police time and resources to deal with more serious crime, including those related to hard drugs or 'Class-A drugs' [Dark and Fuller 2002, Adda *et al.* 2013]. The underlying motivations for the policy, as well as the way in which it was implemented and the targeted

[6] As with the economics literature the bulk of the criminology literature has also focused on the *crime* impacts of drug enforcement policies. One exception is Hughes and Stevens [2010] who study the wider impacts of the decriminalization of cannabis introduced in Portugal in 2001. However the evidence they present is based either on Europe wide survey data and compares trends in Portugal to those in Spain and Italy, or stakeholder interviews in Portugal. They do not present regression estimates to measure causal impacts. MacCoun and Reuter [2001] discuss the health impacts of cannabis depenalization after reviewing evidence from a range of countries.

[7] The LCWS policy applied equally to juveniles and adults (the age of criminal responsibility in England and Wales is 10 years old).

outcomes, were very similar to the way in cannabis depenalization policies have often been implemented throughout the world. In keeping with other experiences of depenalization, the primary motivation behind the policy was to free up police time and resources to tackle other crimes, and there was little or no discussion of the depenalization policy's potential impact on public health. To this extent our results can be informative of the existence of links between police drugs policy and public health in settings outside of the specific London context we study.[8]

Anecdotal evidence suggests local support for the scheme began to decline once the policy was announced to have been extended beyond the initial six-month pilot. Media reports cited that local opposition arose due to concerns that children were at risk from the scheme, and that the depenalization policy had increased drug tourism into Lambeth. The LCWS formally ended on 31st July 2002. Post-policy, Lambeth's cannabis policing strategy did *not* return identically to what it had been pre-policy, partly because of disagreements between the police and local politicians over the policy's true impact. Rather, it adjusted to be a firmer version of what had occurred during the pilot so that police officers in Lambeth continued to issue warnings but would now also have the discretion to arrest where the offence was aggravated.[9] Hence our measured long run impacts of the depenalization policy capture the total effects arising from: (i) the long run impact of the introduction of the depenalization policy between June 2001 and July 2002; (ii) any permanent differences in policing towards cannabis between the pre-policy and post-policy periods.

The impact of the LCWS depenalization policy on patterns of *crime* in Lambeth and other boroughs is extensively studied in Adda *et al.* [2013]. For the purposes of the current study on the relationship between drug-policing and public health, three key results on the impact of the localized depenalization policy on crime need to be borne in mind: its impact on the market for cannabis in Lambeth, its impact on the market for Class-A drugs, and drug tourism induced into Lambeth from other parts of London due to interlinkages in illicit drug markets across boroughs.

First, Adda *et al.* [2013] find the LCWS led to a significant and permanent rise in cannabis related criminal offenses in Lambeth. Using finely disaggregated data by type of drug offence, they find that both the demand and supply of cannabis are likely to have risen significantly in Lambeth after the introduction of the depenalization policy, and that this impact persists into the long run, well after the LCWS policy officially ended. This result is important for the current study because it suggests the depenalization policy caused an abrupt, quantitatively large and permanent shock to the cannabis market, leading to the equilibrium market size to have likely increased by around 60% in the longer

[8]For example, there have been moves over the past decade in California towards more liberal policies related to cannabis. In 2010 California passed into law a depenalization policy that reduced the penalty associated with being found in possession of less than one ounce of cannabis, from a misdemeanor to a civil infraction. Further moves to a more liberal regulation of the cannabis market - almost to the point of legalization - remain on the policy agenda in California [Kilmer *et al.* 2010]. The moves to introduce legislation allowing for medical marijuana have also been pronounced, with 17 US states currently having such laws in place [Chu 2012].

[9]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 child play areas.

term, as proxied by the number of criminal offences for cannabis possession.[10]

Second, this expansion will consequently affect the equilibrium market size for Class-A drugs if the markets are related in some way, either because of economies of scale in supplying both drug markets, or because on the demand side preferences are such that cannabis and Class-A drugs are complements/substitutes. Along these lines, Adda *et al.* [2013] report that the longer term effect of the LCWS was to lead to a significant increase in offenses related to the possession of Class-A drugs: offence rates for the possession of such substances rose by 12% in Lambeth in the post-policy period relative to the rest of London. However, there is little evidence that the police reallocated their efforts towards crimes relating to Class-A drugs: Adda *et al.* [2013] report no change in police effectiveness against Class-A drug crime in Lambeth based on two out of four such measures (arrest and clear-up rates). Rather, Adda *et al.* [2013] document that the policy appears to have allowed the police to reallocate effort towards non-drug crime. The fact that the LCWS policy did not lead to a major reallocation of police resources towards crime related to Class-A drugs suggests that in the current study, any link between the depenalization policy and hospitalizations for Class-A diagnoses most likely stems from the interlinkages between the demand sides of the markets for cannabis and Class-A drugs. Given the addictive nature of Class-A drugs, potential lags between cannabis use and the use of Class-A drugs later in life, and potential lags in seeking out and receiving treatment [Fergusson and Horwood 2000, Patton *et al.* 2002, Arsenault *et al.* 2004], we might also reasonably expect any impact of the LCWS on hospital admissions related to Class-A drug use to last well into the post-policy period. We therefore later consider how the effects of the LCWS on drug-related hospital admissions evolve over time.

Third, Adda *et al.* [2013] document how the LCWS likely induced drug tourism into Lambeth. Such changes in the location where individuals decided to purchase cannabis stems from the fact that local markets for illicit drugs are inherently interlinked across London boroughs. To explore this further in terms of health outcomes, we later investigate whether there is any evidence of individuals permanently changing their actual borough of residence to Lambeth, after the LCWS is introduced.

Standard consumer theory provides clear set of predictions on how such depenalization policies can impact the use of cannabis and other illicit drugs. Most existing studies assume that such policies cause significant reductions in the price of cannabis [Thies and Register 1993, Grossman and Chaloupka 1998, Williams *et al.* 2004]. This will, all else equal, increase the demand for cannabis in part because of greater demands from *existing* users and also because of an impact on the extensive margin so that new individuals choose to *start* consuming cannabis at the lower price. This will have a positive impact on the consumption of Class-A drugs if cannabis and Class-A drugs are contemporaneous complements in user preferences. It will also increase the demand for Class-A drugs over time if the use of cannabis serves either as a gateway to the use of other harder illicit drugs, or their is state dependence so that cannabis users have particular characteristics that also lead them to subsequently misuse Class-A drugs. Of course

[10]Relative to citywide trends, cannabis possession offenses in Lambeth increased by 29% during the policy, and 61% in the post policy period (August 2002 to January 2006) relative to the pre-policy period Adda *et al.* [2013].

if cannabis and Class-A drugs are substitutes, then the increased demand for cannabis resulting from the depenalization of cannabis possession should reduce Class-A drug use and related hospitalizations. Such cross price impacts might also exist between cannabis and alcohol [Pacula 1988, DiNardo and Lemieux 2001]. Hence we later also examine how hospitalization rates for diagnoses primarily related to alcohol use respond to the depenalization policy.

3 Data, Descriptives and Empirical Method

3.1 Administrative Records on Hospital Admissions

Data on hospital admissions are drawn from the Inpatient Hospital Episode Statistics (HES). These provide an administrative record of every inpatient health episode, defined as a single period of care under one consultant in an English National Health Service hospital.[11] These administrative records are the most comprehensive data source on health service usage for England. Inpatients include all those admitted to hospital with the intention of an overnight stay, plus day case procedures when the patient is formally admitted to a hospital bed. As such, these records cover the most serious health events. Patients with less serious conditions receive treatment elsewhere, including outpatient appointments, accident and emergency departments, or primary care services. If such health events are also impacted by the depenalization policing strategy, our estimates based solely on inpatient records provide a strict *lower* bound impact of the policy on public health. For each patient-episode event in the administrative records, the data record the date of admission, total duration in hospital, and ICD-10 diagnoses codes in order of importance. Background patient information covers their age, gender, and their zip code of residence at the time of admission.[12]

We assess how hospital admissions related to Class-A drug use and to cannabis use are impacted by the depenalization of cannabis possession in Lambeth. For Class-A drug related admissions, we include episodes where the drug is mentioned as the primary diagnosis, namely those episodes directly caused by the use of Class-A drugs. As hospital admissions for cannabis are far rarer, we include episodes where the drug is mentioned as either a primary or a secondary diagnosis.[13] As our main outcome relates to

[11] We include all episodes of each hospital stay, so that if a patient is under the care of different consultants during their stay in hospital and before discharge, these count as multiple patient-episodes. Given the infrequency with which the same patient transfers across consultants during a hospital stay, the main results presented are robust to re-defining episodes at the patient-consultant level.

[12] Between 10 and 12% of the population in England have private health insurance, largely provided by employers. However, this is typically a top-up to NHS care, and does not cover serious illness or most emergencies. Private hospitals do not have emergency rooms, and the use of private primary health care is very rare. The data will therefore capture a very high proportion of adverse drug reactions that require treatment in hospital. The ICD is the international standard diagnostic classification for epidemiological and clinical use. Data on admissions to hospital accident and emergency wards is not available for England for the study period, and administrative records on outpatients do not contain the detailed ICD-10 diagnosis codes. Hence some of the health impacts of depenalization policies on more acute conditions that might not require overnight hospitalization, such as drug poisonings or allergic reactions, will not be measured in our administrative records.

[13] Diagnoses that mention Class-A drugs include (drug specific) mental and behavioral disorders (ICD-10 Codes F11 for

rates of hospital inpatient admissions, we aggregate the individual patient-episode level data by borough of residence and quarter, and calculate admission rates per thousand population for diagnosis d, borough of residence b in quarter q of year y as follows,

$$Admit_{dbqy} = \frac{Tot_{dbqy}}{Pop_{by}}, \tag{1}$$

where Tot_{dbqy} are total number of hospital admissions for diagnosis d, amongst those residing in borough b, in quarter q of year y, and Pop_{by} is the population of borough b in year y (measured in thousands). These admission rates are calculated by gender and age cohort, where age is categorized into ten year bins (15-24, 25-34, 35-44) and patient's age is defined as that on the eve of the LCWS policy. For each age-gender cohort, we create a panel of hospital admission rates for all London boroughs, excluding those that neighbor Lambeth (Croydon, Merton, Southwark and Wandsworth). Neighboring boroughs are excluded from our baseline specifications because of the potential for there to be public health spillovers in those boroughs. Our sample therefore covers hospital admissions among residents of 28 London boroughs (including Lambeth), by quarter, from April 1997 to December 2009.[14]

As discussed in more detail later, some specialist services required to treat diagnoses involving the use of Class-A drugs, such as those relating to mental health, are concentrated in a small subset of facilities that are dispersed across London. Each of these specialist facilities would be expected to treat patients from *across* London. Hence the geographic information we use to understand the impact of the localized LCWS policy relates to the patient's borough of *residence*, not the borough in which they are hospitalized. This helps ameliorate concerns that any changes in drug related hospitalization rates are driven by changes in the provision of specific drug-related services through specialized hospitals in London (that serve individuals resident in multiple boroughs). In Section 4.2 we provide evidence ruling out potentially confounding changes on the supply side of medical care for heavy users of Class-A drugs. Hence, any documented change in hospital admissions for Class-A drug related diagnoses in Lambeth following the introduction of the LCWS might then operate through two mechanisms: (i) a change in behavior of those resident in Lambeth prior to the policy; (ii) a change in the composition of Lambeth residents, with the policy potentially inducing a net inflow of people into the borough with a higher

opiods, F14 for cocaine, F16 for hallucinogens), intentional and accidental poisoning (T400-T406 T408-T409), and the finding of the drug in the blood (R781-R785). Diagnoses that mention cannabis include mental and behavioral disorders (F12), and poisoning (T407).

[14]Theory gives no guidance as to which age groups should be used. We focus on groups covering the main ages that would likely be impacted by a policy related to the depenalization of cannabis: those aged 15 to 44. We have then split this population into three equal age cohorts to ensure there are high enough admissions rates in each group, and that the age groups overlap with the age bins for population estimates at the borough level. More precisely, Annual Office for National Statistics population estimates at the borough level are only provided in five-year bands. As such, the estimates will only record the size of a particular 10-year age cohort once every five years. For example, in 2001, the 25-34 cohort was equal to the population aged 20-24 plus the population 25-29. To deal with this populations are interpolated in all other years, but taking a weighted sum of the relevant cohorts. In 2002, the same cohort were 21-30, and therefore split between three five-year age bins. We therefore interpolate as follows: (0.8$\times$ total aged 20-24) + total aged 25-29 + (0.2 $\times$ total aged 30-34). Results are robust to fixing the population at 2001 levels.

propensity for Class-A drug use. In Section 4.2 we use our data to examine the relative importance of these channels: we find little evidence of systematic changes of residence in response to the policy, implying most of the impacts are driven by changes in behavior among those already residing in Lambeth pre-policy.

The administrative records also allow us to create panels based on prior histories of patient admissions because the HES records have unique patient identifiers that allow the same patient to be tracked over episodes between 1997 and 2009. We focus on histories of admissions related to the use of either drugs (Class-A drugs, cannabis, or other illicit drug), or alcohol, and create panels by borough-quarter-age cohort-gender, for those with and without pre-policy histories of admissions related to drugs or alcohol. Among those with no pre-policy admissions, we calculate admission rates as per (1), where by construction this admission rate is zero before the policy. For this group, we effectively estimate whether the depenalization policy differentially impacted hospital admission rates between Lambeth and other non-neighboring boroughs in the period *after* the policy is first initiated. For those with pre-policy admission rates (an obviously far smaller group of individuals than those without admissions histories), we change the numerator in the admission rate to reflect the relevant 'at risk' population: hence Pop_{by} is replaced by the number of distinct individuals admitted for diagnoses related to illicit drugs or alcohol whilst residing in borough b in the pre-policy period between April 1997 and June 2001 (which given the small number of individuals with histories of such hospitalizations, is not measured in thousands).

The depenalization policy likely lowers prices for cannabis in Lambeth, all else equal. Depenalization might then impact hospitalizations for Class-A diagnoses differently across cohorts based on their prior histories of illicit drug use. Among those with no prior history of hospitalization for drug or alcohol use, the reduced price of cannabis induced by the policy might lead to greater consumption of Class-A drugs if they are complements to cannabis, or, for example, cannabis acts a gateway to such substances. To be clear, among this cohort we pick up the combined impacts among those that *were previously* using illicit drugs (and potentially other substances) but not so heavily so as to induce hospitalizations, as well as those that *begin* to use cannabis and Class-A drugs for the first time as a result of the reduced price of cannabis. The administrative data utilized does not allow us to separate out the policy impacts stemming from each type of individual. Among the cohorts with histories of hospitalization for drug or alcohol use even before the LCWS is initiated, there are likely to be long term and heavy users of illicit substances. Such individuals' consumption of Class-A drugs might reasonably be more habitual and so less sensitive to changes in price of cannabis, so that this cohort might be less impacted by the depenalization policy, all else equal.

3.2 Cannabis and Class-A Drug Use

Our primary interest is to understand how changes in police enforcement strategies towards the cannabis market - as embodied in the LCWS policy - impact public health through changes in hospitalization rates related to illicit drug use. Of course the policy would most directly affect the consumption of

cannabis, but changes in inpatient hospital admissions related to cannabis use are statistically hard to detect given the rarity of such events, as documented in detail below. It is therefore instructive to first compare rates of drug related hospital admissions from the HES administrative records, to rates of self-reported drug *use* from household surveys the most reliable of which is the British Crime Survey (BCS). Estimates from the BCS in 2002/3 indicate that cannabis was by far the most popular illicit drug, with 16% of 16-24 year-olds and 9% of 25-34 year-olds reporting to have used cannabis in the month prior to the survey. The corresponding figures for Class-A drug use are just 4% and 2% respectively [Condon and Smith 2003]. The HES records show that there are seven times as many inpatient hospital admissions for Class-A drugs than for cannabis. This reinforces the notion that cannabis related policing policies such as the LCWS, may not lead to a rise in statistically detectable cannabis related hospital admissions even if there is a substantial increase in cannabis usage caused by the policy.

What is important for our analysis is that a body of evidence suggests the cannabis and Class-A drug markets are linked: while little is known about such potential linkages on the supply side, on the demand side this might be because cannabis users are more likely to consume Class-A drugs, both contemporaneously and in the future. There are of course multiple explanations for this positive correlation between admissions for cannabis and subsequent usage of Class-A drugs. One explanation is state dependence so that cannabis users have particular characteristics that also lead them to subsequently misuse Class-A drugs, a channel shown to be of first order importance using data from the NLSY97 by Deza [2011]. Alternatively, the use of cannabis might act as a causal gateway to the use of harder drugs, has been suggested by Beenstock and Rahav [2002], van Ours [2003], Bretteville-Jensen *et al.* [2008] and Melberg *et al.* [2010].

Clearly the empirical debate on the relative importance of state dependence and gateway impacts is far from settled. For our study what is important is that *some* correlation between the market sizes for cannabis and other illicit drugs exists, be it either because of state dependence or gateway effects. To show the relatedness between these markets as recorded in the hospital admissions records we exploit, we present descriptive evidence from the HES to suggest how cannabis consumption today might correlate to Class-A drug use in the future. To do so we exploit the individual identifiers in the administrative records, allowing us to track the same person over time. We then calculate the probability, conditional on an admission in 1997 or 1998, of being readmitted to hospital at least once between 2000 and 2004. Four groups of admission are considered: (i) "cannabis admissions", who were admitted for cannabis; (ii) "Class-A admissions", who were admitted for the use of a harder drug; (iii) "alcohol admissions", who were admitted for alcohol related diagnoses; (iv) "all other admissions", who were admitted for any other cause and serve as a benchmark for the persistence of ill-health over these time periods. Table 1 shows the mean and standard deviation for each probability of readmission, conditional on prior admissions.[15]

[15]Given the infrequency of cannabis related admissions, in Table 1 we expand the geographic coverage of the sample to cover metropolitan local authorities in Greater Manchester, Merseyside, the West Midlands, Tyne and Wear, and South Yorkshire, in addition to London that our main analysis is based on. This covers accounts for approximately 30% of England's population. We exclude Lambeth from this analysis to prevent any impact of the LCWS contaminating these

Two points are of note. First, there is substantial persistence in hospital admissions for the same risky behavior, as shown on the leading diagonal in Columns 1-3. Persistence is particularly high for Class-A drugs and alcohol, where 26 and 23% of individuals respectively, were readmitted for the ill-effects of the same risky behavior over the two time periods. Reading across the last row of Table 1 on subsequent readmission to hospital from 2000 to 2004 for any diagnosis unrelated to drugs or alcohol, we see that this readmission probability is between 15 and 28% conditional on having been previously admitted in 1997-8 for some risky behavior related to illicit drug or alcohol use. Second, although admissions for any form of risky behavior in 2000-4 is best predicted by admission for the same behavior in 1997-8, we note that for those admitted for Class-A drugs in 2000-4, 5.4% will have been admitted for cannabis related diagnoses in 1997-8. This is significantly higher than having been previously admitted for alcohol related diagnoses (2.2%) over the same period. This highlights the particularly robust correlation between cannabis use at a given moment in time, and future hospital admissions for Class-A related drug use.

In this paper our focus is on establishing whether a change in police enforcement in the cannabis market - as embodied in the LCWS - has a causal impact on hospital admissions for Class-A drugs. The evidence presented in Table 1 and the existing evidence documenting a linkage between cannabis consumption on the subsequent use of other illicit substances, suggests that as long as the policy affects the usage of cannabis consumption in some way, this is likely to have a knock on effect on the usage of Class-A drugs *in the long run*. It is these longer term effects on public health that we now focus on.

3.3 Empirical Method

To measure the impact of the depenalization policy on hospital admissions rates, we estimate the following balanced panel data specification for diagnosis d in borough b in quarter q and year y,

$$Admit_{dbqy} = \alpha + \beta_0 P_{qy} + \beta_1 [L_b \times P_{qy}] + \beta_2 PP_{qy} + \beta_3 [L_b \times PP_{qy}] + \delta X_{bqy} + \lambda_b + \lambda_q + u_{bqy}, \qquad (2)$$

where $Admit_{dbqy}$ is the number of admissions to hospital where the primary diagnosis relates to Class-A drugs, per thousand of the population as defined in (1). P_{qy} and PP_{qy} are dummies for the policy and post-policy periods respectively and L_b is a dummy for Lambeth. The specification is estimated separately for each age-gender cohort, where the cohort's age is defined as its age on the eve of the introduction of the LCWS policy.

β_0 captures London-wide cohort trends (excluding Lambeth's neighbors) in hospitalization rates occurring at the same time as the LCWS was in operation in Lambeth. β_2 captures longer term London-wide cohort trends in hospitalization rates for the age cohort after the depenalization policy in Lambeth officially ends. This coefficient mostly picks up the natural time profile of any change in hospitalizations

results. For Class-A drug admissions, we include episodes that mention Class-A drugs as either a primary or secondary diagnosis, as the objective is to assess correlations in drug use, not the cause of admission. We exclude those admitted for more than one risky behavior related to cannabis, Class-A drugs and alcohol. Finally, observations for 1999 are dropped to ensure that we only capture new incidents between 1997-8 and the later time period.

as the cohort ages say because of varying usage of illicit substances, or changes in susceptibility to the same levels of usage. These coefficients also partly pick up any impacts on hospitalization rates related to diagnosis-d for London and nationwide policies, including the nationwide depenalization of cannabis possession that occurred from January 2004 through to January 2009.[16] The parameters of interest are estimated using a standard difference-in-difference research design: β_1 and β_3 capture differential changes in hospital admission rates for a given age cohort, in Lambeth during and after the depenalization policy period, relative to other London boroughs excluding Lambeth's neighbors. Our research design identifies whether: (i) hospitalization rates in Lambeth significantly diverge away from London-wide cohort trends during and after the depenalization policy is in place; (ii) these divergences coincide with the depenalization policy's operation in Lambeth.

In X_{bqy} we control for two sets of borough-specific time varying characteristics. The first contains the shares of the population under 5 and over 75 (by borough and year), who place the heaviest burden on health services. Second, X_{bqy} includes controls for admission rates, by borough-quarter-cohort, for conditions that should be unaffected by the LCWS, in particular malignant neoplasms, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These capture contemporaneous changes in healthcare provision or levels of illness in the population that could affect drug-related admissions. The admission rates for these diagnoses are all constructed from the HES administrative records. The fixed effects capture remaining permanent differences in admissions by borough (λ_b) and quarter (λ_q). Observations are weighed by borough shares of the London-wide population. Defining t as quarters since April 1997: $t = [4 \times (y - 1997)] + q$, we assume a Prais-Winsten borough specific AR(1) error structure, $u_{bqy} = u_{bt} = \rho_b u_{bt-1} + e_{bt}$, where e_{bt} is a classical error term. u_{bqy} is borough specific heteroskedastic, and contemporaneously correlated across boroughs.[17]

As with any difference-in-difference research design, the coefficients of interest measure causal impacts only under some identifying assumptions. First, we have to assume common trends in hospitalization

[16] 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 legal classification of cannabis within the UK's three-tiered system. In March 2002 ACMD recommended cannabis be declassified to a Class-C drug, because the existing Class-B classification was, "disproportionate in relation both to its inherent toxicity, and that of other substances...currently 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. This declassification effectively decriminalized the possession of small quantities of cannabis for personal use, mirroring the LCWS policy experiment and also applied to juveniles. Like the LCWS, the nationwide policy would be reversed – on January 26th 2009 as concerns grew over the potential links between cannabis use and mental health, and changes in the composition of psychoactive ingredients in cannabis supply.

[17] While we think it is important to try and control for the general state of health within the borough using the variables described in X_{bqy}, our main results are robust to excluding such controls. Of the health conditions controlled for, there might be some concerns that prolonged cannabis use is correlated to particular respiratory problems. We note that dropping this control leaves our baseline estimates virtually unchanged (at least to two decimal places on the coefficients of interest). We also note that estimating AR(1) error terms is the most conservative approach: allowing standard errors to be clustered either by borough or by borough-year leads to far smaller estimates of standard errors for the main results, as discussed in the Appendix (Table A3).

rates between Lambeth and the rest of London. We later present evidence to establish whether there is any evidence of such convergent/divergent trends in the pre-policy period, and we also estimate our baseline specifications allowing for borough specific linear time trends. Second, we require there to be no 'Ashenfelter dip', that might otherwise indicate the policy were introduced in response to divergent/convergent hospitalization rates between Lambeth and the rest of London. The descriptive time series evidence presented below helps ameliorate this concern. Third, we require there to be no confounding changes on the supply side of medical care impacting hospital admissions for Class-A drug use, nor any other confounding policies impacting such outcomes. We later provide descriptive evidence to show how the availability of health care in Lambeth for such diagnoses changed over time. We also show the impacts of the LCWS on Class-A admissions in Lambeth *prior* to the introduction of the nationwide depenalization policy in 2004. Fourth, we require there to be no differential change in the underlying populations resident in Lambeth and the rest of London that might drive divergences in hospitalization rates for Class-A admissions. Given our hospitalization rates are based on borough of residence and not borough of treatment, we later discuss, given the available evidence, the plausibility of individuals with differing propensities for drug use changing their borough of residence in response to the policy.

3.4 Descriptive Evidence

3.4.1 Hospitalization Counts

Table 2 shows the raw count data (Tot_{dbqy}) for the average number of hospital admissions for diagnosis d, that occur in borough b in quarter q in year y, covering diagnoses related to the use of illicit substances such as Class-A drugs and cannabis, as well as for alcohol (in each case we show the sum of primary and secondary diagnoses). We break down admission numbers for Lambeth and the rest of London, averaging over the pre-policy and post-policy periods. Given that hospitalization rates for such diagnoses are higher for men than women, Table 2 presents the data for three male age cohorts, where age is defined on the eve of the introduction of the LCWS policy. Three points are of note. First, admission rates for Class-A related diagnoses are low in absolute numbers pre-policy for all age cohorts. These low levels of baseline counts imply that large percentage increases can be generated by a small change in the absolute numbers of admissions related to the use of Class-A drugs. Second, for the younger two age cohorts, admission numbers for Class-A diagnoses rise dramatically post-policy. In each case, the absolute increase between the post- and pre-policy periods is larger in Lambeth than the rest of London average (despite Lambeth having higher admission counts than other London boroughs pre-policy for all age cohorts). For the oldest cohort, those aged 35-44 on the eve of the LCWS policy, the count data suggest a slight fall in Class-A admissions in Lambeth but a rise in the average for the rest of London. These broad descriptive patterns in absolute counts will be replicated later in the formal analysis when (2) is estimated for admission rates.

The third point of note from Table 2 on counts relates to diagnoses for cannabis or alcohol. We see that for each male age cohort, admission counts for cannabis related diagnosis are considerably rarer than for Class-A related diagnoses, and this remains true post-policy. As argued above, using these administrative records on hospital admissions, it is therefore considerably harder to statistically detect any significant impact of the LCWS on cannabis use through hospitalizations for cannabis. In contrast, we see that alcohol related hospital admissions are the most frequent for all age cohorts: pre-policy, there are around four times as many such admissions in London on average than for Class-A related diagnoses. Given the body of existing evidence on potential interlinkages between the use of cannabis, Class-A drugs and alcohol, we later examine whether the depenalization policy had any impact on hospital admissions involving alcohol-related diagnoses.

3.4.2 Unconditional Impacts of Hospitalization Rates

The core outcome considered in the empirical analysis is hospital admissions rates as defined in (1). Figure 1A shows the time series for hospital admission rates in Lambeth against the rest of London averages, for each male age cohort. Each figure is centered on the time of policy change in Lambeth: the dashed red lines indicate the start and end points of the official period of operation of the LCWS policy. Each time series is averaged annually. In order to line up with the policy period, each year starts from Q3 of that year and averages to Q2 in the following year (so for example the value for 1997 is the average over 1997Q3-1998Q2). Although the time series for Lambeth is quite volatile, three points emerge from the comparison with other London boroughs: (i) there is no systematic difference in pre-trends between Lambeth and the rest of London at least for the two older age cohorts, nor is there any evidence of an 'Ashenfelter dip' in admission rates in Lambeth just prior to the introduction of the LCWS; (ii) there are divergences in admissions rates in Lambeth relative to the rest of London for each age cohort; (iii) London wide time series in hospital admissions rates appear rather flat and not trending upwards or downwards, certainly for the two older age cohorts. Figure 1B repeats the figures comparing Lambeth only to other boroughs with a similarly (high) incidence of Class-A drug related hospital admissions pre-policy. The same broad patterns can be seen in the three time series for each male age cohort in Lambeth against this control group.[18]

Table 3 then provides descriptive evidence on the unconditional long term effects of the depenalization policy on Class-A related hospital admissions rates, with each row showing hospital admissions rates ($Admit_{dbqy}$) as defined in (1). We again first focus on male cohorts of various ages on the eve of the LCWS policy. Columns 1 and 2 present mean hospital admission rates related to Class-A drug usage in Lambeth during the pre-policy and post-policy periods respectively; Columns 3 and 4 give the corresponding statistics for the average borough in the rest of London (excluding Lambeth's neighboring

[18] Boroughs are defined to have a high drug admissions rate if their average admission rate for all age 15-44 in the pre-policy period exceeds .08 pre-policy, that is just above the mean rate pre-policy. These nine boroughs are Bexley, Bromley, Camden, Croydon, Greenwich, Kensington and Chelsea, Lewisham, Southwark and Westminster.

boroughs). Pre-policy, Lambeth had substantially higher rates of admissions than the London average. Indeed, ranking boroughs by their per-policy hospital admission rates related to Class-A drugs, Lambeth has the third highest for men and second highest for women. However, as suggested in Figure 1 and shown more formally later, there is no evidence of diverging or converging trends in Class-A related hospital admissions rates between Lambeth and the London average in the pre-policy period from 1997 to 2001. In Lambeth, admissions rates in the pre-policy period are lowest for the youngest cohort, reflecting the overall pattern of drug admissions by age.

Comparing Columns 1 and 2 re-iterates the basic pattern of potential health impacts of the depenalization policy, that was previously shown in the raw counts data in Table 2: hospital admission rates in Lambeth rise over time for the 15-24 and 25-34 cohorts, but fall slightly for the oldest cohort. In contrast for the rest of London admissions rates rise only for the youngest cohort and are stable or declining for the older two age cohorts.

Columns 5 and 6 then present difference-in-difference estimates of how Class-A drug admissions rates relate to the LCWS policy. Column 5 shows that unconditional on any other factor, admission rates for both the 15-24 and 25-34 cohorts significantly rise in Lambeth relative to the London borough average, after the introduction of the policy to depenalize the possession of cannabis. The relative increases in admission rates of .054 and .079 per thousand population for the youngest two age cohorts are statistically significant at the 5% level: the increases correspond to a 146% rise relative to the pre-policy level for the 15-24 cohort, and a 44% increase above the baseline level for the cohort aged 25-34 on the eve of the policy. The effect for the oldest cohort is not statistically significantly different from zero. Column 6 then shows this basic pattern of difference-in-differences to remain in magnitude and significance once borough and quarter year fixed effects are controlled for. These results suggest that among younger male age cohorts, the policy of depenalizing the possession of cannabis is associated with significantly higher hospitalization rates in Lambeth for Class-A drug use in the longer term.

Table A1 shows the corresponding results for female age cohorts: we find no significant impacts on Class-A related hospitalization for any female age cohort. The rate of admissions for such diagnoses among women is generally lower than among men and this might be one reason it is harder to statistically detect any impact at conventional significance levels. At the same time, the fact that there are very different trends in hospitalizations for Class-A drugs across genders within Lambeth, suggests the earlier results for men are not merely picking up other changes in hospital behavior or how diagnoses are recorded within Lambeth, that might otherwise have been expected to impact men and women equally.

Table A2 shows the corresponding descriptive evidence for hospital admissions related to cannabis use for male cohorts. Cannabis hospital admission rates are generally lower than for Class-A drugs, especially among older age cohorts, despite much higher levels of cannabis usage as suggested by survey data. The difference-in-difference results suggest the LCWS had no significant impact on hospital admissions for cannabis: the point estimates for the youngest male cohorts are positive but not precisely estimated, and a similar set of findings are obtained when examining the impact of the depenalization

policy on hospitalizations for cannabis related diagnoses among female cohorts (not shown).

To relate these findings to the literature, recall that Model [1993] find that the *de facto* decriminalization of cannabis in twelve US states from the mid-1970s significantly increased cannabis-related emergency room admissions. Chu [2012] similarly finds that the passage of US state laws that allow individuals to use cannabis for medical purposes leads to a significant increase in referred treatments to rehabilitation centers. Our evidence from London suggests that if a similar effect occurs from the depenalization of cannabis possession, it does not then feed through to significantly higher rates of hospitalization that involve extreme consequences on health leading to overnight hospital stays, which is what our inpatient administrative data measures. For the bulk of our core analysis, we therefore continue to focus on Class-A drug-related hospital admissions among men.

4 Baseline Results

4.1 The Impact of the LCWS by Cohort

Table 4 presents estimates of the full baseline specification (2), where we consider the impact of the LCWS on Class-A drug related hospital admissions rates for three male age cohorts in Columns 1 to 3. These findings represent our core results: they show the addition of time varying borough controls (X_{bqy}) produces estimates very similar to the unconditional estimates shown in Table 2. The first row shows that in the longer term post-policy period, there are statistically significant rises in admission rates of .038 and .075 for the youngest two cohorts in Lambeth, relative to other non-neighboring London boroughs. In line with the earlier descriptive evidence, no policy impact is found on the oldest age cohort, that were aged 34-44 on the eve of the LCWS's introduction in Lambeth. Comparing these increases in admission rates to the mean admission rate in Lambeth pre-policy as reported at the foot of Table 4, the percentage increases conditional on other factors are 103% for the youngest age cohort, and 42% for those men aged 25-34 on the eve of the policy, which are slightly smaller than the unconditional percentages reported in relation to Table 3.

The second row of Table 4 shows that in the short-run, during the 13 months in which the LCWS was actually in operation, there are no statistically significant effects on hospitalization rates for any cohort. Hence, as might be expected, any impact of the cannabis depenalization policy on hospitalization rates for Class-A drug use takes time to work through (in line with the descriptive evidence in Table 1). Table 4 also shows the estimates of β_0 and β_2. These highlight that for London as a whole (excluding Lambeth's neighbors), there are no significant long-term time cohort trends in admission rates during and after the policy period for the older two cohorts. For the youngest cohort aged 15-24 (Column 1), hospital admission rates for Class-A drug related admissions are naturally rising over time as the cohort ages, but the results overall show that hospitalization rates in Lambeth are significantly diverging away from this London wide average in the post-policy period, all else equal.

To assess whether these magnitudes are plausible, we note first that all the evidence in Adda *et al.* [2013] points to an increase use of cannabis as a result of the LCWS policy, an impact that lasted well after the policy officially ended (in part because as discussed in Section 2, policing strategy did not revert back identically to what it had been pre-policy). They estimate the size of the cannabis market to have increase by around 60% in Lambeth. For this to translate into a large percentage increase in hospital admissions for Class-A diagnoses would not require a large increase in the absolute number of such cases because the raw number of counts for hospital admissions by borough-quarter-year are low to begin with. More precisely, this pattern of significant policy impacts is robust to using the absolute number of admissions (Tot_{dbqy}) as the dependent variable. In this case, the coefficient of interest β_3 is positive and significant at the 1% level for the two younger male cohorts (the coefficient is 1.71 for those aged 15-24 on the eve of the policy, and is 3.44 for those aged 25-34.[19] The Appendix also presents estimates of the baseline specification using Tobit specifications, that show the robustness of the findings to treating differently borough-quarter-year observations with zero admissions (Table A4).

Taken together, our results suggest the depenalization of cannabis led to longer term increases in the use of Class-A drugs and subsequent hospitalizations related to Class-A drug use among the two youngest aged cohorts on the eve of the LCWS policy. If depenalization led to a decline in the equilibrium price of cannabis in Lambeth, as is often argued to be an unambiguous effect of such policies [Kilmer *et al.* 2010], then this result suggests that cannabis and Class-A drugs have a negative cross-price elasticity, so that the two types of illicit drug are contemporaneous complements, or the use of cannabis leads through some mechanism to the later use of harder illicit drugs.[20] This would be in line with some other studies that have estimated the cross-price elasticity between cannabis and a specific Class-A drug: cocaine, either using decriminalization as a proxy for a price reduction [Thies and Register 1993, Grossman and Chaloupka 1998], or using actual price information [Williams *et al.* 2004].

An obvious concern with these results is that they might in part be confounded by natural time trends, by age cohort, in hospitalization rates for Class-A drugs, that are not fully being captured in the policy and post-policy dummies. To address this, we repeat the analysis but augment (2) with controls for borough specific linear time trends. Columns 4 to 6 in Table 4 present the results for each male age cohort when time trends are conditioned on. We find that for the two older male age cohorts, hospitalization rates are significantly higher in Lambeth relative to the rest of London comparing the post- and pre-policy periods. Hence policy impacts remain even once linear within borough time trends are controlled for, although we note the descriptive evidence in Figure 1 does not provide compelling evidence that such time trends should necessarily be controlled for.

In summary the evidence suggests that there are significant impacts of the the police policy of

[19] We also note the robustness of findings to using a third dependent variable, the log of the number of Class-A related admissions per 1000 of the population plus one, $Ln\left(\frac{Tot_{dbqy}}{Pop_{by}}+1\right)$. In this case, β_3 is again positive and significant at the 1% level for the two younger male cohorts (the coefficient is .034 for those aged 15-24 on the eve of the policy, and is .059 for those aged 25-34).

[20] No reliable information on the price of illicit drugs exists at the borough level for our study period.

depenalizing cannabis on public health, as measured in hospitalization rates for Class-A related drug use. These impacts are quantitatively large, apply to more than one male age cohort, and are observed well after the policy depenalizing the possession of cannabis is officially ended. To be clear, these results cannot be interpreted as suggesting that there are some individuals that *start* taking Class-A drugs as a result of the depenalization of cannabis. All we can infer is that there are individuals, who prior to the policy might either have not been consuming illicit drugs at all, or were consuming them in quantities that did not lead to hospitalization, who are then in the longer term post-policy, significantly impacted by the depenalization policy so as to require hospitalization for diagnoses related to Class-A drug use.

4.2 Robustness Checks

We now present evidence to underpin the credibility of the difference-in-difference research design. These relate to probing the data to: (i) check for pre-existing divergent trends in hospitalization rates between Lambeth and other London boroughs; (ii) evaluate the robustness of the results to alternative control boroughs to compare Lambeth to; (iii) examine whether differential changes over time in health care provision between Lambeth and other locations, or other policies impacting hospitalizations for Class-A drug use, could confound the results, and; (iv) shed light on whether individuals changed borough of residence in response to the policy.

4.2.1 Pre-Trends

The research design implicitly assumes that in the absence of the depenalization policy, there would have been no natural divergence/convergence in admission rates between Lambeth and the rest of London. The previous set of specifications that allowed for borough specific time trends already partly addressed this concern. A second way to address the issue is to exploit the four years of panel data *prior* to the introduction of the depenalization policy, from 1997 Q2 until 2001 Q2, using this period to test whether there is any evidence of a divergence in trends in hospitalization rates between Lambeth and the rest of London pre-policy. To do so, we estimate a specification analogous to (2) in the pre-policy sample but allow for only one split of the sample, midway through the pre-policy period. We then test whether there are divergent trends across Lambeth and the rest of London in admission rates between the first and second halves of the pre-policy period. As Table A5 shows (and consistent with the descriptive evidence in Figure 1), for all male age cohorts this pre-policy sample split dummy interaction is not significantly different from zero suggesting that hospitalization rates in Lambeth are not diverging from London in the years prior to the depenalization policy. As discussed in Section 2, this is very much in line with the policy discussion around the underlying motivation for the policy, that emphasized the policy enabling the police to reallocate their effort towards non-cannabis crime, and which hardly mentioned the potential impacts on public health. Hence the data supports the assertion that the depenalization policy was not introduced specifically into Lambeth because of worsening public health related to drug-

related hospital admissions. Nor is there any evidence of reversion to the mean in hospitalization rates with Lambeth converging back towards London-wide averages. In short, any form of 'Ashenfelter dip' does not appear to be confounding the estimated parameters, as was also suggested by the descriptive evidence in Figure 1.

4.2.2 Control Boroughs

We now examine the robustness of the findings to comparing Lambeth to other subsets of boroughs, rather then all boroughs London wide (excluding only the immediate neighbors of Lambeth). To begin with, we follow on from the descriptive evidence in Figure 1B and compare Lambeth to a more limited set of nine other London boroughs with similarly high levels of hospital admission rates for Class-A drugs pre-policy. As shown in Columns 1-3 of Table A6, with this restricted comparison sample of boroughs to Lambeth, there remains a significant impact of the policy among males aged 25-34 on the eve of the policy, an effect significant at the 1% level. The point estimate of the impact (.104) is actually larger than the corresponding coefficient in the baseline specification (.075), as reported in Column 2 of Table 4. The remaining Columns in Table A6 then show this result to be robust to alternative modifications to the control group of boroughs included: (i) boroughs with the very highest pre-policy admissions related to Class-A drugs (Columns 4-6, restricting the sample to four boroughs); (ii) including neighbors to Lambeth in the control group where spillovers in hospital admissions might have been greatest (Columns 7-9, expanding the sample to 32 boroughs).

Along similar lines, the remaining columns in Table A6 consider restricting the control group of boroughs to those in which there exists: (i) a mental health trust headquarters (there are six such boroughs including Lambeth); (ii) a teaching hospital (there are eight such boroughs including Lambeth).[21] The intuition for these comparisons is that residents of such boroughs might have access to especially high levels of quality in hospital care, or similar degrees of specialization in dealing with mental health disorders associated with the use of illicit drugs as in Lambeth where one mental health trust is headquartered. In line with the baseline results, we see there to be significant impacts on hospitalization rates post-policy in Lambeth for the youngest two age male cohorts in both these restricted samples. Taken together, these comparisons suggest our baseline results are not driven solely by differences in health care between Lambeth and other London boroughs.

4.2.3 Supply Side Changes and Other Confounders

To provide further evidence on whether changes on the supply side of health care could be driving the difference-in-difference estimates, we utilize information in the HES administrative records both on the

[21] An NHS Mental Health Trust provides health and social care services for people with mental health problems in England. There are 60 mental health Trusts in England. A single trust might cover multiple sites (such as hospitals to clinics) and we measure the borough in which each trust is headquartered. The jurisdictions of Trusts do no coincide with boundaries of boroughs.

borough of residence of the individual, and the borough *of treatment* for each hospital episode. We then construct the time series for the percentage of men treated for a Class-A related diagnoses in a Lambeth health facility (by age cohort), that actually reside in Lambeth. To be clear, nearly all London boroughs have a hospital in them (with a handful of boroughs containing two). However, the specialist services required to treat diagnoses involving the use of Class-A drugs, such as those relating to mental health, are more concentrated in a small subset of facilities that are more dispersed across London. Each of these specialist facilities would be expected to treat patients from *across London*. If such services expanded in Lambeth during the post-policy period *and* were especially targeted towards Lambeth residents, then we would expect to observe, over time, a greater share of individuals treated in Lambeth to also reside in Lambeth.

Figure 2 presents the relevant time series evidence on this, again split by male age cohorts where ages are defined on the eve of the policy. Within each cohort, we find no evidence of changes in this percentage over time: for all male cohorts, between 30 and 40% of admissions into hospital are from individuals that are residents of Lambeth, and this does not change much over time. This evidence suggests that even if medical capacity were expanding in Lambeth, it did not lead to a differential treatment of Lambeth residents versus non-residents for the diagnoses related to Class-A drug usage we focus on in our main analysis.[22]

There are other potential confounding factors to consider. First, if the LCWS policy allowed the policy to reallocate their effort towards crime involving Class-A drugs, then the impacts we have documented would not solely be occurring through any demand side linkage between the use of cannabis and Class-A drugs (whether it arise from unobserved heterogeneity or state dependence via a gateway effect). However as discussed in Section 2, the body of evidence presented in Adda *et al.* [2013] suggests the LCWS policy did *not* lead to a reallocation of police resources towards crime related to Class-A drug crime (rather, the police used the policy to reallocate their effort towards non-drug crime). Hence, in the current study, any link between the depenalization policy and hospitalizations for Class-A diagnoses most likely stems from the interlinkages between the demand sides of the markets for cannabis and Class-A drugs.

A second potential confounding factor is that between January 2004 and January 2009 cannabis was declassified from a Class-B drug to a Class-C drug throughout the UK. This declassification effectively decriminalized the possession of small quantities of cannabis for personal use, mirroring the LCWS policy experiment in many ways.[23] Such a nationwide policy would obviously only bias the difference-in-

[22]The South London and Maudsley NHS Trust is headquartered in Lambeth and was formed in 1999. This provides mental health and substance misuse services across Bexley, Bromley, Croydon, Greenwich, Lambeth, Lewisham and Southwark, as well as specialist services for patients from across the UK. Although it did see a rapid growth in patients from its opening, some of these would be re-allocations of patients from other trusts, and as Figure 2 highlights, the evidence does not suggest that it targeted residents of Lambeth (such targeting would also go against its remit).

[23]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

difference estimates that we focus on if its impact differed between Lambeth and other London boroughs. To show the policy impacts we have documented between Lambeth and other London boroughs likely stem from the localized depenalization policy that only operated in Lambeth, we re-estimate our baseline specification (2) using only data running up to 2003 Q4, so up to the point where the nationwide policy change occurred. The result in Columns 1-3 of Table A7 show that for two out of three male age cohorts, there are significant impacts on hospitalization rates for Class-A related diagnoses in Lambeth, even over this restricted post-policy period before any changes in nationwide policy take hold.

4.2.4 Residential Mobility

Throughout the analysis we have used the borough of residence at the time of admission to build hospitalization rates across cohorts. The documented increase in hospital admissions for Class-A drug related diagnoses in Lambeth following the introduction of the LCWS might then operate through two mechanisms: (i) a change in behavior of those resident in Lambeth prior to the policy; (ii) a change in the composition of Lambeth residents, with the policy inducing a net inflow of people into the borough with a higher propensity for Class-A drug use. Undoubtedly, the geographical distances between London boroughs are small and travel costs are low relative to the fixed costs of permanently changing residence. Similarly the nationwide depenalization policy in place between 2004 and 2009 would further have weakened incentives for individuals to relocate residence with Lambeth in response to the LCWS policy. However, if drug users perceive the depenalization of cannabis in Lambeth as signaling a wider weakening of police enforcement against all illicit drugs, there might be longer term benefits to relocating to the borough. Given the importance of assuming the underlying populations, and hence propensity for drug use, in Lambeth and the rest of London to remain unchanged over time in the difference-in-difference design, we now try to use the administrative records to shed some light on the extent to which drug users relocate their residence into Lambeth from other parts of London as a result of the depenalization policy.[24]

The HES data contain information on borough of residence for each individual admission to hospital, with individual identifiers allowing us to link patients across episodes and time. The major limitation of using hospital administrative records to shed light on changes in borough of residence in response to the policy, is that for those that are admitted *only once* during the study period, the data does not allow us to identify whether they have changed residence over time prior to the admission, or will do so subsequent to the admission. These individuals, that form the bulk of hospital admissions and that are

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. Finally, the nationwide policy also applied also to juveniles. Warburton *et al.* [2005] and May *et al.* [2007] discuss the background to this nationwide policy in more detail. They provide descriptive evidence on how it affected the behavior and perceptions of the police and cannabis users.

[24]We thank Jonathan Caulkins and Libor Dusek for comments that have motivated this subsection.

included in the main analysis, cannot be included in the analysis below examining migration patterns. While this obviously limits our ability to shed light on the potential net migration into Lambeth of drug users in response to the depenalization policy, we know of no data set representative at the London borough level, that would match both changes in residence over time with individual hospital admissions or health outcomes over time.

We therefore proceed by documenting changes in borough of residence for those that have at least two admissions into hospital between 1997 and 2007. To get a sense of the sample selection this induces, we note that in the pre-policy period, 326,683 men are admitted into hospital for any diagnosis, of which 10.6% are re-admitted (at least once) somewhere in London during the one-year period in which the LCWS policy is in place, and 25.3% are re-admitted (at least once) anytime in the post-policy period. Among those 1,746 individuals admitted for Class-A drug related diagnosis in the pre-period, only 14.7% are observed being re-admitted for any diagnosis during the policy period, and 28.2% are observed being re-admitted for any diagnosis during the post-policy period.

If individuals are induced to migrate to Lambeth in response to the depenalization policy, they might do so at some point during its actual period of operation between June 2001 and July 2002. To check for this, we first focus on those 1,630 individuals that are admitted to hospital for any diagnosis in Lambeth during the policy period, and that are observed having at least one prior hospital admission somewhere in London pre-policy. Of these 1,630 individuals,1.7% are admitted for Class-A related diagnosis in Lambeth during the policy period. These are perhaps the most likely individuals to have moved to Lambeth in specific response to the depenalization policy. However we note that among this group, almost all their earlier pre-policy admissions (for any diagnosis) occur in Lambeth, so that there is no strong evidence of these individuals having recently moved to Lambeth during the policy period.

While these results focus on those admitted for Class-A drug related diagnosis in Lambeth during the policy period, it might well be the case that drug users that migrate into Lambeth because of the policy are first admitted for some other diagnosis. Hence, we next focus on the 98.3% of hospital admissions in Lambeth during the policy period for any diagnosis unrelated to Class-A drug usage. Among these individuals, nearly all of them are observed with all their earlier admissions in Lambeth; only 10.3% have their last prior admission in some other borough, indicating that they changed their borough of residence at some point between their last admission and the end of the policy period. Taken together, these two pieces of evidence show that among those men with at least two hospital admissions since 1997, there is very limited evidence of there being significant changes of residence into Lambeth during the formal policy period between June 2001 and July 2002.

Our next set of results examines longer term patterns of changes in borough residence. Given the fixed costs of changing residence and that in the post-policy period policy enforcement in Lambeth remained somewhat different than other boroughs, it might be reasonable to assume that a net inflow of drug users into Lambeth simply takes some time to occur. To check for this we examine whether inflows into Lambeth from other London boroughs change between two four-year windows: the first four

year window occurs entirely pre-policy from April 1997 to April 2001, and the second four year window occurs entirely post-policy from April 2003 to April 2007. In each window we check whether among those admitted to hospital at least twice in the four-year window, and, where at least one admission relates to a diagnosis indicating Class-A drug use, whether changes in borough of residence between the first and last admission vary over time.

In the first four-year pre-policy window from 1997-2001 we observe: (i) of those that have their first admission outside of Lambeth, 1.4% are observed with a later admission in Lambeth; (ii) of those that have their first admission in Lambeth,16% are observed with a later admission outside of Lambeth. Doing the same for the later four-year window from 2003-7 to see if this pattern of migration is altered in the longer term, we find that: (i) of those that have their first admission outside of Lambeth, 3.0% are observed with a later admission in Lambeth; (ii) of those that have their first admission in Lambeth, 30% are observed with a later admission outside of Lambeth. Hence there is evidence of more frequent changes of residence among this subsample post-policy, but that this increase occurs both into Lambeth and from Lambeth: the inflow into Lambeth from other boroughs in the post-policy window relative to the pre-policy window increases (3.0% relative to 1.8%), but this is offset by the percentage increase in outflows from Lambeth to other boroughs among such individuals (30% relative to 16%).[25] Overall this suggests is that, among those with multiple hospital admissions, there is increased mobility of residents across boroughs over time, but there is no strong evidence of systematically increased inflows into Lambeth over the second four year window relative to the first.

5 Extended Results

We now consider four margins of policy impact in more detail: the dynamic responses within age cohorts over time, the heterogeneous impacts within age cohorts by previous admission history, spillover impacts onto hospital admissions for *alcohol*-related diagnoses, and the severity of hospital admissions. Establishing the existence and magnitude of each effect is important to feed into any assessment of the overall social costs of this localized change in drug enforcement policy related to the market for cannabis.

5.1 The Dynamics of the Response

When investigating how the impact of the depenalization policy on hospitalizations for Class-A drugs evolves over time, our objectives are two-fold: to assess how long the change in police enforcement for cannabis took to filter through to hospital admissions for Class-A drug related diagnoses, and whether, and how quickly, those effects eventually die out. To chart the time profile of responses, we replace the

[25] One additional strategy we considered to shed light on changes in residence induced by the policy. First, we considered using the administrative records on outpatients, that would include visits to general practitioners and local health clinics. Such events are far more common than hospital admissions. However such data only reliably exists in the post-policy period from 2006/7, and contains no information on diagnosis.

post-policy period indicator in (2), PP_{qy}, with three 2-year time-bins: 1-2 years post reform (Q3 2002 to Q2 2004, TB^1); 3-4 years post reform (Q3 2004 to Q2 2006, TB^2); and, 5-6 years post reform (Q3 2006 to Q2 2008, TB^3). To further check for pre-trends, we also estimate the impacts in one pre-policy period (Q3 1998 to Q2 2000, TB^{-1}),

$$Admit_{dbqy} = \alpha + \beta_0 P_{qy} + \beta_1[L_b \times P_{qy}] + \sum_{k=1}^{3}(\mu_k TB^k_{qy} + \gamma_k[L_b \times TB^k_{qy}]) + (\mu_{-1}TB^{-1}_{qy} + \gamma_{-1}[L_b \times TB^{-1}_{qy}]) + \delta X_{bqy} + \lambda_b + \lambda_q + u_{bqy} \quad (3)$$

where all other variables are as previously defined. This specification is estimated for each 10-year male age cohort. Impacts of LCWS on admission rates in Lambeth, in each time period (β_1,γ_{-1} γ_1, γ_2, and γ_3), are then plotted in Figure 3, where the reference category (γ_0) is the two year window covering the year prior the policy and the year the policy is implemented. The Figure confirms there are no significant pre-trends for any cohort (although the confidence interval for the pre-policy period is wide for the oldest cohort).

On longer term dynamics, Figure 3 shows that for each cohort there is an inverse-U shaped pattern of dynamic responses across time in the post-policy period. For each cohort the depenalization policy has no significant impact on hospitalization rates during the policy period, estimated impacts increase thereafter for some time before starting to decline. In line with the evidence in Table 4, the magnitude of the impacts are largest for those in the younger two cohorts aged 15-24 and 25-34 on the eve of the policy. For these age cohorts: (i) the impacts on hospitalizations related to Class-A drug use take a year or two to emerge after the policy is first initiated; (ii) the post-policy impacts are highest three to four years into the post-policy period, where the peak impacts correspond to a near doubling of admissions rates relative to the pre-policy period for each cohort.

Although the pattern of coefficients for the oldest cohort also follow an inverse-U shape, the sign of the point estimates are quite different by the final period considered: 4-6 years into the post-policy period, there is a significant and *negative* impact on hospitalization rates. This might in part be driven by a different link between the consumption of cannabis and Class-A for those in this age cohort.

In comparison to the literature linking policies regulating the market for illicit drugs and public health, all of these dynamic responses are of significant duration. For example, Dobkin and Nicosia [2009] study the impact of a government program designed to reduce the supply of methamphetamine on hospitalization rates (by targeting precursors to methamphetamine), as well as other outcomes. This policy is sometimes claimed to have been the DEA's greatest success in disrupting the supply of an illicit drug in the US and indeed Dobkin and Nicosia [2009] find that the policy had significant impacts on public health. However, they document that these effects were short lived: within 18 months admissions rates had returned to pre-intervention levels. In contrast, the depenalization policy we document has an impact on hospitalization rates that lasts at least 3-4 years post-policy for two of the three male cohorts

even though the policy itself is only formally in place for a year.

5.2 Admission Histories

We next examine how the long run policy impacts are heterogeneous within the same age cohort. To do so, we exploit the full richness of the administrative records to consider differing impacts by individual histories of hospital admission for drug and alcohol related diagnoses during the pre-policy period from April 1997 to June 2001. This allows us to shed light on whether those with a prior record of heavy substance abuse, respond differentially to the depenalization of cannabis than does the rest of the population. Relative to the existing literature linking drug enforcement policies and health, exploiting this aspect of the data allows us to present novel evidence on the characteristics of the marginal individuals most impacted by a policy of depenalizing cannabis. Examining heterogeneous impacts along this margin is informative because previous heavy users of illicit drugs might be engaged in habitual behaviors so there is less scope for further increases in hospitalization rates for Class-A related diagnoses.

We construct admission histories related to the use of either illicit drugs or alcohol, and create panels by borough-quarter-age cohort-gender, based on those with and without pre-policy histories of admissions related to drugs or alcohol (the latter group is of course orders of magnitude larger than the former group). Among those with no pre-policy admissions, we calculate admission rates as in (1), so per 1000 of the borough population, where by construction this admission rate is zero before the policy. For this group, we can effectively estimate whether the depenalization policy differentially impacted hospital admission rates between Lambeth and other non-neighboring boroughs only in the time period *after* the policy is first initiated (namely from 2001 Q3 until 2009 Q4). Over this period we then estimate the following balanced panel data specification,

$$Admit_{dbqy} = \alpha + \beta_2 PP_{qy} + \beta_3[L_b \times PP_{qy}] + \delta X_{bqy} + \lambda_b + \lambda_q + u_{bqy}. \tag{4}$$

Hence the post-policy impacts are measured relative to the period in which the LCWS policy is actually in place.

For those with pre-policy histories of admission, we denote the admission rate for diagnosis d, borough of residence b in quarter q of year y as $Admit^{history}_{dbqy}$. We then estimate a specification analogous to (2) over the entire sample period but the numerator for the dependent variable ($Admit^{history}_{dbqy}$) is not the borough population, but rather the number of distinct individuals admitted for diagnoses related to illicit drugs or alcohol whilst residing in borough b between April 1997 and June 2001, that we denote $Admitted_{b,pre}$. These borough-quarter-year aggregates are constructed from 1,888 individuals in the 15-24 age cohort (of which 89 reside in Lambeth), 4,544 in the 25-34 age cohort (of which 385 reside in

Lambeth), and 6,482 in the oldest age cohort (of which 599 reside in Lambeth). Hence,

$$Admit_{dbqy}^{history} = \frac{Tot_{dbqy}^{history}}{Admitted_{b,pre}}, \tag{5}$$

where $Tot_{dbqy}^{history}$ are total number of hospital admissions for diagnosis d, amongst those residing in borough b, in quarter q of year y that have a history of admissions pre-policy. Note that this dependent variable changes over time only because of changes in the numerator: the denominator holds fixed the 'at risk' population of all those with a history of hospital admission for drug and alcohol related diagnoses pre-policy. Given this difference in how the dependent variable is defined, the magnitude of the policy impacts for those with admission histories are not directly comparable to those without admissions histories nor to the baseline results previously reported (that both use admission numbers per 1000 of the borough population).

The results are presented in Table 5. Columns 1 to 3 consider admissions among male each age cohort for those *without* a prior record of admissions. The evidence suggests that for the oldest male age cohort, there is a significant increase in Class-A drug related hospitalizations in Lambeth relative to the rest of London in the post-policy period relative to the policy period. The London-wide trends in admissions rates post-policy shown in Columns 1 to 3 ($\hat{\beta}_2$) reflect how this samples is defined: admission rates for those without previous admissions must necessarily rise (weakly) over time as the cohort ages, given admission rates start at zero by construction and cannot be negative. The data suggests that this upward cohort trend is significantly more pronounced in Lambeth post-policy for the oldest age cohort. The magnitude of the impact for this cohort is large: an increase in hospitalization rates by .160 corresponds to 44% of the pre-policy hospitalization rate for this cohort as a whole. To be clear, among this cohort we pick up the combined impacts among those that *were previously* using illicit drugs (and potentially other substances) but not so heavily so as to induce hospitalizations, as well as those that *begin* to use cannabis and Class-A drugs for the first time as a result of the price impacts on cannabis of the depenalization policy. The administrative data utilized does not allow us to separate out the policy impacts stemming from each type of individual.

Columns 4-6 in Table 5 consider policy impacts within each age cohort among those that have a prior history of at least one hospitalization for drug or alcohol related diagnoses. The results suggests that in the longer term such cohorts are either not affected by the depenalization policy, or for the oldest age cohort, their admission rates significantly decline in Lambeth in the long term.[26] Such long term users, at least among the two younger cohorts, might be more habituated in their behavior and less price sensitive to any change in price of cannabis induced by the depenalization policy. If so, this

[26] This downward trend among the specifications based on those with admissions histories partially reflects the fact that not all such individuals are admitted more than once. Of the 12271 individuals admitted for drugs or alcohol related diagnoses in the pre-policy period, only 56% (6871 individuals) have a second admission at any point during the sample period, and only 38% (4684 individuals) have another episode in the policy or post-policy period, and this naturally induces a downward time trend to be picked up in β_0 and β_2 in (2).

result would be consistent with the evidence based on NLSY97 data in Deza [2011] who uses a dynamic discrete choice model to document that the gateway effect from cannabis to hard drugs use is weaker among older age cohorts.

An obvious concern with these results is that they might in part be confounded by natural time trends in hospitalizations for Class-A drugs. These time trends might also differ across age groups and by hospital admissions histories. To address this, we repeat the analysis but augment (4) and (2) with controls for borough specific linear time trends. Table A8 presents the results, again broken down for cohorts based on age and prior admissions histories.[27] The inclusion of borough specific linear time trends serves to reinforce the earlier conclusions among those without a prior history of admissions (Columns 1-3, Table A8). Among those with a history of admissions, we continue to find no impact among the two youngest age cohorts, although among the oldest cohort the policy now has a positive and significant impact on hospitalization rates.[28]

5.3 Alcohol

There is an established body of empirical work examining the relationship between cannabis and alcohol use: this has generated mixed results with some research finding evidence of the two being complements [Pacula 1988, Farrelly *et al.* 1999, Williams *et al.* 2004], and other studies suggesting the two are substitutes [DiNardo and Lemieux 2001, Crost and Guerrero 2012, Anderson *et al.* 2013], or that there is no statistically significant relationship between the two [Crost and Rees 2013, Yörük and Yörük 2013]. Many of these studies have identified these impacts among young people, sometimes exploiting minimum legal drinking age that should create discontinuities in alcohol consumption for those aged around 21.

We provide a novel contribution to this debate by examining the effect of depenalization on *extreme* forms of alcohol usage, leading to hospitalizations. We do so for all three male age cohorts. As documented in the raw counts data in Table 2, such admissions occur with far higher frequency for men in all age cohorts, than admissions for either Class-A drugs or cannabis. Hence any positive or negative impacts on admissions for alcohol can have dramatic implications for the monetary health costs of the

[27] For the specifications in Columns 1-3 of Table A8 based on samples without a prior record of hospital admissions the sample is only defined from the time the LCWS is initiated and is allowed to be linear thereafter ($\lambda_b \times$quarters post Q3 2001). For the specifications by age cohort with a history of admissions in Columns 4-6 of Table A8, the borough specific time trend is assumed to be linear over the sample period.

[28] We have also tried to investigate one other source of heterogeneous responses within age group: by diagnosis for Class-A admissions. More precisely, we split ICD-10 diagnosis for Class-A drug admissions into two types (using primary and secondary diagnoses): (i) those related to mental health and behavioral disorder (corresponding to ICD-10 codes F11, F14, F16); (ii) those related to acute conditions such as poisoning, and finding the drug in the blood corresponding to ICD-10 codes T400-T406 T408-T409, R781-R785). The former better reflects longer term health problems, and the latter better reflects acute issues related to use. On the relative frequency of these two types of diagnosis, we note that diagnoses related to mental health/behavioral disorders are far more prevalent for the 25-34 and 34-44 age groups: these account for 75% of all Class-A related admissions in Lambeth, while for the 15-24 age group there is a more even split across the diagnoses types. However, when we split along these lines, the results are inconclusive: there is a significant increase in mental health disorders among those aged 15-24 on the eve of the policy, and the point estimate is also positive for the 25-34 age cohort, although not statistically significant (results available upon request).

policy. Throughout, we measure admission rates for alcohol related diagnoses analogously to those used as our dependent variable in the baseline specifications, (1).

To begin with, we focus on admissions for alcohol related diagnoses where the primary diagnosis refers to alcohol. We exclude any admission that additionally refers to the use of Class-A substances as the secondary cause of admission. The results in Columns 1 to 3 of Table 6 show there to be a significant *reduction* in alcohol-related admissions among the youngest cohort in Lambeth relative to the rest of London, but no impacts on such alcohol-related admissions for older cohorts. The result suggests that for the youngest age cohort, if depenalization causes the price of cannabis to fall, then alcohol and cannabis might well be substitutes.

The next set of specifications probe further to examine the evidence of whether and how the policy impacts the *combined* use of alcohol and Class-A drugs: here we define admissions rates where the primary diagnosis is again for alcohol-related diagnosis, but the secondary diagnosis refers to the use of Class-A drugs. We find no evidence the policy causes such combined admissions to change in the longer term (and this occurs against a backdrop of London wide increases in such combined diagnosis admissions). Again, if the depenalization of cannabis caused increased cannabis and Class-A drug use in the longer term, this last set of results supports the assertion that such substances are not being used together, at least among those most prone to extreme abuse of such substances.

5.4 Severity of Hospital Admissions

A final dimension along which to consider policy impacts relates to the severity of hospitalizations, as measured by the number of days the individual is required to stay in hospital for conditional on admittance. This margin is of policy relevance because it maps directly into the resultant healthcare costs associated with the depenalization of cannabis, as calculated in the next Section. We therefore first document how the length of *individual* hospital episodes for diagnoses related to Class-A drug use changes differentially between Lambeth and other London boroughs post-policy relative to the pre-policy period. To do so, we estimate a specification analogous to (2) but where the dependent variable is the individual length of hospital stay in days and the sample is confined to episodes where the primary diagnoses relates to Class-A drugs. We focus on the first episode for any hospital stay (that is the same as the entire hospital stay for 93% of observations), and to avoid the results being driven by outliers, we drop observations where the length of the stay is recorded to be longer than 100 days (that excludes a further 2% of all stays).[29] As the outcome variable now relates to individual outcomes (rather than borough-quarter-year aggregates), we cluster standard errors by borough to capture unobservables determining the length of hospital stays that are assumed correlated across residents of the same borough.

Columns 1 to 3 in Table 7 present the results, again split by age cohort. The data suggests that in the longer term post-policy, across all three age cohorts, the length of stay for Class-A drug related

[29] These results remain robust to having the dependent variable specified in logs so that outliers are less likely to drive the estimated impacts.

admissions significantly increases in Lambeth relative to the London average. For example, among the 15-24 age cohort, hospital stays increased by 3.7 days, and this is relative to a baseline pre-policy hospital stay length of 7.2 days, an increase of 49%. The proportionate changes for the other age cohorts are 29% for the 25-34 age cohort and 20% for the oldest age cohort. Hence, the proportionate changes in length of hospital stay are greater for age cohorts that were younger at the time the depenalization policy was introduced. This emphasizes that quite apart from the impacts of the depenalization of cannabis on hospitalization rates for Class-A diagnosis that has been the focus of our analysis so far, the policy also has impacts on the severity of those admissions for Class-A drug use. Both margins are relevant for thinking through the public health costs of the policy as detailed in the next subsection.

We note further that the coefficients in the third row of Table 7 show that in other London boroughs there are *negative* time trends in the duration of such individual hospitalizations conditional on all other controls in (2). Hence the findings for Lambeth post-policy do not appear to be driven by some systematic lengthening of hospital stays for such diagnosis that might be occurring more generally across London.

6 The Public Health Costs of the Depenalization Policy

Our final set of results attempt to provide a lower bound estimate of the public health costs to Lambeth associated with the depenalization policy, as measured exclusively through hospitalizations related to Class-A drug usage. This combines the earlier unconditional estimates from Table 3 (that do not differ much from the baseline estimates in Table 4) on changes in the number of individuals being hospitalized for such diagnosis, and the results from Table 7 showing the policy impacts on the length of hospital episodes, holding constant hospitalization rates related to Class-A diagnoses. Combining the evidence on both margins allows us to infer an overall lower bound increase in hospital bed-days related to Class-A drug use attributable to the depenalization policy. Specifically, the change in average hospital-bed days from the pre to the post policy periods, per quarter for residents of borough b in cohort c is given by,

$$\triangle H_{bc} \quad = \quad N_{post,bc}\overline{L_{post,bc}} \quad - \quad N_{pre,bc}\overline{L_{pre,bc}} \tag{6}$$

where $N_{post,bc}$ represents the number of admissions per quarter in the post period in borough b for cohort c, and $\overline{L}_{post,bc}$ is the average length of stay of those that are admitted in this group in the post-policy period; $N_{pre,bc}$ and $\overline{L_{pre,bc}}$ are of course analogously defined over the pre-policy period. Rearranging (6), the change in hospital bed-days can be decomposed as occurring through two channels,

$$\triangle H_{bc} = (N_{post,bc} - N_{pre,bc})\overline{L_{pre,bc}} + N_{post,bc}(\overline{L_{post,bc}} - \overline{L_{pre,bc}}) \tag{7}$$

The first channel represents the policy impact occurring through a change in the number of hospital admissions for Class-A diagnoses, holding constant the length of stay fixed at the pre-reform levels. The $(N_{post,bc} - N_{pre,bc})$ term can be straightforwardly derived from the unconditional baseline estimates presented in Table 2. The second channel represents the policy impact through a change in the average length of hospital stays, holding constant admission numbers at the post-policy level. The $(\overline{L_{post,bc}} - \overline{L_{pre,bc}})$ corresponds exactly to the estimates reported for each cohort in Table 7. The total public health cost of the policy is then $\triangle H_{bc}$ multiplied by the cost of a hospital bed-day.

We use published National Health Service estimates of the cost per hospital bed-day.[30] This cost is comprised largely of hospital ward costs (nursing, therapies, basic diagnostics and overheads), hence there is actually little variation by diagnosis: the average cost per additional bed day across all adult inpatient diagnoses categories is £240 [Department of Health 2012], but the upper end of the hospital bed-day costs, relating to those for adult acute (inpatient) mental health stays, are only slightly higher at £295 [PSSRU 2011]. We therefore use a figure between these estimates, of £250 per hospital bed-day, as quoted by the NHS Institute [NHS Institute 2012]. This likely represents a lower bound of the true cost of a hospital bed-day because it does not include any specific treatment costs or the additional costs from any associated stay in intensive care.[31]

We then take our estimates from Tables 2 and 7 for each age cohort to calculate each component of (7). For the youngest age cohort of those 15-24 on the eve of the depenalization policy, $\triangle H_{bc}$ =27.2 bed-days per quarter; of this total change, 24.1 bed-days operate through the first channel of increased hospitalization rates, and 4.1 bed-days through the second channel of longer hospital stays conditional on admission. Among those aged 25-34 on the eve of the policy, $\triangle H_{bc}$ =52.5 bed-days per quarter, where the first channel corresponds to an increase of 42.6 bed-days, and the second component generates an increase of 9.9 bed-days. Finally, for the oldest cohort of 35-44 year olds, $\triangle H_{bc}$ =-26.9 bed-days per quarter, where the point estimate in the third row of Table 2 implies a decrease in hospitalization rates post-policy of 30.7 bed-days (although this point estimate was not statistically different from zero), and this is only partially offset by the increase through the second channel of 3.8 bed-days.

Applying the estimated costs per hospital bed-day of £250 to the change in the total number of hospital bed-days per quarter, for each cohort in Lambeth in the post-policy period on average, reveals the increased public health cost to be: (i) £6,802 among those aged 15-24 on the eve of the policy; (ii) £13,136 among those aged 25-34 on the eve of the policy. Summing across four quarters we derive the conservative public health cost of the depenalization policy to be £79,752 per annum, on average across

[30]Since 2003/4, hospitals have been paid a price or ''tariff' for each patient, based on the diagnosis group. These are based on the average stay for the diagnosis group. Additional days spent in hospital are paid at a daily rate. Prior to 2003/4, including the period covered by the LCWS, funding was not as strongly linked to patient numbers or diagnoses.

[31]The NHS does not break down daily hospital bed costs into anything analogous to fixed and marginal cost, or variations by diagnosis. This is partly because prior to 2003/4, NHS hospitals were funded through block grants, with volume guarantees. Hospitals were not therefore required to collect or publish costs about specific procedures or treatment. Since 2003/04 a subset of NHS hospital activity has been subject to Payment by Results (PbR), so that hospitals are paid on the basis of their activity. PbR initially covered a few select elective procedures, but expanded to cover 60% of an average hospitals activity by the end of 2012. PbR was not introduced to Mental Health care until 2013/14.

all the post-policy years in the sample.

There are a number of ways this monetary amount can be benchmarked. One way to do this would be relative to health costs in Lambeth as a whole. However, there are multiple components of health costs related to preventative and curative care, and it is unclear which subset of these costs provide the most appropriate benchmark. Moreover, in England health expenditures stem from both local borough sources but also expenditures of the national government. Given these complications, perhaps the more transparent method through which the benchmark the public heath costs of the depenalization policy is to compare it to the *London-wide* time trends in hospital bed-days, by cohort. This provides a sense of the increased public costs through natural rises over time in hospital bed-days for hospitalizations related to Class-A drug use that would have to be borne between the pre and post policy periods absent the depenalization policy. More precisely this London-wide time trend for cohort c is given by,

$$\triangle H_c = (N_{post,c} - N_{pre,c})\overline{L_{pre,c}} + N_{post,c}(\overline{L_{post,c}} - \overline{L_{pre,c}}), \tag{8}$$

where $(N_{post,c} - N_{pre,c})$ can be derived from the coefficient on the post-policy dummy presented in the unconditional estimates in Table 2, and $(\overline{L_{post,c}} - \overline{L_{pre,c}})$ is measured from the coefficient on the post policy dummy in Table 7. For the youngest age cohort of those 15-24 on the eve of the depenalization policy, $\triangle H_c$ =3.79 bed-days per quarter; of this total change, -4.7 bed-days operate through the first channel of increased hospitalization rates, and 8.5 bed-days through the second channel of longer hospital stays conditional on admission. Among those aged 25-34 on the eve of the policy, $\triangle H_c = -3.5$ bed-days per quarter, where the first channel corresponds to a decrease of 6.5 bed-days, and the second component generates an increase of 3.0 bed-days. Finally, for the oldest cohort of 35-44 year olds, $\triangle H_{bc} = -2.4$ bed-days per quarter, where the first channel corresponds to an increase of 2.6 bed-days, and the second component generates a decrease of 5.2 bed-days.

Aggregating these cohorts across four quarters then suggests the natural *decrease* in costs associated with hospital bed-days is £4,935. Hence the increase in bed-days attributable to the policy more than offsets this natural decrease in hospital bed-days attributable to London wide time trends.

Of course, this calculation still underestimates the total public costs of the increased hospital bed days within Lambeth due to the policy because of the existence of many additional channels that we have ignored. First, we have ignored any additional demands placed on other parts of the national health service unrelated to hospital inpatient stays, as a result of the depenalization policy. These include demands through outpatient appointments, hospital emergency departments, and through treatment centers. Indeed, the existing evidence from the US on the link between the availability of cannabis and health relate to emergency or treatment costs: Model [1993] find that the *de facto* decriminalization of cannabis in twelve US states from the mid-1970s significantly increased cannabis-related emergency room admissions. Chu [2012] similarly finds that the passage of US state laws that allow individuals to use cannabis for medical purposes leads to a significant increase in referred treatments to rehabilitation centers. Second, we have ignored any cost to individual users of being hospitalized. Such events almost

surely impact individual welfare, especially given the robust association found across countries in the gradient between health and life satisfaction.

7 Discussion

We evaluate the impact of a policing experiment that depenalized the possession of small quantities of cannabis in the London borough of Lambeth, on hospital admissions related to illicit drug use. Despite health costs being a major social cost associated with markets for illicit drugs, evidence on the link between how such markets are regulated and public health remains scarce. Our analysis provides novel evidence on this relationship, at a time when many countries are debating moving towards more liberal policies towards illicit drugs markets. We have exploited administrative records on individual hospital admissions classified by ICD-10 diagnosis codes. We use these records to construct a quarterly panel data set by London borough running from 1997 to 2009 to estimate the short and long run impacts of the depenalization policy unilaterally introduced in Lambeth between 2001 and 2002.

We find the depenalization of cannabis had significant longer term impacts on hospital admissions related to the use of hard drugs. Among Lambeth residents, the impacts are concentrated among men in younger age cohorts. The dynamic impacts across cohorts vary in profile with some cohorts experiencing hospitalization rates remaining above pre-intervention levels three to four years after the depenalization policy is first introduced. We combine these estimated impacts on hospitalization rates with estimates on how the policy impacted the severity of hospital admissions to provide a lower bound estimate of the public health cost of the depenalization policy.

Our analysis contributes to the nascent literature evaluating the health impacts of changes in enforcement policies in the market for illicit drugs. The depenalization of cannabis is one of the most common forms of such policy either implemented (such as in the Netherlands, Australia and Portugal) or being debated around the world (such as in many countries in Latin America). The practical way in which the localized depenalization policy we study was implemented is very much in line with policy changes in other countries that have changed enforcement strategies in illicit drug markets and as such we expect our results to have external validity to those settings. However unlike those settings, we are able to exploit a (within-city) borough level intervention and so estimate the policy impacts using a difference-in-difference design, as well as exploring differential impacts across population cohorts, where cohorts are defined by gender, age, previous admissions history, and borough of residence. This is different from much of the earlier research that, with the exception of studies based on US or Australian data, can typically only study nationwide changes in drug enforcement policies such as depenalization, and have therefore had to rely on time variation alone to identify policy impacts [Reuter 2010]. The administrative records we exploit allow us to provide novel evidence on how the impacts of such policies vary across population cohorts, over time within a cohort, and how they interact with potential changes of residence of drug users.

Clearly, such policy impacts are unlikely to ever be estimated using randomized control trial research designs. We have used a difference-in-difference research design exploiting an unusual policy experiment in one London borough that allows us to exploit within and across borough differences in health outcomes to identify policy impacts. The key concern with such a research design is to distinguish policy impacts from time trends. To do so, we have used the detailed administrative records to present evidence on how different cohorts (by gender, age and previous admissions history) are differentially impacted by the policy, how the results are strengthened when controlling for time trends, and checked for the presence of trends in the pre-policy period.

Our results suggest policing strategies have significant, nuanced and lasting impacts on public health. In particular our results provide a note of caution to moves to adopt more liberal approaches to the regulation of illicit drug markets, as typically embodied in policies such as the depenalization of cannabis. While such policies may well have numerous benefits such as preventing many young people from being criminalized (around 70% of drug-related criminal offenses relate to cannabis possession in London over the study period), allowing the police to reallocate their effort towards other crime types and indeed reduce total crime overall [Adda *et al.* 2013], there remain potentially offsetting costs related to public health that also need to be factored into any cost benefit analysis of such approaches.

Two further broad points are worth reiterating. First, our analysis relates to the more general study of the interplay between the consumption of different types of drug. In particular there is a large literature testing for the "gateway hypothesis" that the consumption of one "soft" drug causally increases the probability of subsequently using a "harder drug". The crucial challenge for identification is the potential for unobserved factors or heterogeneity that could drive consumption of multiple types of drug. Existing work has tried to tackle this problem by either: (i) instrumenting the gateway drug with a factor unrelated to the underlying heterogeneity, typically using cigarette and alcohol prices [Pacula 1998, DiNardo and Lemieux 2001, Beenstock and Rahav 2002]; or, (ii) using econometric techniques to model the possible effects of unobserved heterogeneity [Pudney 2003, van Ours 2003, Melberg *et al.* 2010]. To be clear, in our analysis we make no attempt to test for gateway effects directly, but our contribution to this literature is to demonstrate that the markets for cannabis and hard drugs are concretely linked - be it because of gateway effects or some other channel - so that changes in policy that affect one market will have important repercussions for the other [DeSimone and Farrelly 2003, van Ours and Williams 2007, Bretteville-Jensen *et al.* 2008].

Finally, our analysis highlights the impact that policing strategies can have on public health more broadly. It is possible that other policing strategies, such as police visibility or zero-tolerance policies, could also have first order implications for public health. These effects could operate through a multitude of channels including: (i) police behavior directly impacting markets and activities that determine individual health, such as the case studied in this paper; (ii) police behavior affecting perceptions of crime and thus influencing psychic well-being. This possibility opens up a rich area of further study at the nexus of the economics of crime and health.

8 Appendix

8.1 Standard Errors

Throughout the analysis, when estimating policy impacts on hospitalization rates, we have assumed the disturbance terms follow a Prais-Winsten borough specific AR(1) error structure, as described in Section 3.3. In Appendix Table A3 we show the sensitivity of our baseline results to the alternative assumption that standard errors are clustered by borough, without any imposing any further assumptions on the correlation structure within borough. The results are shown in Columns 4 to 6 of Table A3, where as a point of comparison we repeat our baseline specification in Columns 1 to 3. We see that the standard errors are far smaller assuming clustering by borough: they are at least half the magnitude on the coefficient of interest, and as a result, we find significant impacts on all three male age cohorts. One concern with such clustered standard errors is that raised by Cameron *et al.* [2008]: cluster-robust standard errors may be downwards biased when the number of clusters is small (and in our specification the number of clusters corresponds to 28, the number of boroughs in the sample). They propose various asymptotic refinements using bootstrap techniques, finding that the wild cluster bootstrap-t technique performs particularly well in their Monte Carlo simulations. We have implemented this method on our baseline specifications and show in brackets in Columns 4 to 6 the resulting p-values. This does not alter the significance of any of the coefficients shown in Table A3 ($\hat{\beta}_0$,$\hat{\beta}_1$, $\hat{\beta}_2$, $\hat{\beta}_3$). In short, the AR(1) error structure assumed for our main results produces by far the most conservatively estimated standard errors.

8.2 Tobit Estimates

In our baseline specification, the dependent variable is the hospital admissions rate, defined in (1). By definition this variable cannot be negative. We now present a robustness check on our baseline results using Tobit estimates that treat zeroes differently from strictly positive values.[32] The Tobit model allows us to estimate the impact of the policy on both the extensive margin (i.e. the probability that there is at least one admission in a given borough-quarter) and the intensive margins (the admission rate per borough-quarter, conditional on at least one admission). However, the introduction of non-linearity means the difference-in-difference coefficient no longer equals the marginal effect of the interaction term [Ai and Norton 2003]. Policy impacts are therefore produced by using our Tobit estimates to calculate

[32] Zeroes are mostly an issue for the youngest cohort – those aged 15-24. In Lambeth there are no quarter-year observations in the pre or post policy period in which zero hospitalizations are recorded for Class-A drugs. For the youngest cohort aged 15-24, around 70% of observations are zero pre-policy, and 3% are zero post policy. In the rest of London in the pre-period, around one-third (two-thirds) of borough-quarter-year observations are zero for the 25/24 and 35/44 (15-24) age cohorts. In the post-policy period this falls to zero for the two older age cohorts (as for Lambeth) and falls to around one third for the youngest cohort. The proportion of zeroes is lower in Lambeth than the London wide average because Lambeth is a high-incidence borough.

the average interaction term for $PP_{qy} \times Lambeth$ and $P_{qy} \times Lambeth$.[33] Estimated policy effects on the extensive and intensive margins are presented in Table A4 by male age cohort. In line with the results in Table 4, the policy leads to a statistically significant increase in admission rates on the intensive margin, that is an increase in the admission rate conditional on at least one admission per borough quarter, for the two youngest age cohorts. On the extensive margin, namely the probability of a positive admission rate, the impact is positive but not statistically significant except for the oldest cohort.

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[33] Following Buis [2010], given that both interacted variables are binary, the average interaction effect on each margin can be calculated by: first, using the Tobit estimates to produce the conditional expected value of admissions for the four Lambeth x policy period (PP_{qy} or P_{qy}) cells (e.g, Lambeth = 0, PP_{qy}=0; Lambeth = 1, PP_{qy} = 0; Lambeth = 0, PP_{qy} = 1; Lambeth = 0, PP_{qy} = 0); and, second, taking the double difference of those conditional expected admission rates. The average interaction effect for the intensive margin in the post-policy period is therefore equal to the following:

$$\begin{aligned}\widehat{\beta_3} = (\widehat{E}[AR_{qyb}|PP_{qy}=1, L_b=1, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, AR_{qyb}>0]- \\ \widehat{E}[AR_{qyb}|PP_{qy}=0, L_b=1, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, Admits_{qyb}>0])- \\ (\widehat{E}[AR_{qyb}|PP_{qy}=1, L_b=0, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, Admits_{qyb}>0]- \\ \widehat{E}[AR_{qyb}|PP_{qy}=0, L_b=0, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, Admits_{qyb}>0]) \quad (9)\end{aligned}$$

where the conditional expected values are taken over all observations and then averaged. The corresponding difference-in-difference coefficient on the extensive margin (the probability of a non-zero admission rate) can be calculated analogously. The exercise is repeated for the policy-period.

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Table 1: Hospital Re-admission Probabilities

Means, standard deviations in parentheses, standard errors in square brackets

	Admitted in 1997 or 1998 for:			
	(1) Cannabis Related Diagnoses	**(2) Class-A Drug Related Diagnoses**	**(3) Alcohol Related Diagnoses**	**(4) All Other Diagnoses**
Admitted in 2000-2004 for:				
Cannabis Related Diagnoses	.092	.011	.005	.001
	(.289)	(.105)	(.071)	(.034)
Class-A Related Diagnoses	.054	.257	.022	.004
	(.227)	(.440)	(.145)	(.061)
Alcohol Related Diagnoses	.060	.064	.225	.015
	(.238)	(.245)	(.418)	(.121)
All Other Diagnoses	.283	.146	.208	.316
	(.451)	(.353)	(.409)	(.465)
Observations (individuals)	533	3950	15595	485992

Notes: The figures refer to the probability of re-admission as a hospital inpatient over the period 2000 to 2004 (as shown in each row), conditional on an earlier hospital admission in 1997 or 1998 (as shown in each column). Class-A drugs include cocaine, opioids, and hallucinogens. For each type of admission related to a risky behavior (Class-A drugs, cannabis, alcohol), we include episodes that mention this substance as either a primary or secondary diagnosis. We exclude a small number of cases for those admitted for more than one behavior related to cannabis, Class-A drugs and alcohol in 1997 or 1998. The sample is based on all men aged 10-39 on 1st July 2001, the eve of the LCWS policy. The sample is drawn from all London boroughs, except Lambeth, plus all unitary authorities Greater Manchester, Merseyside, the West Midlands, Tyne and Wear, and South Yorkshire (that are outside of London). The total numbers of men admitted between 2000 and 2004 are as follows: 3446 for cannabis related diagnoses; 14105 for Class-A drug related diagnoses; 53033 of alcohol related diagnoses; 1325795 for all other diagnoses. The ICD-10 classifications used for each diagnosis are as follows: Class-A drug related (F11, F14, F16, T400-T406, T408-T409, R781-R785); Cannabis related (F12, T407); Alcohol related (F10, X45, X65, Y90, and Y91).

Table 2: Hospital Admission Counts by Male Age Cohort, Diagnosis, Borough and Time Period

Number of Individuals Admitted Into Hospital, by Cohort-Borough-Time Period Cells

		Pre-Policy		Post-Policy	
Male Age Cohort	**Diagnosis**	**Rest of London**	**Lambeth**	**Rest of London**	**Lambeth**
15-24	**Class-A Drug**	.722	1.30	3.79	7.53
	Cannabis	.339	.942	1.53	3.16
	Alcohol	2.40	3.30	10.8	14.6
25-34	**Class-A Drug**	3.14	11.5	6.25	17.6
	Cannabis	.475	2.59	.978	3.13
	Alcohol	6.34	15.2	22.1	40.9
35-44	**Class-A Drug**	2.75	15.3	4.20	13.0
	Cannabis	.309	2.94	.625	1.89
	Alcohol	12.8	38.1	31.6	61.6

Notes: The table shows the number of men admitted to hospital for each diagnosis, by age cohort, time period and borough. Each count data includes diagnoses for primary and secondary causes. Class-A drugs include cocaine, opioids, and hallucinogens. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. The unit of observation is the number of admissions per borough-quarter-year, averaged across quarters for all boroughs and years in each cell. The pre-policy period from Q1 1997 to Q2 2001. The post policy period runs from Q3 2003 to Q4 2009. The rest of London refers to all other London boroughs excluding Lambeth. The ICD-10 classifications used for each diagnosis are as follows: Class-A drug related (F11, F14, F16, T400-T406, T408-T409, R781-R785); Cannabis related (F12, T407); Alcohol related (F10, X45, X65, Y90, and Y91).

Table 3: Class-A Drug Related Hospital Admission Rates for Male Cohorts, by Borough and Time Period

Means, standard deviations in parentheses, standard errors in square brackets

	Lambeth		Rest of London		Post-Policy Minus Pre-Policy	
	(1) Pre-Policy	(2) Post-Policy	(3) Pre-Policy	(4) Post-Policy	(5) Unconditional	(6) Fixed Effects
Men aged 15-24	.037	.131	.028	.069	.054**	.054**
	(.067)	(.082)	(.049)	(.074)	[.022]	[.022]
Men aged 25-34	.179	.259	.084	.086	.079**	.080**
	(.103)	(.127)	(.094)	(.086)	[.034]	[.034]
Men aged 35-44	.362	.311	.069	.061	-.039	-.039
	(.122)	(.186)	(.088)	(.080)	[.065]	[.065]
Observations (borough-quarter-year)	17	30	459	810	-	-

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the number of male Class-A drug related hospital admissions per 1000 of the male population in the cohort, where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. All observations are at the borough-quarter-year level, and are weighted by the population of the borough relative to the population of London. In Columns 1 and 3 the pre-policy period runs from Q1 1997 to Q2 2001. The policy period runs from Q3 2001 to Q2 2002. In Columns 2 and 4 the post-policy period runs from Q3 2001 to Q4 2009. In Columns 3 and 4 the sample is based on all London boroughs excluding Lambeth and boroughs neighboring Lambeth. In Columns 5 and 6, standard errors on differences are calculated assuming a Prais-Winsten borough specific AR(1) error structure, that allows for borough specific heteroskedasticity and error terms to be contemporaneously correlated across boroughs. In Column 6 the differences are calculated from a regression specification that also controls for borough and quarter fixed effects.

Table 4: The Impact of the LCWS on Hospital Admision Rates for Class-A Drug Diagnoses

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

Male Age Cohort:	(1) Aged 15-24	(2) Aged 25-34	(3) Aged 35-44	(4) Aged 15-24	(5) Aged 25-34	(6) Aged 35-44
Post-Policy x Lambeth	.0380*	.0749**	-.0339	.0593	.137**	.211**
	(.0229)	(.0334)	(.0626)	(.0411)	(.0617)	(.103)
Policy Period x Lambeth	.0282	-.0288	-.156	.0364	(.0131	(.0549
	(.0396)	(.0606)	(.104)	(.0397)	(.0595)	(.0964)
Post-Policy	.0289***	-.00715	.000513	.00576	.00459	.0465**
	(.00609)	(.00707)	(.00766)	(.0133)	(.0176)	(.0189)
Policy-Period	.00986	-.0227***	-.0123	.000577	(.00794	.0219*
	(.00765)	(.00775)	(.00892)	(.00900)	(.0118)	(.0127)
Mean of Dependent Variable, Lambeth Pre-Policy	.037	.179	.362	.037	.179	.362
Borough and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Linear Borough Specific Time Trend	No	No	No	Yes	Yes	Yes
Adjusted R-squared	.256	.395	.435	.320	.424	.504
Observations (borough-quarter-year)	1428	1428	1428	1428	1428	1428

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. All observations are at the borough-quarter-year level, and are weighted by the population of the borough relative to the population of London. The sample period runs from Q2 1997 until Q4 2009. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. Column 1 relates to admissions of those aged 15-24 on 1st July 2001. Control boroughs are all other London boroughs, excluding Lambeth's neighbors (Croydon, Merton, Southwark and Wandsworth). Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighboring boroughs) London population that year in the borough. Columns 1 and 4 relate to admissions of those aged 15-24 on 1st July 2001.Columns 2 and 5 relate to admissions of those aged 25-34 on 1st July 2001. Columns 3 and 6 relate to admissions of those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These admission rates are derived from the HES administrative records at the borough-quarter-year level. Columns 4 to 6 additionally control for a linear borough specific time trend.

Table 5: The Impact of the LCWS by Male Age Cohort and Admission History

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

Male Age Cohort:	Aged 15-24	Aged 25-34	Aged 35-44	Aged 15-24	Aged 25-34	Aged 35-44
Pre-Policy Drugs or Alcohol Admissions:	No	No	No	Yes	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Policy x Lambeth	.0157	.0873	.160**	0.00390	-0.00206	-0.00812***
	(.0291)	(.0646)	(.0776)	(0.00414)	(0.00147)	(0.00130)
Policy Period x Lambeth				0.00179	-0.00115	-0.00567**
				(0.00725)	(0.00287)	(0.00238)
Post-Policy	.0140*	.0156**	.0148**	-0.00563***	-0.00985***	-0.00479***
	(.00719)	(.00745)	(.00685)	(0.000519)	(0.000567)	(0.000421)
Policy-Period				-0.00669***	-0.00929***	-0.00479***
				(0.000933)	(0.000938)	(0.000682)
Mean of Dependent Variable, Lambeth Pre-Policy	0	0	0	.00727	.0150	.0152
Borough and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	.327	.413	.381	.160	.364	.353
Observations (borough-quarter-year)	952	952	952	1428	1428	1428

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable in Columns 1-3 is the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. The dependent variable in Columns 4-6 is the number of admissions for Class A drugs amongst men who were admitted for drugs or alcohol diagnoses pre-policy, divided by the total number of men admitted for a drugs or alcohol related diagnoses in a given borough and quarter during the pre-policy period. All observations are at the borough-quarter-year level, and are weighted by the population of the borough relative to the population of London. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. The sample in Columns 1 to 3 then runs in the period after the LCWS is introduced, from Q3 2001 to Q4 2009. Columns 4 to 5 restrict hospital admission rates to be constructed from those individuals that have at least one such admissions in the pre-policy period. The sample in Columns 4 to 6 then runs from Q2 1997 until Q4 2009. Control boroughs are all other London boroughs, excluding Lambeth's neighbors (Croydon, Merton, Southwark and Wandsworth). Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighboring boroughs) London population that year in the borough. Columns 1 and 4 relates to admissions of those aged 15-24 on 1st July 2001.Columns 2 and 5 relate to admissions of those aged 25-34 on 1st July 2001. Columns 3 and 6 relate to admissions of those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These admission rates are derived from the HES administrative records at the borough-quarter-year level.

Table 6: The Impact of the LCWS on Hospital Admissions for Alcohol

Dependent Variable: Male Hospital Admission Rates for Alcohol Related Diagnoses

Admission rates for:	Primary Diagnosis: Alcohol Related (Class-A not recorded as secondary)			Primary Diagnosis: Alcohol Related Secondary Diagnosis: Class-A Drug Related		
Male Age Cohort:	**Aged 15-24**	**Aged 25-34**	**Aged 35-44**	**Aged 15-24**	**Aged 25-34**	**Aged 35-44**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Post-Policy x Lambeth	-.0455**	-.00875	.108	.0490	.0416	.255
	(.0220)	(.0459)	(.0702)	(.0879)	(.311)	(.522)
Policy Period x Lambeth	-.00316	-.0420	-.255**	.232	-.00288	.00861
	(.0365)	(.0750)	(.124)	(.156)	(.522)	(.863)
Post-Policy	.0119	.0407***	.0941***	.0373	.0848*	.136***
	(.00895)	(.0153)	(.0224)	(.0243)	(.0443)	(.0508)
Policy-Period	-.0123	-.000998	.0396	.0135	.0697	.0421
	(.0115)	(.0183)	(.0252)	(.0284)	(.0507)	(.0538)
Mean of Dependent Variable, Lambeth Pre-Policy	.0679	.152	.590	.00316	.00867	.0331
Borough and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	.398	.591	.664	.099	.122	.130
Observations (borough-quarter-year)	1428	1428	1428	1428	1428	1428

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable the number of alcohol related hospital admissions per 1000 of the population in the cohort. In Columns 1-3, the primary diagnosis refers to alcohol, and Class-A drug diagnoses do not appear in the secondary diagnoses. In Columns 4-6, primary diagnoses refers to alcohol and secondary diagnoses that refer to Class-A drug use are included. Class-A drugs include cocaine, opioids, and hallucinogens. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. All observations are at the borough-quarter-year level, and are weighted by the population of the borough relative to the population of London. The sample period runs from Q2 1997 until Q4 2009. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. Column 1 relates to admissions of those aged 15-24 on 1st July 2001. Control boroughs are all other London boroughs, excluding Lambeth's neighbors (Croydon, Merton, Southwark and Wandsworth). Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighboring boroughs) London population that year in the borough. Columns 1 and 4 relate to admissions of those aged 15-24 on 1st July 2001.Columns 2 and 5 relate to admissions of those aged 25-34 on 1st July 2001. Columns 3 and 6 relate to admissions of those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These admission rates are derived from the HES administrative records at the borough-quarter-year level.

Table 7: Impacts on Length of Hospital Stay for Class-A Drug Related Diagnoses

Dependent Variable: Length of Hospital Stay in Days for Males Admitted with:

Male Age Cohort:	(1) Aged 15-24	(2) Aged 25-34	(3) Aged 35-44
Post-Policy x Lambeth	3.72***	3.49***	2.38***
	(1.34)	(.820)	(.570)
Policy Period x Lambeth	.077	4.80***	-6.24***
	(1.28)	(1.09)	(1.53)
Post-Policy	-2.74*	-2.43***	-1.97***
	(1.34)	(.798)	(.615)
Policy-Period	-.030	-.297	1.98
	(1.39)	(1.10)	(1.54)
Mean of Dependent Variable, Lambeth Pre-Policy	7.46	15.14	10.58
Borough and Quarter Fixed Effects	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
Adjusted R-Squared	.083	.098	.071
Observation (individual hospital episode)	1374	2810	1806

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the number of days spent in hospital (discharge date - admission date) amongst those admitted to hospital for Class A drugs. Observations are at the episode or admission level. The sample includes only the first episode of a hospital stay (93% of all episodes) and episodes lasting less than 100 days (excluding 160 or 2% of episodes across all years and cohorts). Standard errors are clustered at the borough level. The sample period runs from January 1997 to December 2009. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. All columns include borough and quarter fixed effects, and control for borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level.

Table A1: Class-A Drug Related Hospital Admission Rates for Female Cohorts, by Borough and Time Period

Means, standard deviations in parentheses, standard errors in square brackets

	Lambeth		Rest of London		Post-Policy Minus Pre-Policy	
	(1) Pre-Policy	(2) Post-Policy	(3) Pre-Policy	(4) Post-Policy	(5) Unconditional	(6) Fixed Effects
Women aged 15-24	.060	.098	.016	.033	.021	.021
	(.067)	(.079)	(.047)	(.066)	[.024]	[.024]
Women aged 25-34	.159	.149	.037	.038	-.010	-.009
	(.090)	(.071)	(.059)	(.057)	[.023]	[.023]
Women aged 35-44	.116	.116	.023	.021	-.003	-.003
	(.071)	(.082)	(.044)	(.041)	[.028]	[.028]
Observations (borough-quarter-year)	17	30	459	810	-	-

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the number of female Class-A drug related hospital admissions per 1000 of the female population in the cohort, where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Female age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. All observations are at the borough-quarter-year level, and are weighted by the population of the borough relative to the population of London. In Columns 1 and 3 the pre-policy period runs from Q1 1997 to Q2 2001. The policy period runs from Q3 2001 to Q2 2002. In Columns 2 and 4 the post-policy period runs from Q3 2001 to Q4 2009. In Columns 3 and 4 the sample is based on all London boroughs excluding Lambeth and boroughs neighboring Lambeth. In Columns 5 and 6, standard errors on differences are calculated assuming a Prais-Winsten borough specific AR(1) error structure, that allows for borough specific heteroskedasticity and error terms to be contemporaneously correlated across boroughs. In Column 6 the differences are calculated from a regression specification that also controls for borough and quarter fixed effects.

Table A2: Cannabis Related Hospital Admissions for Male Cohorts, by Borough and Time Period

Means, standard deviations in parentheses, standard errors in square brackets

	Lambeth		Rest of London		Post-Policy Minus Pre-Policy	
	(1) Pre-Policy	(2) Post-Policy	(3) Pre-Policy	(4) Post-Policy	(5) Unconditional	(6) Fixed Effects
Men aged 15-24	.055	.108	.025	.083	0.001	0.001
	(.075)	(.072)	(.055)	(.094)	[0.018]	[0.018]
Men aged 25-34	.083	.088	.025	.047	-0.018	-0.017
	(.087)	(.057)	(.044)	(.060)	[0.027]	[0.027]
Men aged 35-44	.125	.100	.018	.039	-0.046	-0.044
	(.136)	(.078)	(.044)	(.064)	[0.041]	[0.040]
Observations (borough-quarter-year)	17	30	459	810	-	-

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the number of male cannabis related hospital admissions per 1000 of the male population in the cohort, where either the primary or secondary diagnosis refers to cannabis. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. All observations are at the borough-quarter-year level, and are weighted by the population of the borough relative to the population of London. In Columns 1 and 3 the pre-policy period runs from Q1 1997 to Q2 2001. The policy period runs from Q3 2001 to Q2 2002. In Columns 2 and 4 the post-policy period runs from Q3 2001 to Q4 2009. In Columns 3 and 4 the sample is based on all London boroughs excluding Lambeth and boroughs neighboring Lambeth. In Columns 5 and 6, standard errors on differences are calculated assuming a Prais-Winsten borough specific AR(1) error structure, that allows for borough specific heteroskedasticity and error terms to be contemporaneously correlated across boroughs. In Column 6 the differences are calculated from a regression specification that also controls for borough and quarter fixed effects.

Table A3: Standard Errors

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

Standard Errors:	Baseline: AR(1)			Clustered By Borough		
				Wild Cluster Bootstrap p-values in Brackets		
Male Age Cohort:	**(1) Aged 15-24**	**(2) Aged 25-34**	**(3) Aged 35-44**	**(4) Aged 15-24**	**(5) Aged 25-34**	**(6) Aged 35-44**
Post-Policy x Lambeth	.0380*	.0749**	-.0339	.0374***	.0748***	-.0336***
	(.0229)	(.0334)	(.0626)	(.00934)	(.0115)	(.00745)
				[.000]	[.000]	[.002]
Policy Period x Lambeth	.0282	-0.0288	-.156	.0275***	-0.0323	-.145***
	(.0396)	(.0606)	(.104)	(.00681)	(.0144)	(.0103)
				[.995]	[.995]	[.002]
Post-Policy	.0289***	-.00715	.000513	.0328***	-.00487	-.00309
	(.00609)	(.00707)	(.00766)	(.00839)	(.00934)	(.0120)
				[.000]	[.547]	[.167]
Policy-Period	.00986	-.0227***	-.0123	.0121*	-.0189	-.0163
	(.00765)	(.00775)	(.00892)	(.00634)	(.0128)	(.0115)
				[.058]	[.146]	[.737]
Borough and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Linear Borough Specific Time Trend	No	No	No	Yes	Yes	Yes
Adjusted R-squared	.256	.395	.435	.219	.353	.471
Observations (borough-quarter-year)	1428	1428	1428	1428	1428	1428

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. All observations are at the borough-quarter-year level, and are weighted by the population of the borough relative to the population of London. The sample period runs from Q2 1997 until Q4 2009. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. Column 1 relates to admissions of those aged 15-24 on 1st July 2001. Control boroughs are all other London boroughs, excluding Lambeth's neighbors (Croydon, Merton, Southwark and Wandsworth). In Columns 1 to 3, 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 Columns 4 to 6, standard errors are clustered by borough. In Columns 4 to 6 we also report the cluster wild bootstrap p-values following the procedure of Cameron *et al.* [2008]. Observations are weighted by the share of the total (excluding neighboring boroughs) London population that year in the borough. Columns 1 and 4 relate to admissions of those aged 15-24 on 1st July 2001.Columns 2 and 5 relate to admissions of those aged 25-34 on 1st July 2001. Columns 3 and 6 relate to admissions of those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These admission rates are derived from the HES administrative records at the borough-quarter-year level.

Table A4: Tobit Specifications

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

Robust standard errors in parentheses

Margin:	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Male Age Cohort:	Aged 15-24		Aged 25-34		Aged 35-44	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Policy x Lambeth	.150	.0434***	.0133	.0712***	.0385*	-.0441
	(.109)	(.0147)	(.0252)	(.0241)	(.0220)	(.0308)
Borough and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations (borough-quarter-year)	1428	1428	1428	1428	1428	1428

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. All observations are at the borough-quarter-year level, and are weighted by the population of the borough relative to the population of London. The sample period runs from Q2 1997 until Q4 2009. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. Column 1 relates to admissions of those aged 15-24 on 1st July 2001. Control boroughs are all other London boroughs, excluding Lambeth's neighbors (Croydon, Merton, Southwark and Wandsworth). Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighboring boroughs) London population that year in the borough. Columns 1 and 4 relate to admissions of those aged 15-24 on 1st July 2001.Columns 2 and 5 relate to admissions of those aged 25-34 on 1st July 2001. Columns 3 and 6 relate to admissions of those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These admission rates are derived from the HES administrative records at the borough-quarter-year level. The estimates on the interaction terms, Post-Policy x Lambeth coefficients are produced by taking the double difference of the conditional expected values for the four Lambeth (0 and 1) x Post Policy (0 and 1) cells.

Table A5: Pre-Policy Common Trends Check

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

Male Age Cohort:	(1) Aged 15-24	(2) Aged 25-34	(3) Aged 35-44
2nd Half Pre-Policy x Lambeth	.0216	-.00403	-.00509
	(.0278)	(.0356)	(.0468)
2nd Half Pre-Policy	.0122**	.0103	.00206
	(.00494)	(.00838)	(.0123)
Borough and Quarter Fixed Effects	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
Adjusted R-squared	.187	.393	.680
Observations (borough-quarter-year)	476	476	476

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. All observations are at the borough-quarter-year level, and are weighted by the population of the borough relative to the population of London. The sample period runs from Q2 1997 until Q2 2001, the eve of the LCWS policy. The "2nd half pre-poliy" dummy indicator is equal to one in the second half this sample period, and zero otherwise. Control boroughs are all other London boroughs, excluding Lambeth's neighbors (Croydon, Merton, Southwark and Wandsworth). Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighboring boroughs) London population that year in the borough. Column 1 relates to admissions of those aged 15-24 on 1st July 2001.Column 2 relates to admissions of those aged 25-34 on 1st July 2001. Column 3 relates to admissions of those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These admission rates are derived from the HES administrative records at the borough-quarter-year level.

Table A6: Varying the Sample of Comparison Boroughs

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

Comparison Boroughs:	**High Drug Admission Rates, Pre-Policy**			**Very Drug Admission Rates, Pre-Policy**			**Including Neighboring Boroughs**			**Boroughs with Teaching Hospitals**			**Boroughs with Mental Health Trusts**		
Male Age Cohort:	**15-24**	**25-34**	**35-44**	**15-24**	**25-34**	**35-44**	**15-24**	**25-34**	**35-44**	**15-24**	**25-34**	**35-44**	**15-24**	**25-34**	**35-44**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**
Post-Policy x Lambeth	.0279	.104***	-.0181	.0445	.134***	-.0371	0.0394*	0.0775**	-0.0361	.0573**	.113***	-.0245	.0494*	.0909***	-.0611
	(.0250)	(.0301)	(.0578)	(.0279)	(.0372)	(.0589)	(0.0232)	(0.0325)	(0.0622)	(.0245)	(.0319)	(.0603)	(.0260)	(.0348)	(.0621)
Policy-Period x Lambeth	.0458	.0360	-.111	.0757	.0732	-.106	0.0315	-0.0174	-0.151	.0408	.0183	-.141	.0285	-.0263	-.153
	(.0435)	(.0530)	(.0988)	(.0462)	(.0549)	(.0987)	(0.0403)	(0.0593)	(0.104)	(.0421)	(.0568)	(.100)	(.0413)	(.0600)	(.0999)
Post-Policy	.0352***	-.00874	.0242	.0268*	.00368	.0932*	0.0250***	-0.00617	0.00370	.00174	-.00666	.0468**	.0147	-.00297	.0238
	(.0103)	(.0163)	(.0208)	(.0151)	(.0379)	(.0477)	(0.00574)	(0.00762)	(0.00776)	(.00863)	(.0149)	(.0223)	(.0108)	(.0142)	(.0174)
Policy-Period	-.00380	-.0839***	-.0520**	-.0263	-.129***	-.0400	0.00500	-0.0308***	-0.0168*	-.0150	-.0562***	-.0101	.00469	-.0234*	-.0124
	(.0132)	(.0207)	(.0240)	(.0184)	(.0453)	(.0511)	(0.00735)	(0.00882)	(0.00934)	(.00999)	(.0160)	(.0228)	(.0125)	(.0140)	(.0162)
R-squared	.321	.353	.485	.237	.593	.382	0.261	0.417	0.479	.242	.451	.533	.196	.414	.546
Observations (borough-quarter-year)	510	510	510	204	204	204	1,632	1,632	1,632	408	408	408	306	306	306
Number of Boroughs (incl. Lambeth)	10	10	10	4	4	4	32	32	32	8	8	8	6	6	6

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. All observations are at the borough-quarter-year level, and are weighted by the population of the borough relative to the total population of the sample boroughs. The sample period runs from Q2 1997 until Q4 2009. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. In Columns 1-3, control boroughs are all other boroughs with high drug admissions rates in the pre-policy period (defined as having a pre-policy admission rate of at least .08 across cohorts aged 15 and 44). These nine boroughs are Bexley, Bromley, Camden, Croydon, Greenwich, Kensington and Chelsea, Lewisham, Southwark, Westminster. In Columns 4-6, control boroughs are all other boroughs with very high drug admissions rates in the pre-policy period, defined as all those boroughs that pre-policy admission rate of at least '.16 across cohorts aged 15 and 44). These three boroughs are Greenwich, Lewisham and Southwark. In Columns 10-12 the control boroughs are those with a teaching hospital in them. In Columns 13-15 the control boroughs are those with a mental health trust headquartered in them. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighboring boroughs) London population that year in the borough. Columns 1, 4, 7, 10 and 15 relate to admissions of those aged 15-24 on 1st July 2001.Columns 2, 5, 8, 11 and 14 relate to admissions of those aged 25-34 on 1st July 2001. Columns 3, 6, 9, 12 and 15 relate to admissions of those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These admission rates are derived from the HES administrative records at the borough-quarter-year level.

Table A7: Nationwide Depenalization Policy

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

Sample:	**1997 Q2-2003 Q4**		
Male Age Cohort:	**Aged 15-24**	**Aged 25-34**	**Aged 35-44**
	(1)	**(2)**	**(3)**
Post-Policy x Lambeth	.0362	.0753**	.134*
	(.0281)	(.0378)	(.0799)
Policy-Period x Lambeth	.0222	-.0550	-.147
	(.0320)	(.0445)	(.0924)
Post-Policy	.00152	-.0164*	-.00546
	(.00556)	(.00938)	(.0139)
Policy-Period	-.000276	-.0242***	-.0107
	(.00531)	(.00841)	(.0125)
Mean of Dependent Variable, Lambeth Pre-Policy	.037	.179	.362
Borough and Quarter Fixed Effects	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
Adjusted R-squared	.191	.416	.581
Observations (borough-quarter-year)	756	756	756

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. All observations are at the borough-quarter-year level. In columns 1-3, the sample period runs from Q2 1997 until Q4 2003, and the control boroughs are all other London boroughs, excluding Lambeth's neighbors (Croydon, Merton, Southwark and Wandsworth). In columns 4-6, the sample runs from Q2 1997 until Q4 2009 and exclude both Lambeth and her neighbors. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighboring boroughs) London population that year in the borough. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. The National-Policy is a dummy equal to 1 after Q1 2004 and zero otherwise. Column 1 4 relates to admissions of those aged 15-24 on 1st July 2001. Column 2 relates to admissions of those aged 25-34 on 1st July 2001. Column 3 relates to admissions of those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These admission rates are derived from the HES administrative records at the borough-quarter-year level.

Table A8: Time Trends and The Impact of the LCWS by Male Age Cohort and Admission History

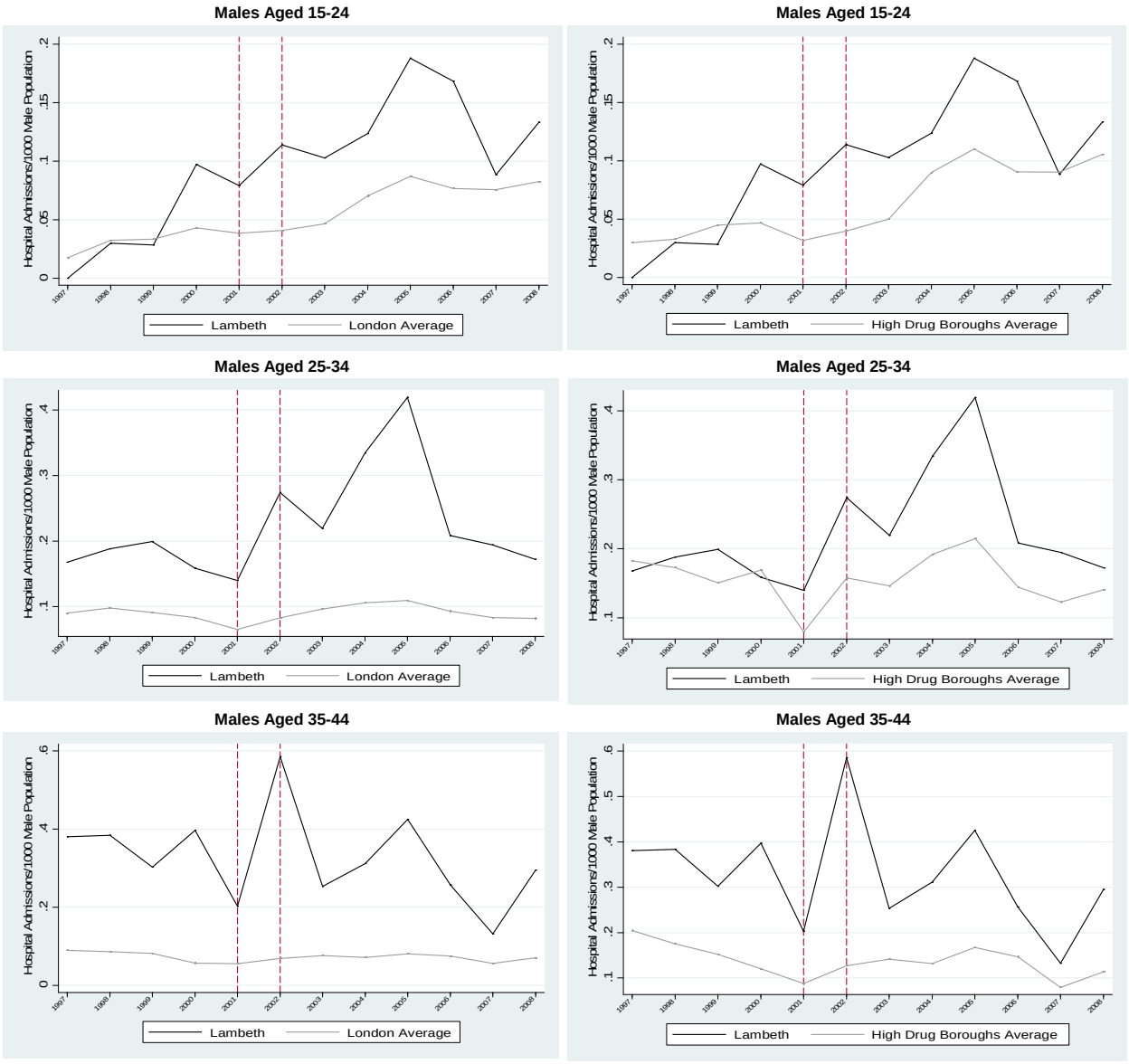
Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

	Aged 15-24	Aged 25-34	Aged 35-44	Aged 15-24	Aged 25-34	Aged 35-44
Pre-Policy Drugs or Alcohol Admissions:	**No**	**No**	**No**	**Yes**	**Yes**	**Yes**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Post-Policy x Lambeth	.0442	.121	.260***	.000168	.00215	.00447**
	(.0342)	(.0753)	(.0779)	(.00775)	(.00279)	(.00227)
Policy-Period x Lambeth				.000387	.00101	.00455**
				(.00746)	(.00285)	(.00221)
Post-Policy	-.00411	.0122	.0159**	.00942***	.0111***	.00329***
	(.00688)	(.00797)	(.00749)	(.00109)	(.00106)	(.000769)
Policy-Period				.00863***	.01000***	.00411***
				(.000972)	(.000954)	(.000699)
Borough and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Borough Specific Linear Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	.171	.332	.394	.188	.418	.431
Observations (borough-quarter-year)	952	952	952	1428	1428	1428

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable in Columns 1-3 is the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001. The dependent variable in Columns 4-6 is the number of admissions for Class A drugs amongst men who were admitted for drugs or alcohol diagnoses pre-policy, divided by the total number of men admitted for a drugs or alcohol related diagnoses in a given borough and quarter during the pre-policy period. All observations are at the borough-quarter-year level, and are weighted by the population of the borough relative to the population of London. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. Columns 1 to 3 restrict hospital admission rates to be constructed from those individuals that have no such admissions in the pre-policy period. The sample in Columns 1 to 3 then runs in the period after the LCWS is introduced, from Q3 2001 to Q4 2009. Columns 4 to 5 restrict hospital admission rates to be constructed from those individuals that have at least one such admissions in the pre-policy period. The sample in Columns 4 to 6 then runs from Q2 1997 until Q4 2009. Control boroughs are all other London boroughs, excluding Lambeth's neighbors (Croydon, Merton, Southwark and Wandsworth). Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighboring boroughs) London population that year in the borough. Columns 1 and 4 relates to admissions of those aged 15-24 on 1st July 2001.Columns 2 and 5 relate to admissions of those aged 25-34 on 1st July 2001. Columns 3 and 6 relate to admissions of those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These admission rates are derived from the HES administrative records at the borough-quarter-year level. All specifications control for a linear borough specific time trend.

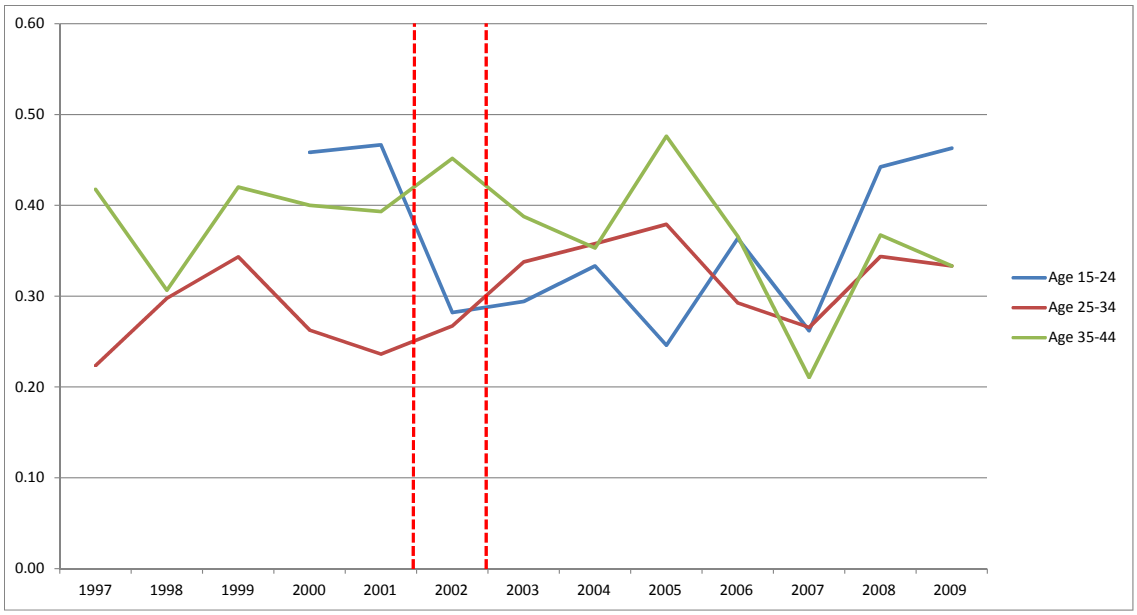
Figure 1: Time Series for Class-A Drug Related Hospital Admission Rates for Male Cohorts

A. Lambeth versus All Other London Boroughs | **B. Lambeth Versus All Other High Drug Admission Boroughs**



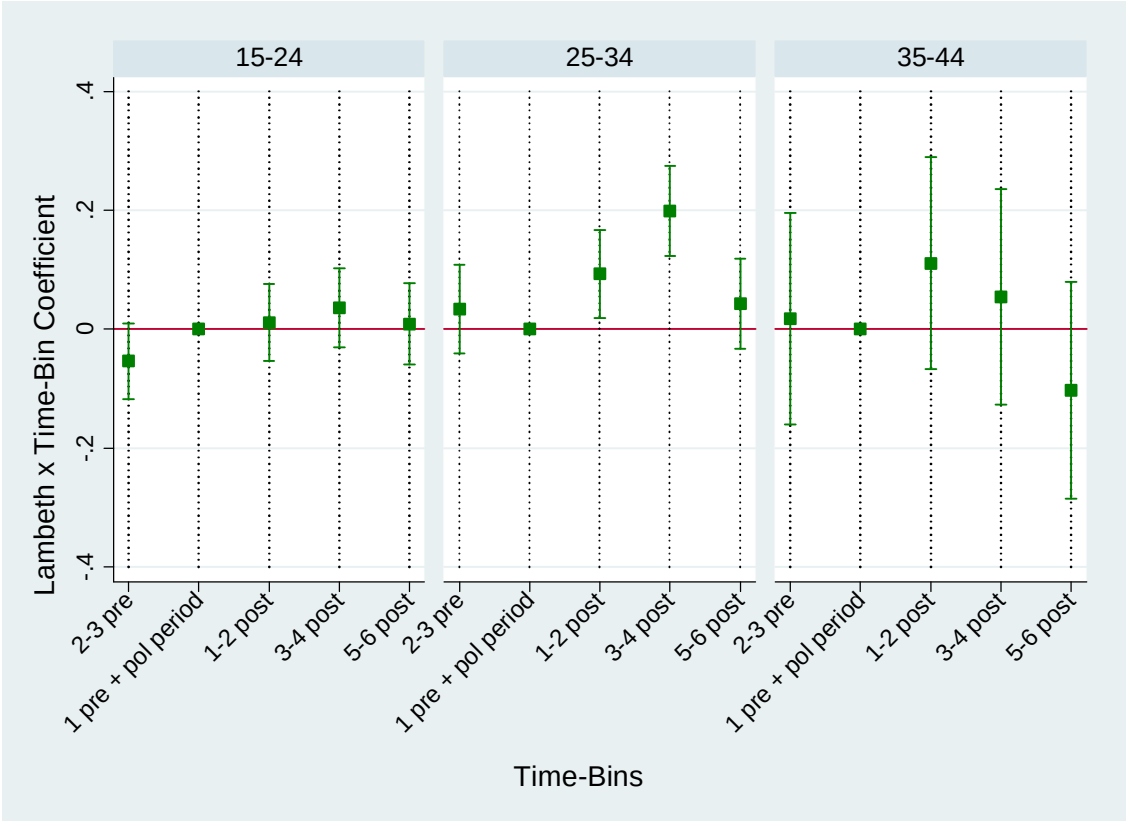
Notes: Figure 1A plots the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Year denotes year beginning in quarter 3 (June) of that year, running though quarter 2 the following year. The solid black line gives admission rates for Lambeth. The solid grey line gives the averages across the remaining 31 London Boroughs. Cohorts are defined by age on 1st July 2001. The dashed vertical lines in each figure signify the start and end of the period when the LCWS was officially in place. Figure 1B also plots the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. The solid black line gives admission rates for Lambeth, and the solid grey line gives the averages across the remaining boroughs with high drug admission pre-policy (an average of 0.08 per quarter or higher in the pre-policy period). There are nine such boroughs: Bexley, Bromley, Camden, Croydon, Greenwich, Kensington and Chelsea, Lewisham, Southwark, Westminster.

Figure 2: Percentage of All Individuals Treated For Class-A Related Diagnoses in Lambeth that are Resident in Lambeth



Notes: Figure 3 plots the share of all male individuals in the age cohort that are treated in a Lambeth hospital for Class-A drug related hospital admissions, that are residents of Lambeth. Class-A drugs include cocaine, opioids, and hallucinogens. Age cohorts are defined by age on 1st July 2001. The dashed vertical lines in each figure signify the start and end of the period when the LCWS was officially in place. The time series for the 15-24 years old cohort stops in 1999 because for earlier years there are fewer than 10 admissions in Lambeth per quarter.

Figure 3: Impact of the LCWS by Time-Bin Relative to the Reform



Notes: Each panel in Figure 3 refers to a separate specification, where the policy and post policy dummies are replaced by five 2-year time-bin dummies: 2-3 years pre reform (Q3 1998-Q2 2000); 1 year pre reform policy and the policy period (Q3 2000 - Q2 2002); 1-2 years post reform (Q3 2002 - Q2 2004); 3-4 years post reform (Q3 2004 - Q2 2006); and 5-6 years post reform (Q3 2006 - Q2 2008). The omitted category is the 1 year pre and policy period category. Data pre Q3 1998 and post Q3 2008 onwards are excluded. Each plotted square corresponds to the Lambeth x Time-Bin dummy coefficient. The vertical green lines give the 95% confidence intervals. Age refers to age on the eve of the LCWS introduction (1st July 2001).