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

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Abstract

We evaluate the impact on hospital admissions related to illicit drug use, caused by a policing experiment that depenalized the possession of small quantities of cannabis in the London borough of Lambeth. We exploit administrative records covering all individual hospital admissions and with detailed ICD-10 diagnosis classifications. We use these records to construct a panel data set by London borough and quarter from 1997 to 2009 to estimate the short and long run impacts of the depenalization policy unilaterally introduced into 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, and proportionately larger in younger age cohorts and among those with prior histories of hospitalization related to drug or alcohol use. The magnitudes of the impacts are large, corresponding to between 33% and 64% of baseline admission rates across age cohorts. The dynamic impacts across cohorts vary in profile with some cohorts experiencing hospitalization rates remaining above pre-intervention levels six years after the depenalization of cannabis was first introduced. We find evidence of positive spillover effects in hospitalization rates related to hard drugs among those resident in boroughs neighboring Lambeth, and these are concentrated among cohorts without prior histories of hospitalizations related to the use of illicit drugs or alcohol. Finally, the severity of hospital admissions, as measured by the length of hospital stays, significantly increases for both admissions related to the use of hard drugs and cannabis. Taken together, our results suggest policing strategies related to the cannabis market have significant, nuanced and lasting impacts on public health.

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

JEL Classification: I18, K42, H75.

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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 [Office for National Drug Control Strategy, 2004], and for the UK, Gordon *et al.* [2006] estimated the total 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 [Office for National Drug Control Strategy, 2011]. 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. While such policies might help free up resources from the criminal justice and health systems, these more liberalized policies also carry their own risks including potentially encouragement activities that have deleterious consequences for user's health, the use of certain drugs providing a causal 'gateway' to more harmful and addictive substances [van Ours, 2003; Melberg *et al.*, 2010], and possible impacts onto other forms of anti-social behavior beyond criminal activity.

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. Under the policing experiment we study, the possession of small quantities of cannabis was temporarily depenalized, so that this was no longer a prosecutable offence. The experiment - known as the Lambeth Cannabis Warning Scheme (LCWS) - 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. We evaluate the short and long run consequences of this policy on healthcare usage as measured by detailed and comprehensive administrative records on drug-related admissions to hospital. Such hospital admissions represent 60% of total drug-related healthcare costs [Gordon *et al.*, 2006]. We use this administrative data to shed light on the broad question of whether policing strategies towards the market for cannabis impact upon public health, through changes in the usage of illicit drugs and subsequent health of drug users.[1]

[1] Donohue *et al.* [2011] categorize illicit drug policies into three type: (1) legalization - a system in which

Our primary data source is the Inpatient Hospital Episode Statistics (HES), an administrative source that records every admission to a public hospital in England.[2] 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 this impact varies across cohorts of the population.

The panel data we construct covers each of the 32 London boroughs between January 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 change in policing strategy related to cannabis.

The administrative records also 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 the usage of other illicit drugs, this has to be weighed against the issue 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.[3] The administrative records also contain information on hospital stays associated with each patient admission, and we use this to explore

possession and sale are lawful but subject to regulation and taxation; (ii) criminalization - a system of proscriptions on possession and sale backed by criminal punishment, potentially including incarceration; (iii) depenalization - a hybrid system, in which sale and possession are proscribed, but the prohibition on possession is backed only by such sanctions as fines or mandatory substance abuse treatment, not incarceration. 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].

[2]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, and we use only the inpatient data. The inpatient data set includes all those admitted to hospital (under the order of a doctor) who are expected to stay at least one night, and contains detailed 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 does not have information on diagnosis codes.

[3]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].

whether the depenalization policy thus impacts the severity of hospital admissions, both where the primary diagnosis relates to Class-A drugs and for cannabis related hospitalizations.

We present five 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. Second, exploring the heterogeneous impacts across male cohorts resident in Lambeth, we find the impacts to be proportionately larger among cohorts that were younger at the start of the policy, and proportionately larger among those with prior histories of hospitalization related to drug or alcohol use. The magnitudes of the impacts are large across age cohorts, corresponding to between 33% and 64% of average baseline admission rates. Third, the dynamic impacts across cohorts vary in profile with some cohorts experiencing hospitalization rates remaining above pre-intervention levels six years after the depenalization of cannabis was first introduced. Fourth, we find evidence of positive spillover effects on hospitalizations related to Class-A drug use among those resident in boroughs neighboring Lambeth, and these are concentrated among cohorts without prior histories of hospital admissions related to the use of illicit drugs or alcohol. Finally, the severity of hospital admissions, as measured by the length of stay in hospital, significantly increases for both admissions related to Class-A and cannabis diagnoses. Taken together, our results suggest policing strategies 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 related to 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]. Our analysis 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 of the links between 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].[4]

[4] 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-

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. 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 six years after the policy was initiated and despite the fact that the policy itself was only formally in place for one year.[5]

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 LCWS by gender, age cohort and previous admissions history, and associated robustness checks. Section 5 presents extended results related to dynamic effects, geographic spillovers, and the severity of admissions. Section 6 concludes by discussing the implications of our findings for drug policy, the literature linking cannabis and Class-A drug use, and the broader relationship between police behavior and public health.

2 The Lambeth Cannabis Warning Scheme (LCWS)

The Lambeth Cannabis Warning Scheme (LCWS) was unilaterally introduced by the borough police force in the London borough of Lambeth on 4th July 2001, as a pilot intended to last six months. Under the scheme, those found in possession of small quantities of cannabis for their personal use within Lambeth: (i) had their drugs confiscated; (ii) were given a warning rather than being arrested. The main objective behind the policy was to reduce the number of individuals being criminalized, and to free up police time and resources to deal with more serious crime, including those related to Class-A drugs [Dark and Fuller, 2002; Adda *et al.*, 2011].

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.

[5]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.

The underlying motivation for the policy, as well as the way in which it was implemented and the targeted outcomes, are very similar to the way in 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 might be informative of the links between police drugs policy and health in settings outside of the specific London context we study.[6]

Anecdotal evidence suggests local support for the scheme began to decline once the policy was announced to have been extended beyond the initial pilot. Media reports cited that local opposition arose due to concerns that children were at risk from the scheme, and that the LCWS had increased drug tourism into Lambeth. The LCWS formally ended on 31st July 2002. In part because of disagreements between the police and local politicians over the policy's true impact, post-policy Lambeth's cannabis policing strategy did not return identically to what it had been pre-policy. Rather, it adjusted to be a firmer version of what had occurred during the pilot so that police officers in Lambeth continued to issue warnings but would now also have the discretion to arrest where the offence was aggravated.[7] Hence when we refer to measuring the long run impacts of the depenalization policy, we are capturing 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 longer term differences in policing towards cannabis from the post and pre-policy periods.

The impact of the LCWS depenalization policy on patterns of *crime* in Lambeth and neighboring boroughs is studied extensively by Adda *et al.* [2011]. For the purposes of the current study on health outcomes, there are three key results on the impact of the depenalization policy on crime to bear in mind. These results relate to its impact on the market for cannabis, on the market for Class-A drugs, and drugs tourism. First, the LCWS led to a significant and permanent rise in cannabis related criminal offences in Lambeth. Using data on finely disaggregated offence type reveals that both the demand for and supply of cannabis are likely to have significantly risen in Lambeth after the introduction of the depenalization policy. This result is important for the current study because it suggests the equilibrium market size for cannabis in Lambeth likely increased post-policy, and that the depenalization policy did cause an abrupt, quantitatively large and permanent shock to the cannabis market in Lambeth. This will consequently affect the market size for Class-A drugs in all but the knife-edge case of the two markets being independent.[8]

[6] 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].

[7] 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.

[8] Cannabis possession offences increased by 13.5% during the policy, and 24.2% in the post policy period (August

Second, the depenalization policy had impacts beyond the market for cannabis: the longer term effect of the LCWS was to lead to a significant increase in offences related to the possession of Class-A drugs. However, there is little evidence that the police reallocated their efforts towards crimes relating to Class-A drugs; rather the police appear to have reallocated effort towards non-drug crime. In this paper we estimate the relationship between the policing of cannabis and Class-A drug hospital admissions. It is therefore important that changes the reallocation of police resources to other crimes did not counteract any mechanism linking cannabis and Class-A drug consumption. In our analysis when we consider the long term impact of the depenalization policy we take as given the results established in Adda *et al.* [2011] that in response to the policy, the police reallocated effort away from cannabis related crime and towards non-drugs crime; the police did not reallocate effort towards Class-A drug related crime. Given the addictive nature of drug-consumption, and potential lags in seeking out and receiving treatment, we might also reasonably expect any impact of the LCWS on hospital admissions to last well into the post-policy period. We later consider how the effects of the LCWS on drug-related hospital admissions evolve over time across various cohorts.

The third key finding from Adda *et al.* [2011] on the impact of the LCWS on crime is the existence of drug tourism from geographically neighboring boroughs into Lambeth. Indeed, these flows into Lambeth explain almost half the increase in cannabis offences in Lambeth. To explore this further in terms of health outcomes, we later investigate whether there are similar geographic spillovers in hospital admission rates related to Class-A admissions after the LCWS is introduced in Lambeth. Our administrative data on hospital admissions further allow us to shed light on the nature of drug tourists, by exploring how the marginal impact on those resident in neighboring boroughs, differs from the marginal impact on residents of Lambeth.

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 spell of care in a National Health Service hospital.[9] 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

2002 to January 2006) relative to the pre-policy period Adda *et al.* [2011].

[9] 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 episodes. This is similar to the way in which episodes are recorded in Model [1993] where multiple drug mentions in emergency room admissions correspond to different episodes. Given the infrequency with which the same patient transfers across consultants during a hospital stay, the majority of results presented are robust to re-defining episodes at the patient-consultant level.

less serious conditions receive treatment elsewhere, including outpatient appointments, 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 lower bound impact of the depenalization of cannabis 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.[10]

We assess how hospital admissions related to Class-A drugs or to cannabis are impacted by the depenalization of cannabis possession in Lambeth. For Class-A drug related admissions, we include episodes where the drug is mentioned either in the primary diagnosis, or those episodes directly caused by 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.[11] Given that our main outcome relates to rates of hospital inpatient admissions, we aggregate the individual patient-episode level data by borough of residence and quarter, and calculate admission rates for diagnosis d and borough of residence b as follows:

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

where Tot_{dbqy} are total number of 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. These admission rates are calculated by gender and age cohort, where age is categorized into ten year bins (10-19, 20-29, 30-39) 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 32 London boroughs by quarter, running from January 1997 to December 2009. To reiterate, the geographic information we use relates to the patient's borough of *residence*, not the borough in which they are hospitalized. This helps ameliorate concerns that the results are driven by changes in the location of hospitals, or changes in drug-related services provided by hospitals.[12]

[10]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 accident and emergency (emergency room) departments, 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.

[11]Diagnoses that mention Class-A drugs include (drug specific) mental and behavioral disorders (ICD-10 Codes F11 for opiods, F14 for cocaine, F16 for hallucinogens), intentional and accidental poisoning (T400-T406 T408-T409, X42, X62 Y12), and the finding of the drug in the blood (R781-R785). Diagnoses that mention cannabis include mental and behavioral disorders (F12), and poisoning (T407).

[12]Annual Office for National Statistics (ONS) population estimates at the borough level are only provided in five-year bands [Office for National Statistics, 2011]. 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 20-29 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× total aged 20-24) + total aged 25-29 + (0.2 × total

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). For those with pre-policy admission rates, Pop_{by} is replaced by the number of distinct individuals admitted for diagnoses related to illicit drugs or alcohol whilst residing in borough b between January 1997 and June 2001.

It is instructive to briefly 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). Two points are of note. First, 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 rise in cannabis related hospital admissions even if there is a substantial increase in cannabis usage caused by the policy.

Second, the HES records show the age profile of drug-related hospital admissions is Λ-shaped, rising sharply up to the mid thirties, and falling thereafter for older age groups. This compares to the BCS survey data that suggests illicit drug use declines after individuals reach their mid twenties. There are at least two explanations for this slight divergence by age between drug use and hospitalization: the aging process might increase the probability of an adverse reaction to illicit drugs after the age of 25. Alternatively, drug use might start in the early twenties but take several years to result in hospitalization, perhaps as patterns of drug use change with age. These two factors will need to be borne in mind when interpreting some of the later findings for the oldest cohort we study, who were aged 30-39 at the time the depenalization policy was introduced.

3.2 Linking Admissions for Cannabis and Class-A Drugs

Our primary interest is to understand how changes in police enforcement towards the cannabis market - as embodied in the LCWS policy - impacts public health through changes in hospitalization rates related to illicit drug use. Of course the policy would most directly affect the consumption of cannabis, although changes in hospital admissions related to cannabis usage are statistically hard to detect given the rarity of such events. However, a body of evidence suggests cannabis users are more likely to consume Class-A drugs, both contemporaneously and in the future [van Ours, 2003;

aged 30-34). Results are robust to fixing the population at 2001 levels.

Melberg *et al.*, 2010; Bretteville-Jensen *et al.*, 2008; Colea *et al.*, 2004].

As a preliminary step, we present descriptive evidence from the HES to suggest how cannabis consumption today might correlate to Class-A drug use in the future, be it either due to a true intertemporal causal impact or because of state dependence [Deza, 2011]. To do so we exploit the identifiers in the administrative records allowing us to track individuals 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 with prior admissions are considered: (i) "cannabis admits", who were admitted for cannabis, the drug affected by the LCWS; (ii) "Class-A admits", who were admitted for the use of a harder drug; (iii) "alcohol admits", who were admitted for alcohol related diagnoses; (iv) "all other admits", who were admitted for any other cause and serve as a benchmark for the persistence of ill-health over these time periods.[13] Table 1 shows the mean and standard deviation for each probability of readmission, conditional on prior admissions. Two points are of note.

First, reading across the first 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. There is substantial persistence in hospital admissions for the same risky behavior, as shown on the leading diagonal in Columns 2-4. Persistence is particularly high for Class-A drugs and alcohol, where 26 and 23% respectively, were readmitted for the ill-effects of the same risky behavior.

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 consumption and hospital admissions for Class-A related drugs. Of course we make no claims about whether this picks up true intertemporal causal impact or because of state dependence, but merely highlight that the correlation between cannabis hospitalizations and later hospitalizations related to the use of Class-A drugs is significantly higher than the correlation between alcohol admissions and later admissions for Class-A drug use.

There are of course multiple explanations for the positive correlation between admissions for

[13] As already noted, cannabis related admissions are rare and so 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 sample accounts for approximately 30% of England's population. We exclude Lambeth from this analysis to prevent any impact of the LCWS contaminating these 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.

cannabis and subsequent risky behaviors. One explanation is state dependence so that the cannabis admit group have a particular set of characteristics that also lead to the subsequent misuse of Class-A drugs and alcohol, 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, as has been suggested by earlier studies [Beenstock and Rahav, 2002; Bretteville-Jensen *et al.*, 2008; Melberg *et al.*, 2010; van Ours, 2003]. 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 causal impact of 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 identifying.

3.3 Empirical Method

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

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

where $Admit_{dbqy}$ is the log of the number of admissions to hospital where the primary diagnosis relates to Class-A drugs, per thousand of the population, plus one. P_{qy} and PP_{qy} are dummies for the policy and post-policy periods respectively and L_b is a dummy for the borough of Lambeth. The parameters of interest are estimated using a standard difference-in-difference (DD) research design: β_1 and β_3 capture differential changes in hospital admission rates in Lambeth during and after the depenalization policy period, relative to other London boroughs. β_0 and β_2 capture London-wide trends in admissions during and post policy.

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 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), quarter (λ_q) and year (λ_y). Observations are weighed by borough shares of the London-wide population. Defining t as quarters since January 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.[14]

Before estimating (2) in full, Table 2 provides descriptive evidence on the long term effects of the depenalization policy on Class-A related hospital admissions, with each row showing admissions rates for specific gender and age cohorts. Columns 1 and 2 present means and standard deviations of hospital admission rates related to Class-A drug usage (without the log transformation) in Lambeth during the pre-policy and post-policy periods respectively; Columns 3-4 give the corresponding statistics for the average borough in the rest of London. We note that in the pre-policy period 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 formally shown 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.

On the potential health impacts of the depenalization policy, we note that admission rates rise over time for all male age cohorts in Lambeth. A similar pattern is observed for the average London borough although increases outside of Lambeth are less pronounced over time.Columns 5-6 that present difference-in-difference estimates of how Class-A drug admissions rates relate to the LCWS policy. Column 5 shows that unconditional on all other factors, the male admission rate in Lambeth increased by .050 per thousand population, relative to the London borough average, after the introduction of the LCWS policy, which is statistically significant the 10% level. The effect for women is not significantly different from zero. On the one hand the different trends across genders suggest the findings are not spuriously driven by changes in how hospital admissions might be recorded in Lambeth vis-à-vis other parts of London. On the other hand, we are able to say little on why only men are impacted by the depenalization policy.

Focusing on the bottom three rows showing Class-A drug admissions rates by 10-year age cohort, where age is defined on the eve of the policy, we note first that admission rates are particularly low for the youngest cohort in the pre-policy period, replicating survey evidence that the use of hard drugs is rare before age 18 [Pudney, 2003; Condon and Smith, 2003]. These low admissions rates for some cohorts emphasize that there are borough-quarter-year observations in which zero inpatient admissions occur for Class-A drug related causes. Hence in the appendix we present Tobit regression estimates to investigate further how the depenalization policy differentially impacts the extensive and intensive margins of hospitalization rates in Lambeth relative to the rest of London.

Focusing on the unconditional policy impacts by age cohort in Column 5, we see that there are significant rises post-policy for each age cohort in Lambeth relative to the rest of London in each time period. The absolute magnitude of the impacts rise with the age of the cohort,

[14] 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.

from a 3.5% impact on the youngest age cohort to a significant impact of 10.6% on the oldest age cohort. However when benchmarking the impacts against pre-policy levels of admissions rates (that reflect higher admission rates among older age cohorts) we see that in proportionate terms, these unconditional estimated policy impacts correspond to 91% of the pre-policy average admission rates for the 10-19 cohort, 57% of the pre-policy average admission rates for the 20-29 cohort, and 38% of pre-policy admission rates, for the 30-39 cohort. Hence, relative to baseline, the policy impacts are proportionately larger for younger age cohorts.

Column 6 then shows this basic pattern of difference-in-differences to remain in magnitude and significance once borough, quarter and year fixed effects are controlled for. Of course these patterns across age cohorts confound the impacts on individuals with different histories of drug use and hospital admissions. For example the documented larger impacts on older age cohorts might well reflect the fact that such individuals have been using illicit drugs for a longer and so are more exposed to changes in the market for cannabis and Class-A drugs caused by changes in policing towards the cannabis market. In the analysis below we will therefore also estimate the policy impacts on different cohorts of the male population where cohorts are defined by age and pre-policy admissions histories.

Finally, the corresponding descriptive evidence for hospital admissions related to cannabis use are given in Table A1. As already discussed, cannabis hospital admission rates are far lower than for Class-A drugs despite much higher levels of usage. The difference-in-difference results suggest the LCWS had no significant impact on hospital admissions for cannabis, for either gender or cohort: the point estimates for the youngest male cohorts are positive but not precisely estimated. 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. Our evidence from London suggests that if such a similar effect occurs from the depenalization of cannabis possession, it does not then feed through to significantly higher rates of hospitalization that involve an overnight stay, which is what our administrative data measures. For the remaining analysis we therefore continue to focus on Class-A hospital admissions among men, although later we return to study the impact of the depenalization policy on the severity of admissions, as measured by the length of hospital stays, for both Class-A and cannabis related admissions.

4 Baseline Results

4.1 The Impact of the LCWS by Cohort

Table 3 presents estimates of the baseline specification (2), where we consider the impact of the LCWS on Class-A drug related hospital admissions rates by male age cohorts. Column 1 shows that in the long-run among those aged 10-19 when the LCWS was introduced, admissions rates rise by 3.1% more in Lambeth relative to the London average, a difference that is statistically significant at the 5% level. Columns 2 and 3 show there also to be significant increases for the two older cohorts at the 10% level. The relative magnitude of the estimated coefficients follow the same pattern as in Table 2, with the largest quantitative impact - in absolute terms - on 30-39 year old males. These impacts are large: the corresponding changes in the rate of admissions per thousand population are 0.025, 0.043, and 0.092 per thousand population, for cohorts aged 10-19, 20-29 and 30-39, respectively. Normalizing each impact by the pre-policy levels of admission rates reported in Column 1 of Table 2, we note that the estimated impact of the depenalization policy corresponds to a 64% increase over baseline levels of hospital admissions for the 10-19 age cohort, an increase of 51% over baseline levels for the 20-29 age cohort, and a 33% increase over baseline levels for the 30-39 age cohort. as expected each of these is slightly smaller than the unconditional policy impact reported in relation to Table 2, but it remains the case that in proportionate terms, the impacts of the depenalization policy are largest for the younger age cohorts.

The second row of Table 3 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 depenalization policy on hospitalization rates takes some time to work through. The other coefficients in Table 3 - the estimates of β_0 and β_2 - show that for London on average, there are no significant long-term time trends in admission rates before and after the policy except for the 30-39 cohort shown in Column 3, where rates rise by 5.2% in the post-policy period relative to the pre-policy period.

Our results therefore suggest the depenalization of cannabis led to longer term increases in the use of Class-A drugs. If the depenalization policy led to a decline in the equilibrium price of cannabis in Lambeth, as it is often argued to be one unambiguous effect of such policies [Kilmer *et al.*, 2010], then this result suggests that cannabis an Class-A drugs have a negative cross-price elasticity.[15] This would be in line with other studies that have estimated the cross-price elasticity between cannabis and cocaine - either using decriminalization as a proxy for a price reduction [**?**Grossman and Chaloupka, 1998], or using actual price information [Williams *et al.*, 2004].

The remaining Columns in Table 3 split each age cohort by hospital admission histories related to drugs and alcohol in the pre-policy period from January 1997 to June 2001. We do so to allow for the possibility that those with a prior record of substance abuse resulting in hospital admission might respond differently to the depenalization of cannabis than does the rest of the population.[16] Relative to the existing literature linking drug enforcement policies and health, this allows us to present novel evidence on the characteristics of the marginal individuals most impacted by a policy of depenalizing cannabis. Our coefficients of interest remain the differential impact over time of the policy in Lambeth relative to the rest of London.

Columns 4 and 5 consider admissions among the cohort aged 10-19 on the eve of the LCWS

[15] Unfortunately no reliable information on the price of illicit drugs exists at the borough level for our study period.

[16] During this pre-policy period 9368 individuals were admitted to hospital for drugs or alcohol (primary or non-primary): 710 in the 10-19 cohort; 2709 among those 20-29; and 5949 among those aged 30-39.

policy. For those without a prior record of admissions, the estimated policy effects in both the policy and post-policy periods are quantitatively very similar to those for the 10-19 aged cohort as a whole. Amongst those with a prior history, a group of 710 individuals, hospital admissions rates for Class-A drugs increase by 15.2% more in Lambeth relative to the rest of London during the post-policy period, and by 19.3% more during the actual policy-period. Both impacts are significant at the 1% level. Columns 6 and 7 show a very similar pattern of changes in hospital admissions rates for the 20-29 cohort: among those with no prior history of drugs or alcohol admissions, the LCWS is associated with 4.1% long-run rise in admission rates, while the corresponding long-run effect amongst those with a history (2,709 individuals) is ten times larger, at 41% as shown in Column 7. In the short-run when the LCWS is actually in place, there is no impact among those without prior admissions histories, yet there is a large and statistically significant increase of 154% among those with prior admissions. Finally we note the estimates of β_0 and β_2 in Columns 4 to 7 suggest that for these age cohorts, hospital admission rates are generally trending *downwards* in London boroughs on average.

For the oldest cohort, we again find a statistically significant long run impact of the policy on hospital admissions rates, of 15.4%. This impact is over and above the London-wide upward trends in admissions rates for this cohort. However for those aged 30-39 on the eve of the policy and with pre-policy admissions histories for illicit drug or alcohol use, we find no significant impact of the LCWS. This result is in sharp contrast to those for younger cohorts. This divergence might relate to the characteristics of substance abusers aged 30-39 to be different from those abusers at younger ages. For example, given that most individuals begin using illicit drugs earlier on life, such individuals are likely to be long term drug users and as such may be selectively more resistant to the ill effects on health from drug use. Hence on the margin, such long term drug users are not much impacted by the depenalization of cannabis in terms of health.[17]

The results in Table 3 lead us to three important observations. First, amongst those without a history of admissions, there are positive and statistically significant effects only in the post-policy period. This indicates that it took some time for changes in drug use to filter through to hospital admissions. By contrast, the response amongst those with a prior record was more immediate. Second, for the 10-19 and 20-29 age cohorts, the marginal effect on admissions in the post-policy period were greater amongst those with a prior admission history. This is as expected, as those with with a prior record of risky behavior are likely to use drugs at the start of the policy or have access to the illicit drugs market. Third, although the marginal effect amongst those with prior admissions is high, the sample is very small relative to the population as a whole. As a result, the absolute increase in the number of admissions is greater among those without a record of pre-policy risky behavior. Our results therefore reveal that, on the margin, individuals with

[17] This finding is also 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.

previous admissions for risky behavior are the most vulnerable to changes in this type of police enforcement policy in the cannabis market, but that the majority of any increase in healthcare costs will be originate from those without such a history.[18]

4.2 Robustness Checks

We present three sets of robustness checks on the main results presented in Table 3. These relate to whether the documented policy impacts persist once borough specific time trends in Class-A hospitalizations are allowed for, whether the results might just reflect a general worsening in public health in Lambeth relative to the rest of London over this period, and estimating policy impacts accounting for censoring.

The first set of robustness checks address the concern that differential time trends between Lambeth and the rest of London could account for at least part of the documented policy impacts across cohorts. We address this using two methods. The first exploits the four years of panel data prior to the introduction of the depenalization policy to test whether within the pre-policy period there is any evidence of a divergence in trends in hospitalization rates between Lambeth and the rest of London. To do so we estimate (2) but additionally control for an interaction between the Lambeth dummy and a dummy set equal to one from mid way during the pre-policy period (Q1 2000) until the actual start of the policy (Q2 2001), and zero otherwise. For all male age cohorts, this placebo dummy interaction is not found to be 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 evidence related to the underlying motivation behind why the policy was introduced, that emphasized the ability of 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 concerns over worsening public health related to drug-related hospital admissions.

A second method by which we address the concern of differential trends is to augment (2) with controls for borough specific linear time trends. Results are presented in Table A2, again broken down for cohorts based on age and prior admissions histories.[19] For the specification by age cohort in Columns 1-3, the magnitude of the long run impact of the depenalization policy on hospitalization rates is *larger* for all three cohorts relative to the baseline point estimates

[18]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 those that have no prior history of hospital admissions related to illicit drugs or alcohol, be it because they were not consuming illicit drugs, or were consuming them in moderation, are significantly impacted by the depenalization policy.

[19]For the specifications by age cohort (irrespective of admissions history) in Columns 1-3 of Table A2, the borough specific time trend is assumed to be linear over the sample period. For the specifications where cohorts are split by pre-policy drug or alcohol admissions in Columns 4-9 of Table A2, for the samples without a prior record of hospital admissions the time trend is set to zero pre-policy and allowed to be linear thereafter ($\lambda_b \times$quarters post Q3 2001).

presented in Table 3, and remains precisely estimated for two of them. In Columns 4-6 for those without a pre-policy record of risky behavior, the results are similar to the baseline results reported earlier. In particular, for all age cohorts we find no short run impact of the policy, and we do find significant long run increases in Class-A drug related admissions for all age cohorts. As with the baseline results, the magnitude of the effect increases with age cohort so the most pronounced quantitative effect is for the 30-39 age cohort of males with no prior history of substance abuse related admissions. Moreover, once borough specific time trends are controlled for, the magnitudes of the effects are even larger than those reported in the baseline estimates.

For those cohorts with pre-policy admissions for drugs or alcohol, controlling for linear time trend does not effect the estimated short run impact of the LCWS during its actual operation: admission rates rise by 18.8% for the 10-19 cohort and 152%, for the 20-29 cohort, while there is no impact on the oldest cohort, all in line with the baseline estimates presented. However, the long run impact of the policy remains statistically significant only for the 20-29 year old cohort, and not for the 10-19 year old cohort with prior admissions histories.

Overall, we therefore find the majority of the effects previously documented continued to hold with the inclusion of borough specific linear time trends. This conclusion remains unchanged when we also control for quadratic and cubic time trends.

The second set of robustness checks address the concern the results pick up divergent trends in ill health more generally between Lambeth and other London boroughs. To check for this in Table A3 we re-estimate (2) using admissions rates for the most common ICD-10 'chapter', or group of diagnoses, among these age cohorts: those related to digestive diagnoses (including appendicitis and hernias). Such admissions should be unaffected by the LCWS policy related to the regulation of cannabis. The first three columns show results for age cohorts, the last six columns again split by age cohort and pre-policy admission histories for drugs or alcohol. For each age cohort, the evidence suggests hospitalization rates are significantly *falling* over time during the post-policy period. This is in complete contrast to the results for Class-A drug related admissions shown in Table 3. When the cohorts are further split by pre-policy admissions histories related to drugs or alcohol, most of the impacts become imprecisely estimated. Overall this does not suggest that the baseline estimates in Table 3 reflect worsening population health in Lambeth *per se*, but rather the increase in hospitalization rates for Class-A drug use is specifically linked to changes in police enforcement in the cannabis market in Lambeth.[20]

Finally, we note that the specifications estimated using OLS regressions obviously do not account for the censoring in the data. This is an issue given that in the pre-policy period, there

[20] Further robustness tests find no evidence of a corresponding increase in admission rates in the London Borough of Camden. This borough is chosen as a suitable comparison as it had high rates of pre-policy drug admissions but experience limited spillovers from the LCWS, as it is located in north London. We also find the results to be robust when we restrict the sample to the seven boroughs (including Lambeth) that have an Emergency Hospital Department, suggesting the results are not driven by any change, contemporaneous with the LCWS, in how the residence of patients is recorded when they arrive at hospital.

are many observations where the admission rate is zero are - indeed this is the case for the vast majority of observations among the 10-19 age cohort. We address this issue by re-estimating (2) using Tobit specifications. This allows us to estimate the impact of the policy at both the extensive (probability that there is at least one admission in a given borough-quarter) and intensive margins (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 the average interaction term for $PP_{qy} \times Lambeth$ and $P_{qy} \times Lambeth$.[21] Estimated policy effects on the extensive and intensive margins are presented in Table A4 by age cohort. Among the youngest cohort for whom the censoring issue is most severe, shown in Columns 1 and 2, there is a significant increase in admission rates in the post-policy period, on both extensive and intensive margins of hospital admissions related to Class-A drug use. Hence post-policy there are more quarters in Lambeth in which at least one hospital admissions occurs related to a Class-A drug diagnosis, and greater admissions conditional on their being at least one admission. For the two older cohorts the Tobit specifications show that, in line with the OLS results, on the intensive margin there is a significant increase in admission rates related to Class-A diagnoses at the 5% significance level.

5 Extended Results

In this Section we consider three margins of policy impact in more detail: the dynamic response, spillover effects into neighboring boroughs driven by drug tourism, 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 the change in drug enforcement policy.

[21] 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 in the post-policy period is therefore equal to the following:

$$\begin{aligned}\widehat{\beta_3} = (&\widehat{E}[AR_{qyb}|PP_{qy} = 1, L_b = 1, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, AR_{qyb} > 0]- \\ &\widehat{E}[AR_{qyb}|PP_{qy} = 0, L_b = 1, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, Admits_{qyb} > 0])- \\ &(\widehat{E}[AR_{qyb}|PP_{qy} = 1, L_b = 0, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, Admits_{qyb} > 0]- \\ &\widehat{E}[AR_{qyb}|PP_{qy} = 0, L_b = 0, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, Admits_{qyb} > 0]) \end{aligned} \quad (3)$$

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.

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 took to filter through to hospital admissions, and whether, and how quickly, those effects eventually die out. This is of policy relevance as the resultant healthcare costs will depend on the duration of heightened admissions. To chart the time profile of responses, we replace the post-policy period indicator in (2), PP_{qy}, with three 2-year time-brackets: 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),

$$Admit_{dbqy} = \alpha + \beta_0 P_{qy} + \beta_1[L_b \times P_{qy}] + \sum_{k=1}^{3}(\mu_k TB_{qy}^k + \gamma_k[L_b \times TB_{qy}^k]) + \delta X_{bqy} + \lambda_b + \lambda_q + \lambda_y + u_{bqy} \quad (4)$$

where all other variables are as previously defined. This specification is estimated for each 10-year age cohort by pre-policy drugs or alcohol admission history. Impacts of LCWS on admission rates in Lambeth, in each time period (β_1, γ_1, γ_2, and γ_3), are then plotted in Figures 1A and 1B. Figure 1A shows that for those who were not admitted for drugs or alcohol in the pre-policy period, there is a similar pattern of dynamic responses across age cohorts although, in line with the evidence in Table 3, the magnitudes of the impacts are largest for those in the oldest cohort aged 30-39. For each cohort the depenalization policy has no impact during the policy period, but estimated impacts increase thereafter, and are statistically significant at the 5% level 1-2 years post reform and 3-4 years post reform. In the final period considered, the estimated effect of the reform falls, and remains statistically significant only for the oldest cohort.

The results for those who were admitted for drugs or alcohol in the pre-policy period are very different, as shown in Figure 1B. For the youngest cohort, there are significant rises in hospital admission rates related to Class-A drugs during the policy period, and 1-2 years post reform. For the 20-29 cohort, there is an increase of over 150% in the policy-period, but this drops thereafter and remains relatively stable in the longer term. As suggested in Table 3, the depenalization policy has no impact on the oldest cohort when the policy is initiated. This dynamic pattern of responses for those with prior records of risky behavior therefore points to a change in drug consumption induced by the new policing regime, which led to an immediate rise in hospital admissions and are long lasting for those who were aged 10-19 and 20-29 when the policy began.

In comparison to the literature linking policies to regulate the market for illicit drugs and public health, 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 years for many cohorts even though the policy itself is only in place for a year.

5.2 Spillovers in Neighboring Boroughs

As mentioned earlier, Adda *et al.* [2011] provide detailed evidence on the impact of the depenalization of cannabis in Lambeth on patterns of crime both in Lambeth and in neighboring boroughs. An important finding that is relevant for the current study is that the depenalization policy in Lambeth induced a substantial degree of drugs tourism from geographically neighboring boroughs into Lambeth. Indeed such drugs tourism can explain around half the estimated long run increase in cannabis possession offences within Lambeth. We analyze whether there are similar spillover effects on health in Lambeth's neighbors, in terms of hospital admission rates for Class-A related drugs. Our administrative data on hospital admissions further allows us to shed light on the nature of drug tourists, by exploring how the marginal individual impacted in neighboring boroughs differs from the marginal individual resident in Lambeth itself. The existence of such spillover effects are important from a policy perspective if the true impact of the policy on public health is to be evaluated.

To do so we augment (2) with interactions between the policy and post-policy period and whether the borough (of residence) is a geographic neighbor ($N_l = 1$) or not ($N_l = 0$).[22] As the characteristics of drug tourists might correlate to their pre-policy hospital admissions history, we find it informative to again split the results by age and admissions history cohorts. Table 4 presents the results.

In Columns 1, 3 and 5 we see that among those without pre-policy admissions records, there are significant increases in admissions rates in neighboring boroughs for the 20-29 and 30-39 age cohorts. For the 20-29 cohort, the estimated 2.5% rise in neighboring boroughs is not significantly different from the corresponding increase in Lambeth (4.6%). The impact for the 30-39 cohort is around four times larger in Lambeth (16%) than in neighboring boroughs (4%), with the difference statistically significant at the 1% level. The presence of such positive spillover effects into neighboring boroughs suggests our baseline results likely underestimate the true differential impact of the policy between Lambeth and boroughs truly unaffected by the depenalization of cannabis in Lambeth. Columns 2, 4 and 6 suggest that for those who were admitted for drugs or alcohol in the pre-policy period, there are no significant spillovers in terms of hospitalizations for Class-A related drug diagnoses. Indeed the coefficients for the interactions between the neighbor dummy, N_l,

[22] Boroughs that neighbor Lambeth are Southwark, Croydon, Wandsworth and Merton.

and the post-policy period is negative and statistically significant for the 20-29 and 30-39 cohorts suggesting such admissions are *falling faster* in neighbors to Lambeth than the London average.

Taken together these results suggest that among those that reside in neighboring boroughs, the drug tourists that are induced to travel to Lambeth as a result of the depenalization of cannabis are more likely to be those that have no prior history of hospitalization for drug or alcohol: these cohorts include those that are likely to have never consumed cannabis or consumed it in small quantities. In contrast among those cohorts whose prior involvement is risky behavior has been extreme enough to result in hospital admission for diagnoses related to illicit drugs or alcohol, relatively fewer individuals are induced to travel to Lambeth as a result of cannabis being depenalized there. This is in contrast to the baseline results for Lambeth presented in Table 3 where the proportionate impact of the depenalization policy among Lambeth residents was largest among those with prior admissions histories related to the use of illicit drugs or alcohol.

5.3 Severity of Hospital Admissions

A final dimension along which to consider the policy impact relates to the severity of hospitalizations, as measured by the number of days the individual is required to stay in hospital for the episode. As with the other extended results considered, this margin is of policy relevance because it maps directly into the resultant healthcare costs associated with the depenalization of cannabis. To check for this we estimate a specification analogous to (2) but where the dependent variable is the length of hospital stay in days. To avoid the results being driven by outliers we drop observations where the length of stay is recorded to be longer than 100 days. Table 5 presents the results split by age cohort. Columns 1-3 focus on the policy impact on the length of stays related to Class-A drug diagnoses. We see that in the long run across all three age cohorts, the length of stay significantly increases in Lambeth relative to the London average. For example, among the 10-19 age cohort, hospital stays increase by 3.7 days, and this is relative to a baseline pre-policy hosp[ital stay length of 6.3 days, an increase of 59%. the proportionate changes for the age other cohorts are 36% for the 20-29 age cohort and 20% for the oldest age cohort. Hence as with the main estimates on hospitalization rates for Class-A drug admissions, the proportionate changes in length of hospital stay are also greater for age cohorts that were younger at the time the depenalization policy was introduced. Finally, the results show that in other London boroughs there are no time trends in the duration of such hospitalizations conditional on all other controls in (2). Hence the findings do not appear to be driven by some systematic lengthening of hospital stays for such diagnosis that might be occurring more generally across London.

Columns 4-6 replicate this analysis but specifically related to the length of hospital stays for cannabis related diagnoses. As discussed earlier and shown formally in Table A1, hospitalization rates for cannabis-related diagnoses are very low and as such, it is impossible to find statistically significant policy impacts on this margin. However the results in Columns 4-6 suggest that among such hospitalizations, length of stay significantly diverges between Lambeth and the London average in the long run. In terms of the magnitudes of the effects, relative to baseline levels the impact corresponds to an 86% increase in hospital stay length for the 10-19 age cohort, and a 52% increase for both the 20-29 and 30-39 age cohorts. As with the results related to Class-A drug diagnoses, there are no trends in hospital stays for cannabis related diagnosis in the rest of London. This final piece of evidence is reassuring in the sense that there are some margins related to cannabis hospitalization that are impacted by the depenalization of cannabis in Lambeth.

6 Discussion

This paper evaluates the impact on drug-related hospital admissions of a localized policing experiment that depenalized the possession of small quantities of cannabis in the London borough of Lambeth. To do so we exploit administrative records covering individual admissions into hospital and that provide information on detailed ICD-10 diagnosis classifications. We use these records to construct a panel data set by London borough and quarter from 1997 to 2009 to estimate the short and long run impacts of the depenalization policy unilaterally introduced into Lambeth between 2001 and 2002. Our analysis contributes to the nascent literature evaluating the health impacts of changes in enforcement policies in the market for illicit drugs.

We document five main findings. First, relative to the rest of the London, the depenalization policy had significant long term impacts on hospital admissions in Lambeth related to the use of Class-A drugs. The impacts are concentrated among men, for whom hospitalization rates significantly increase as a result of the depenalization policy. Second, exploring the heterogeneous impacts across cohorts we find the impacts to be proportionately larger relative to baseline among cohorts that were younger at the start of the policy, and proportionately larger among those with prior histories of hospitalization related to drug or alcohol use. The magnitudes of the impacts are large, corresponding to between 33 and 64% of pre-policy levels of hospital admission rates, across age cohorts. Third, the dynamic impacts across cohorts vary in profile with some cohorts experiencing hospitalization rates remaining above pre-intervention levels six years after the depenalization of cannabis was first introduced. Fourth, we find evidence of positive spillover effects on hospitalizations related to Class-A drug use in geographic neighbors to Lambeth, and these are concentrated among cohorts without prior histories of hospital admissions related to the use of illicit drugs or alcohol. Finally, the severity of hospital admissions, as measured by the length of stay in hospital, significantly increases for both admissions related to Class-A diagnoses and those related to cannabis diagnoses.

Taken together, 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 offences 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.*, 2011], there remain potentially large 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, given our administrative records include only those patient-episodes where the patient was admitted with the intention of an overnight hospital stay, these records cover the most extreme health events. Patients with less serious conditions receive treatment as a hospital outpatient, namely without an overnight hospital stay or through primary care services. If such health events are also impacted by drugs policing strategies, our estimates based solely on inpatient records provide a lower bound impact of the depenalization of cannabis on public health.

Second, the LCWS policing experiment we evaluate is a policy whereby the possession of small quantities of cannabis was depenalized. The practical way in which the policy 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 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].

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". This is in part because early intervention could then prevent the use of drugs that generate greater costs to society. 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; Beenstock and Rahav, 2002; DiNardo and Lemiuex, 2001]; or, (ii) using econometric techniques to model the possible effects of unobserved heterogeneity [van Ours, 2003; Pudney, 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 linked in some way - 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. In the ongoing debate about the relative weight to give to policies of drug prevention, enforcement, and treatment [Grossman *et al.*, 2002], our evidence suggests that more liberal enforcement policies can have substantial consequences on public health, in addition to impacts on crime that have been previously studied [Draca *et al.*, 2011; Adda *et al.*, 2011; DiTella and Schargrodsky, 2004]. It is therefore 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.

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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:			
	All Other Diagnoses	Cannabis	Class-A Drugs	Alcohol
	(1)	(2)	(3)	(4)
Admitted in 2000-2004 for:				
All Other Diagnoses	.316	.270	.146	.208
	(.465)	(.444)	(.353)	(.409)
Cannabis	.001	.077	.011	.005
	(.034)	(.267)	(.105)	(.071)
Class-A	.004	.058	.257	.022
	(.061)	(.233)	(.440)	(.145)
Alcohol	.015	.094	.064	.225
	(.121)	(.292)	(.245)	(.418)
Observations (individuals)	485992	711	3950	15595

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The figures refer to the probability of re-admission as a hospital inpatient over the period 2000 to 2004, conditional on an earlier hospital admission in 1997 or 1998. 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 (any further) diagnosis. We exclude a small number of cases for those admitted for more than one behavior related to cannabis, Class-A drugs and alcohol. The sample is based on all male individuals 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, in addition to London.

Table 2: Class-A Drug Related Hospital Admissions, by Borough and Time Period

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

	Lambeth		Rest of London		Post-Policy - Pre-Policy Difference	
	(1) Pre-Policy	(2) Post-Policy	(3) Pre-Policy	(4) Post-Policy	(5) Unconditional	(6) Fixed Effects
All Men	.156	.223	.0617	.0829	.0500*	.0495*
	(.0609)	(.0921)	(.0619)	(.0634)	[.0293]	[.0284]
All Women	.104	.106	.0286	.0367	-.00707	-.00546
	(.0508)	(.0510)	(.0390)	(.0377)	[.0131]	[.0124]
Men aged 10-19	.00388	.0840	.00445	.0489	.0354**	.0354**
	(.0165)	(.0882)	(.0197)	(.0680)	[.0171]	[.0166]
Men aged 20-29	.0849	.162	.0632	.0921	.0481**	.0493**
	(.101)	(.0856)	(.0819)	(.0932)	[.0222]	[.0245]
Men aged 30-39	.281	.385	.0965	.101	.106*	.107*
	(.111)	(.187)	(.118)	(.117)	[.0620]	[.0581]
Observations (borough-quarter-year)	18	30	558	930	-	-

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the log of the number of Class-A drug related hospital admissions plus one, where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Observations are at the quarter-borough-year level and are weighted by 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. 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 regression specification that also controls for borough, quarter and year fixed effects. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, July 4th 2001.

Table 3: 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			By Cohort and Pre-Policy Drugs or Alcohol Admissions					
Age Cohort:	**Aged 10-19**	**Aged 20-29**	**Aged 30-39**	**Aged 10-19**		**Aged 20-29**		**Aged 30-39**	
Pre-Policy Drugs or Alcohol Admissions:				**No**	**Yes**	**No**	**Yes**	**No**	**Yes**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Post-Policy x Lambeth	.0307**	.0373*	.0736*	.0288*	.152***	.0409**	.409**	.154***	-.283
	(.0147)	(.0205)	(.0407)	(.0153)	(.054)	(.0169)	(.195)	(.0382)	(.212)
Policy Period x Lambeth	.00622	.0334	-.0444	.00381	.193***	.00998	1.539***	.0252	-.241
	(.0278)	(.0358)	(.0695)	(.0276)	(.074)	(.0286)	(.380)	(.0618)	(.396)
Post-Policy	.00937	-.0166	.0517**	.0123	-.204	.0034	-.629***	.0722***	-.387
	(.0102)	(.0136)	(.0202)	(.0103)	(.135)	(.012)	(.240)	(.0155)	(.278)
Policy-Period	.00222	-.0176*	.00936	.00521	-.220**	.0154*	-1.03***	.0472***	-.741***
	(.00698)	(.00922)	(.0138)	(.00729)	(.0972)	(.00813)	(.165)	(.0107)	(.192)
Borough, Quarter and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	.284	.264	.491	.225	.080	.299	.242	.384	.410
Observations (borough-quarter-year)	1664	1664	1664	1664	1664	1664	1664	1664	1664

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the log of the number of Class-A drug related hospital admissions per 1000 of the population in the cohort, plus one, where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Observations are at the quarter-borough-year level and are weighted by population of the borough relative to the population of London. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. The 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. Columns 1, 4, and 5 relate admissions to those aged 10-19 on 1st July 2001. Columns 2, 6, and 7 relate to admissions of those aged 20-29 on 1st July 2001. Columns 3, 8 and 9 relate to admissions for those aged 30-39 on 1st July 2001. All specifications include borough, quarter and year 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. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level.

Table 4: Impacts in Neighboring Boroughs

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

Age Cohort:	Aged 10-19		Aged 20-29		Aged 30-39	
Pre-Policy Drugs or Alcohol Admissions:	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Policy x Lambeth (PP x L)	.0291*	.162***	.0455***	.345*	.157***	-.336
	(.0150)	(.0567)	(.0168)	(.200)	(.0383)	(.218)
Policy Period x Lambeth (P x L)	.00239	.199**	.00826	1.45***	.0246	-.341
	(.0270)	(.0773)	(.0285)	(.390)	(.0624)	(.405)
Post-Policy x Neighbours (PP x N)	.00246	.0961	.0253**	-.380**	.0396***	-.334**
	(.00651)	(.101)	(.0112)	(.154)	(.0137)	(.158)
Policy-Period x Neighbours (P x N)	-.0102	.0314	-.0218	-.530*	.000268	-.700**
	(.0113)	(.172)	(.0196)	(.281)	(.0222)	(.283)
Post-Policy (PP)	.0120	-.220	-.000	-.568**	.0675***	-.337
	(.0103)	(.136)	(.0120)	(.240)	(.0154)	(.279)
Policy-Period (P)	.00671	-.227**	.0183**	-.939***	.0469***	-.644***
	(.00732)	(.100)	(.00848)	(.167)	(.0107)	(.197)
Testing Differences between Lambeth and Her Neighbors						
(PP x L) - (PP x N)	.0266	.0660	.0203	.725***	.118***	-.00206
	(.0181)	(.0972)	(.020)	(.219)	(.036)	(.221)
(P x L) - (P x N)	.0126	.167	.0301	1.978***	.0243	.359
	(.0293)	(.167)	(.0346)	(.420)	(.0662)	(.411)
Borough, Quarter and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	.225	.081	.305	.244	.389	.412
Observations (borough-quarter-year)	1664	1664	1664	1664	1664	1664

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the log of the number of Class-A drug related hospital admissions per 1000 of the population in the cohort, plus one, where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Observations are at the quarter-borough-year level and are weighted by population of the borough relative to the population of London. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. The 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. Columns 1 and 2 relate admissions to those aged 10-19 on 1st July 2001. Columns 3 and 4 relate to admissions of those aged 20-29 on 1st July 2001. Columns 5 and 6 relate to admissions for those aged 30-39 on 1st July 2001. The Neighbors dummy variable is equal to one when the local authority neighbors Lambeth (Croydon, Merton, Southwark and Wandsworth), and zero otherwise. All columns include borough, quarter and year 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. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level.

Table 5: Impacts on Length of Hospital Stay

Dependent Variable: Length of Hospital Stay in Days for Males Admitted with Drug-Related Diagnoses

Drug:	Class-A			Cannabis		
Age Cohort:	Aged 10-19	Aged 20-29	Aged 30-39	Aged 10-19	Aged 20-29	Aged 30-39
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Policy x Lambeth	3.679*	3.710***	2.249***	8.815**	8.381***	8.204***
	(2.150)	(1.132)	(.652)	(3.779)	(2.605)	(2.505)
Policy Period x Lambeth	.473	10.61***	-4.050***	24.32***	4.006	-5.077
	(3.410)	(1.363)	(1.197)	(5.009)	(4.363)	(3.092)
Post-Policy	-5.680	-2.089	1.590	3.398	4.206	1.955
	(7.342)	(4.425)	(1.891)	(8.734)	(5.062)	(4.942)
Policy-Period	-2.067	-.415	2.305	5.462	.114	-.622
	(3.075)	(2.885)	(1.459)	(11.67)	(5.421)	(8.183)
Borough, Quarter and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-Squared	.104	.081	.099	.096	.110	.121
Observations (borough-quarter-year)	790	2770	3725	1588	1685	1407

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable in all columns is the number of days spent in hospital (discharge date - admission date) capped at 100 days. In Columns 1-3 the sample is restricted to those who are admitted for Class-A drugs; in Columns 4-6, the sample is restricted to those admitted with a cannabis related diagnoses. Observations are at the episode level, and the sample is restricted to those admitted to hospital for cannabis. 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. Columns 1 and 4 relate admissions to those aged 10-19 on 1st July 2001. Columns 2 and 5 relate to admissions of those aged 20-29 on 1st July 2001. Columns 3 and 6 relate to admissions for those aged 30-39 on 1st July 2001. All columns include borough, quarter and year 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: Cannabis Related Hospital Admissions, by Borough and Time Period

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

	Lambeth		Rest of London		Post-Policy - Pre-Policy Difference	
	(1) Pre-Policy	**(2) Post-Policy**	**(3) Pre-Policy**	**(4) Post-Policy**	**(5) Unconditional**	**(6) Fixed Effects**
All Men	.075	.120	.023	.082	-.011	-.011
	(.075)	(.012)	(.033)	(.075)	[.031]	[.025]
All Women	.054	.041	.012	.029	-.033	-.031
	(.0508)	(.051)	(.039)	(.038)	[.020]	[.017]
Men aged 10-19	.015	.183	.009	.121	.063	.061
	(.036)	(.039)	(.031)	(.089)	[.065]	[.057]
Men aged 20-29	.073	.095	.033	.073	-.019	-.015
	(.074)	(.049)	(.059)	(.093)	[.021]	[.017]
Men aged 30-39	.104	.110	.025	.061	-.029	-.030
	(.132)	(.093)	(.048)	(.079)	[.035]	[.032]
Observations (borough-quarter-year)	18	30	558	930	-	-

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the log of the number of hospital admissions plus one, where the diagnosis (primary or secondary) refers to cannabis. Observations are at the quarter-borough-year level and are weighted by 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. 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 regression specification that also controls for borough, quarter and year fixed effects. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001.

Table A2: Robustness Check 1 - Time Trends

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

Age Cohort:	Aged 10-19	Aged 20-29	Aged 30-39	Aged 10-19		Aged 20-29		Aged 30-39	
Pre-Policy Drugs or Alcohol Admissions:				No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post-Policy x Lambeth	.0530*	.0525	.203***	.0475**	.106	.0559**	.561**	.230***	-.0125
	(.0300)	(.0385)	(.0729)	(.0211)	(.0801)	(.0232)	(.275)	(.0451)	(.284)
Policy-Period x Lambeth	.0152	.0386	.00238	.00455	.188**	.0114	1.517***	.0257	-.282
	(.0293)	(.0371)	(.0682)	(.0270)	(.0754)	(.0287)	(.384)	(.0546)	(.381)
Post-Policy	.00982	-.0172	.0488***	.0196*	-.316**	.0112	-.862***	.0682***	-.528*
	(.00998)	(.0133)	(.0189)	(.0105)	(.139)	(.0120)	(.231)	(.0163)	(.291)
Policy-Period	.00257	-.0172*	.00868	.00972	-.292***	.0209***	-1.163***	.0454***	-.819***
	(.00678)	(.00899)	(.0128)	(.00726)	(.0971)	(.00801)	(.157)	(.0111)	(.198)
Borough, Quarter and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borough Specific Post-Policy Linear Time Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	.307	.251	.514	.248	.096	.265	.120	.332	.259
Observations (borough-quarter-year)	1664	1664	1664	1664	1664	1664	1664	1664	1664

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the log of the number of Class-A drug related hospital admissions per 1000 of the population in the cohort, plus one, where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Observations are at the quarter-borough-year level and are weighted by population of the borough relative to the population of London. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. The 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. Columns 1, 4 and 5 relate admissions to those aged 10-19 on 1st July 2001. Columns 2, 6 and 7 relate to admissions of those aged 20-29 on 1st July 2001. Columns 3, 8 and 9 relate to admissions for those aged 30-39 on 1st July 2001. Columns 1-3 control for borough specific linear time trends; Columns 4-9 control for post Q3 2001 borough specific linear time trends. All Columns include borough, quarter and year 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. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level.

Table A3: Robustness Check 2 - Admission Rates for Diseases of the Digestive System

Dependent Variable: Male Hospital Admission Rates for Non-drug Related Diseases

Age Cohort:	Aged 10-19	Aged 20-29	Aged 30-39	Aged 10-19		Aged 20-29		Aged 30-39	
Pre-Policy Drugs or Alcohol Admissions:				No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post-Policy x Lambeth	-.248***	-.513***	-.329***	.0509	1.171**	-.0146	-.000536	.0668**	-.245
	(.0900)	(.0797)	(.0536)	(.0735)	(.554)	(.0377)	(.415)	(.0294)	(.235)
Policy-Period x Lambeth	-.0314	-.168	-.0910	.101	-.158	.0824	.0782	.0361	.0403
	(.0999)	(.120)	(.0874)	(.116)	(.944)	(.0658)	(.750)	(.0507)	(.409)
Post-Policy	.0170	-.0540	-.0484	-.0392	-.0915	-.126	-.744	-.101	-.790*
	(.0770)	(.156)	(.166)	(.143)	(.316)	(.196)	(.462)	(.227)	(.473)
Policy-Period	-.00239	-.0389	-.0368	-.0239	.107	-.0682	-.653**	-.0810	-.558*
	(.0530)	(.108)	(.114)	(.102)	(.221)	(.136)	(.322)	(.156)	(.324)
Borough, Quarter and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	.976	.973	.974	.838	.163	.937	.246	.939	.590
Observations (borough-quarter-year)	1662	1664	1664	1664	1664	1664	1664	1664	1664

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the log of the number of admissions for diseases of the digestive system (ICD-10 Chapter K) per 1000 of the population in the cohort, plus one. Observations are at the quarter-borough-year level and are weighted by population of the borough relative to the population of London. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. The 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. Columns 1, 4 and 5 relate admissions to those aged 10-19 on 1st July 2001. Columns 2, 6 and 7 relate to admissions of those aged 20-29 on 1st July 2001. Columns 3, 8 and 9 relate to admissions for those aged 30-39 on 1st July 2001. All columns include borough, quarter and year 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. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level.

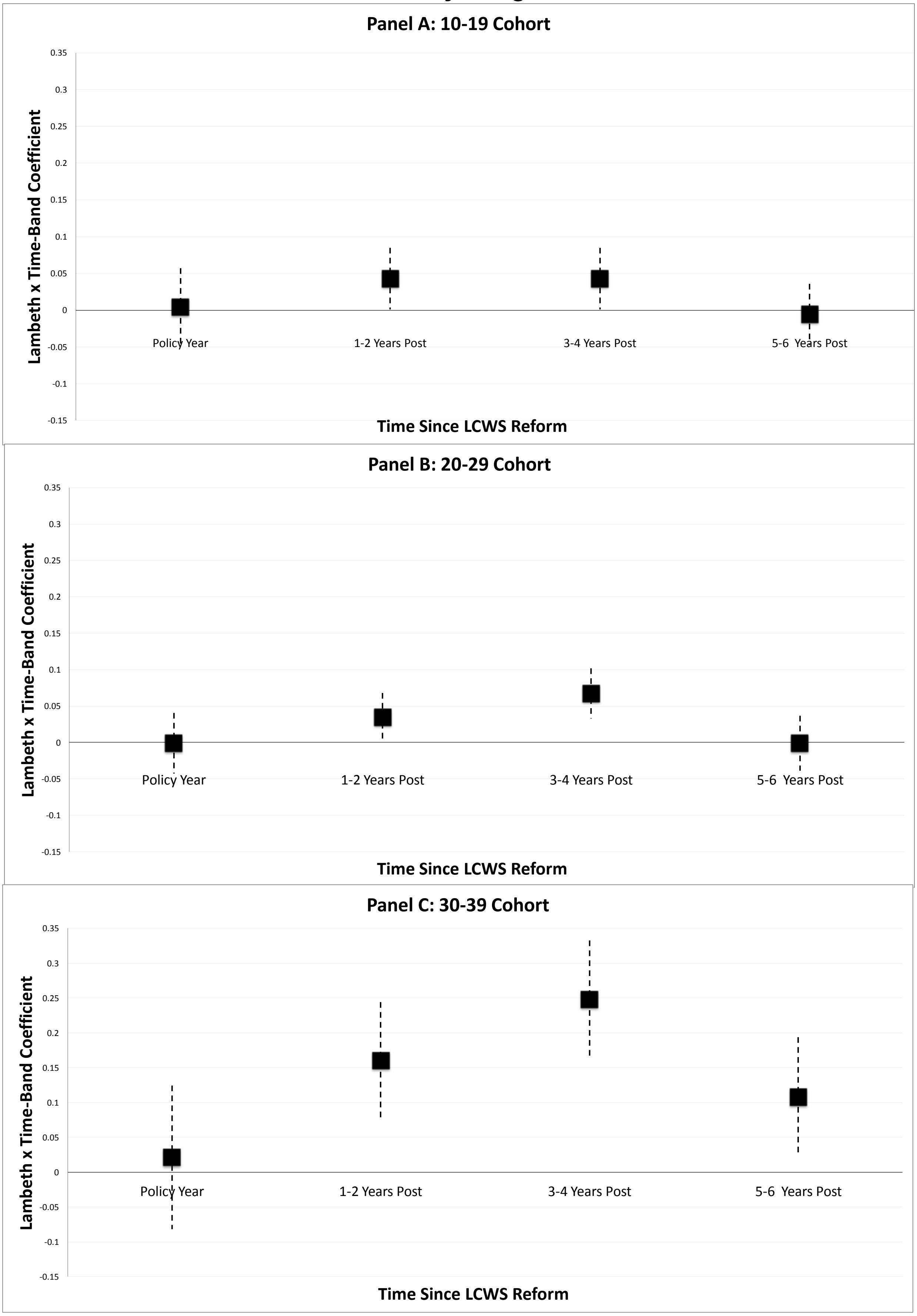
Table A4: Robustness Check 3 - Tobit Specifications

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

Margin:	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Age Cohort:	Aged 10-19		Aged 20-29		Aged 30-39	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Policy x Lambeth	.218***	.0267**	.0645	.0383**	-.0123	.0677**
	(.0824)	(.0123)	(.0874)	(.0168)	(.0169)	(.0294)
Policy-Period x Lambeth	-.0765	-.00864**	-.0448	-.00336	.0831	.0137**
	(.0349)	(.00348)	(.0358)	(.00434)	(.0332)	(.00478)
Borough, Quarter and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations (borough-quarter-year)	1664	1664	1664	1664	1664	1664

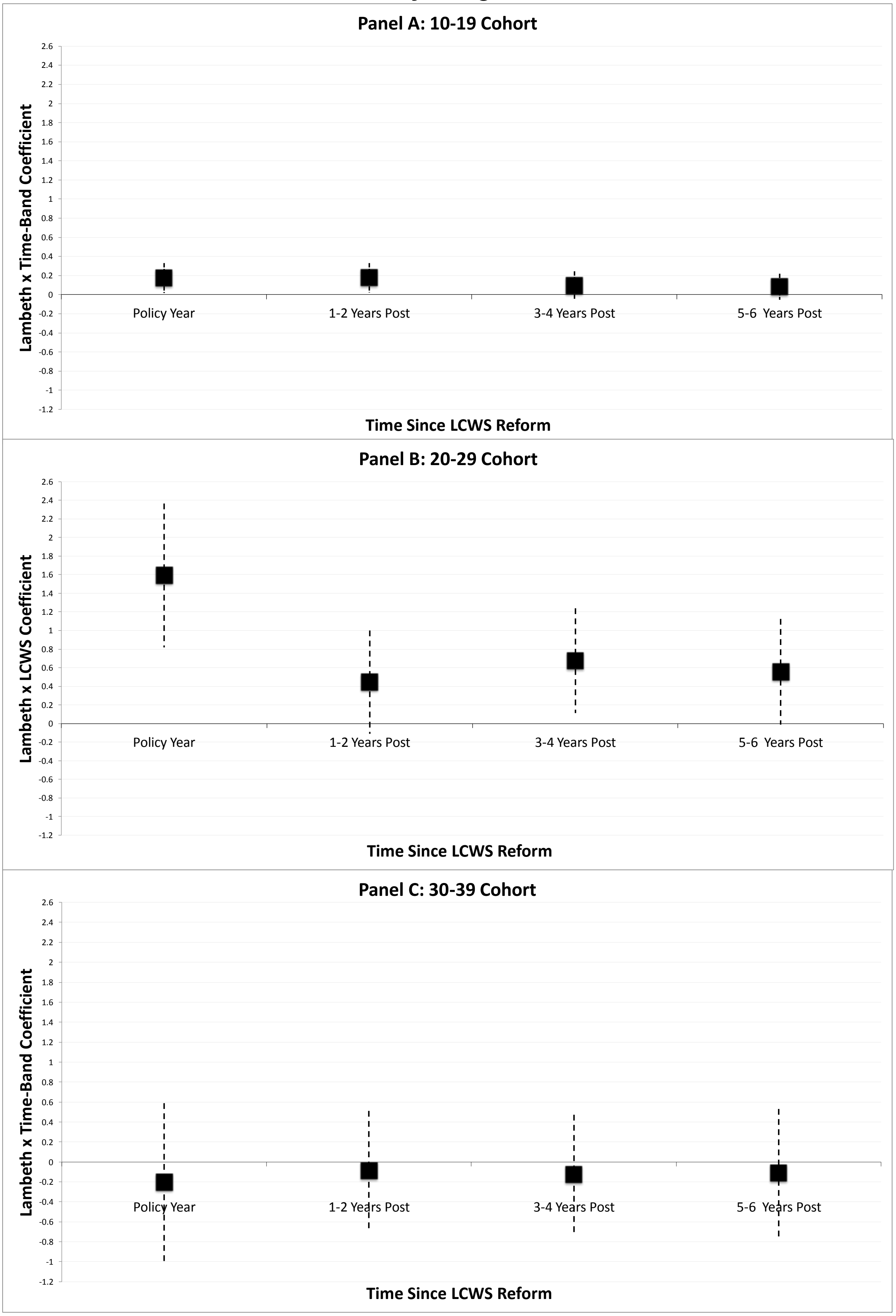
Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the log of the number of Class-A drug related hospital admissions per 1000 of the population in the cohort, plus one, where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Observations are at the quarter-borough-year level and are weighted by population of the borough relative to the population of London. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. The 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 specifications include borough, quarter and year 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. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level. The estimates on the interaction terms, Policy-Period x Lambeth and Post-Policy x Lambeth 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.

Figure 1A: Dynamic Policy Impacts by Male Age Cohort: Cohorts With No Pre-Policy Drugs or Alcohol Admissions



Notes: Each panel refers to a separate specification, analogous to (1), but where Pqy and PPqy by four time dummies: the policy period (Q3 2001-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 pre-policy period is the omitted category. Data from Q3 2008 onwards is excluded. Each plotted square corresponds to the Lambeth x Time Band dummy coefficient. The vertical black lines give the 95% confidence intervals. Age refers to age on the eve of the LCWS introduction (1st July 2001). The sample includes all those who were not admitted for

Figure 1B: Dynamic Policy Impacts by Male Age Cohort: Cohorts With Pre-Policy Drugs or Alcohol Admissions



Notes: Each panel refers to a separate specification, analogous to (1), but where Pqy and PPqy by four time dummies: the policy period (Q3 2001-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 pre-policy period is the omitted category. Data from Q3 2008 onwards is excluded. Each plotted square corresponds to the Lambeth x Time Band dummy coefficient. The vertical black lines give the 95% confidence intervals. Age refers to age on the eve of the LCWS introduction (1st July 2001). The sample includes all those who were previously admitted for drugs or alcohol in the pre-policy period.