Home Office

Do initiatives involving substantial increases in stop and search reduce crime? Assessing the impact of Operation BLUNT 2

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Executive summary

- Stop and search is a well-established police power. Over the last decade use of the power has varied widely but at its peak, in the final quarter of 2008/09, a search was undertaken every 20 seconds on average nationwide. The evidence base on the effectiveness of stop and search on crime is limited. This paper examines whether a police initiative that involved a large increase in the number of stop and searches was effective at reducing crime.

- As part of Operation BLUNT 2 – a Metropolitan Police initiative aimed at reducing knife crime that began in the spring of 2008 – there was a marked increase in the number of weapons searches conducted in London. London boroughs were assigned to one of three tiers based on intelligence on their knife crime problem. Resources were prioritised to ten Tier 1 boroughs, and to a lesser extent to six Tier 2 boroughs. The ten Tier 1 boroughs recorded a more than threefold increase in the number of weapons searches, up from 34,154 in the year before BLUNT 2 to 123,335 in the first year of the operation. Over this period, the 16 Tier 3 boroughs also recorded an increase in weapons searches but on a smaller scale (up by 18,103, an 87% increase on pre-BLUNT 2 levels).

- Under normal circumstances, it is hard to interpret the relationship between changes in stop and search and crime rates. Because it is a form of responsive policing, trends in stop and search often mirror trends in crime, so it is difficult to establish whether stop and searches lead to a fall in crime, or simply reflect it. However, the scale of the increases in searches under Operation BLUNT 2 was less clearly the result of changes in short-term crime rates. This strengthens the robustness of the evaluation.

- The analysis focuses on crimes that might be affected by large increases in weapons searches, and compares changes in offence numbers across the three tiers. If a large increase in weapons searches is effective at reducing knife crime then a drop in offences in Tier 1 boroughs would be expected, compared with boroughs that recorded smaller increases in stop and searches.

- Nine measures of police recorded crime were used in the analysis. These included:
    - different types of assault involving sharp instruments;
    - robbery;
    - weapons and drugs possession offences; and
    - three types of acquisitive crime.

  A difference-in-difference regression analysis, which controlled for other factors that might affect crime trends, found no statistically significant crime-reducing effect from the large increase in weapons searches during the course of Operation BLUNT 2. This suggests that the greater use of weapons searches was not effective at the borough level for reducing crime.

- London Ambulance Service data on calls for weapons-related injuries were also analysed. Unlike recorded crime measures, these should be unaffected by police recording or victim reporting issues. The number of London Ambulance Service call-outs for weapons injuries did not fall more in the Tier 1 boroughs than in the other boroughs. Rather, ambulance call-outs actually fell faster in those boroughs that had smaller increases in weapons searches.

- Knife homicides were examined separately, as the small numbers involved prevent meaningful difference-in-difference analysis. Both Tier 1 (high resource) and Tier 3 (low resource) boroughs saw reductions in knife homicides, so it is unlikely that the falls in Tier 1 boroughs can be attributed to the Operation BLUNT 2 increases in weapons searches.

- Overall, analysis shows that there was no discernible crime-reducing effects from a large surge in stop and search activity at the borough level during the operation. However, it does not necessarily follow that stop and search activity does not reduce crime. This study is based on data at the London borough level, with an average population of over 200,000 per borough. It is possible that there are localised crime-reducing effects of stop and search activity that are masked when analysing data on such a large geographic area. This might be a useful focus of future research. It is also possible that a base level of stop and search activity does have an effect after which there are diminishing, or even zero, returns. This current study has not been able to shed light on what that level would be.

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1. Overview, aims and objectives

'Stop and search' typically refers to the statutory police powers to stop and physically search an individual. The powers originate from the 19th century Vagrancy Act and retain an important role in current police operations. Most types of search need to be justified by 'reasonable suspicion'. This is usually based on intelligence that the search will uncover a specific illegal activity, or the possession of a prohibited or an illegal item. However, more recent legislation allows stop and searches without reasonable suspicion when terrorism, football hooliganism or violence is anticipated. Table 1 sets out the different types of stop and search.

Table 1: Types of stop and search

	Act	Reason/offence targeted	Reasonable suspicion required?
Section 1	The Police and Criminal Evidence Act 1984 (PACE)	Stolen property	Yes
		Offensive weapons	Yes
		Going equipped for stealing	Yes
		Going equipped for criminal damage (a)	Yes
Section 23	Misuse of Drugs Act 1971	Drugs	Yes
Section 43	Terrorism Act 2000	Terrorism	Yes
Section 44 (b)	Terrorism Act 2000	Terrorism	No – not required in and around specific protected areas/places
Section 47	Firearms Act 1968	Firearms	Yes
Section 60	Criminal Justice and Public Order Act 1994	Anticipated violence	No
Section 139	Criminal Justice Act 2003	Offensive weapons – schools	Yes
Section163	Road Traffic Act 2003	Stops of vehicles	No (for stop) Yes (for search)

Notes:

(a) Searches for articles intended to commit criminal damage were added to the S1 PACE categories in the 2003 Criminal Justice Act.

(b) S44 searches were scrapped in July 2010 following a European Court of Human Rights ruling that they were illegal.

The subjectivity of any decision to stop and search an individual has raised questions about the use of the power (Quinton, 2011). An HM Inspectorate of Constabulary (HMIC) report (2013) examined police records of stop and searches and found that 27 per cent did not contain evidence of 'reasonable grounds' for the stop to take place.[1] A large number of UK-based studies on stop and search have focused on the extent to which the power has been disproportionately used against individuals from Minority Ethnic backgrounds (for example, Delsol and Shiner, 2006; Bowling and Phillips, 2007; Van Bueren and Woolley, 2009; Quinton, 2015). The latest published statistics on stop and search for England and Wales – covering the 12 months to end March 2015 – show that those from Black and Minority Ethnic (BME) groups were twice as likely to be stopped and searched as those who were White. In particular, those who were Black (or Black British) were more than four times more likely to be stopped than those who were White (Home Office, 2015). The continued greater likelihood for Black people to be subject to stop and search was one of the reasons that the Home Secretary launched a public consultation on stop and search in July 2013. The consultation received over 5,000 responses and the Government response, published in April 2014, put forward a number of proposals, including:

- to clarify what constitutes 'reasonable grounds for suspicion';[2]
- to commission the College of Policing to review training for stop and search;
- to introduce a voluntary 'Best Use of Stop and Search' scheme in which participating police forces record more details on the outcomes of stop and searches.

Amongst those who responded to the public consultation on stop and search, 61 per cent agreed that the powers were effective in preventing and detecting crime and anti-social behaviour. However, the research evidence on the effectiveness of stop and search on reducing crime is less clear-cut.

This paper examines the impact of an initiative to reduce knife crime in London – Operation BLUNT 2 – which involved a large increase in the number of weapons-related stop and searches.

Specifically, this paper sets out to:

- briefly review the current evidence on the effectiveness of stop and searches on reducing crime;
- describe the methodology to analyse the impact of a large increase in the use of stop and searches on crime in London in 2008 (Operation BLUNT 2);
- review trends in stop and searches and key crime types before, during and after Operation BLUNT 2; and
- summarise findings from regression analysis to explore whether the marked increase in numbers of stop and searches during Operation BLUNT 2 resulted in statistically significant falls in crime.

[1] *Stop and Search Powers: Are the police using them effectively and fairly?* HMIC, July 2013. http://www.justiceinspectorates.gov.uk/hmic/media/stop-and-search-powers-20130709.pdf

[2] This has resulted in changes to PACE Code A.

2. Evidence on the effectiveness of stop and search on reducing crime

There are several ways in which stop and search might bring about a reduction in crime (Miller *et al*., 2000):

- directly detecting offences that have been, or are about to be, committed;
- incapacitating prolific offenders as a result of the detection of crimes;
- deterring offences by raising the perceived risk of detection; and
- indirectly as part of wider initiatives promoting order maintenance (for example, through 'broken windows' type approaches).

However, the evidence base on the crime reduction effects of stop and search – either through these specific mechanisms or more generally – is limited.

Directly detecting offences through stop and search is relatively rare. Of the 539,788 stop and searches that took place in England and Wales in 2014/15 under Section 1 of the Police and Criminal Evidence Act 1984 (PACE) only 14 per cent resulted in an arrest. Of those Section 1 searches where the reason for the search was an offensive weapon, the arrest rate was 22 per cent. For the smaller number of stop and searches undertaken under Section 60 (in anticipation of violence), only 3 per cent of the 1,082 searches resulted in arrest[3] (Home Office, 2015). Comparatively low arrest rates for stop and search are also evident in the US. For instance the average arrest rate for 'stop, question, frisk' (SQF) in New York was 6.6 per cent between 2003 and 2010 (Rosenfeld and Fornango, 2014). Arrests arising from any form of stop and search also made up only 8 per cent of total arrests in England and Wales in 2014/15.

Miller *et al*. (2000) provide a basic estimate of the direct *reduction* in crimes as a result of stop and search arrests. By comparing what they define as 'disruptable' crimes with the corresponding number of arrests as a result of stop and search, their analysis suggests that stop and search could reduce the potential number of 'disruptable' crimes recorded by the police by 2.3 per cent.

The comparatively small proportion of stop and searches that yield an arrest is not, on its own, sufficient evidence to indicate that the use of the power is ineffective at reducing crime. It might lead to the arrest of serious or prolific offenders. Alternatively, the absence of arrests might in fact point to the success of the approach, that is it is actually discouraging would-be offenders from offending. But there is limited evidence on the pattern of offending of stop and search arrestees. Previous analysis of stop and search records in several divisions of the Metropolitan Police in 1998/99 found that most arrests arising from stop and searches were for less serious drugs offences, that is drugs possession rather than drugs supply (FitzGerald, 1999).

US evidence

Only a few research studies have explicitly sought to explore the link between stop and search and crime. One of the first, the San Diego field-interrogation experiment (Boydstun,

[3] Section 60 stop searches are, in any case, more of a preventative power.

1975) is one of the few to have a quasi-experimental design. The study examined the impact of field-interrogation (FI) – which has some similarities to stop and search in England and Wales – on crime and arrests.[4] Three matched areas were selected for the study:

- in one, FI continued as normal;
- in the second it was conducted only by officers who were given special training in conducting FIs;
- in the third it was suspended.

The area where FI was suspended showed a statistically significant increase in 'suppressible' crimes while the numbers of crimes did not change statistically significantly in either the control or the 'special' training FI area. The study was not able to identify changes for individual crime types. Although the study has limitations,[5] it suggests that some level of FI activity provides a deterrent effect – which could be described as an *absolute* deterrence effect – in a local area. But the study does not help to shed any light on the *marginal* impact on deterrence (that is whether increases or decreases in the use of FI or stop and searches have an impact on crime rates). Finally, since frequency of arrests was not influenced by changes to the level of FI activity, the implication is that arrest of suspects is not a key factor in the way FIs may influence crime.

Other supportive US evidence that some types of police-initiated contact may have a positive effect on crime levels comes from a study by Rosenfeld *et al.* (2014). They undertook a randomised control trial of police-initiated tactics in St Louis, Missouri. In this study greater targeted patrols were implemented in crime 'hotspots' in experimental areas, but in only one of the experimental areas was further 'police-initiated' contact encouraged. Non-domestic firearm assaults – but not firearm robberies – were statistically significantly reduced in the experimental area that encouraged police-initiated contact compared with the control areas. In addition, the findings suggested that these reductions were associated particularly with the use of occupied vehicles checks, rather than other police-initiated activities. Other US studies that have focused on initiatives to tackle the possession and use of illegal firearms often incorporate some element of police-initiated stop, for example, Koper and Mayo-Wilson (2012). These initiatives have also generally been found to be effective.[6]

Arguably the most relevant US analyses on the relationship between **increases** in stop and search and **reductions** in crime levels come from studies that have focused on its use in New York. In New York City the total rate of police stops (SQF) tripled between 2003 and 2010, while burglary and robbery rates fell by around a half between 2000 and 2010. This has encouraged speculation as to whether the increase in police stops was one of the main reasons behind the fall in these crimes. The first study, by Smith and Purtell in 2008 provided broadly supportive evidence that the crime fall had been influenced by increased rates of SQF. Their analysis used lagged monthly data to estimate the effects of stops on citywide crime trends for seven offence types, and in 'hotspot' precincts that had been the

[4] Defined as *"contact initiated by a patrol officer who stops, questions, and sometimes searches a citizen because the officer has 'reasonable suspicion' that the subject may have committed, may be committing, or may be about to commit a crime".*

[5] Suspension of FI took place in only one part of San Diego and some geographical displacement may have taken place. There may have been other reasons behind the change in crime rates such as less disruption of offenders or reductions in so-called 'order maintenance'.

[6] Koper and Mayo-Wilson undertook a systematic review of the evidence on what works in reducing illegal possession and use of firearms; four studies met the inclusion criteria. The authors concluded that directed patrols focused on illegal gun carrying did prevent gun crime, although generalisations on the basis of these studies were difficult. Several other studies cited pedestrian and in particular traffic stops as part of often wide-ranging interventions.

subject of an increase in the number of patrols. Their results were described as 'mixed', with statistically significant effects found for robbery, burglary, vehicle theft and homicide, but no effects for assault, rape or grand larceny.

A second study that examined the marginal deterrent effect of police stops on robbery and burglary rates, also in New York City (Rosenfeld and Fornango, 2014), was less positive in its assessment of the impact of SQF. The authors reviewed Smith and Purtell's analysis, and highlighted several methodological shortcomings. Most importantly, Rosenfeld and Fornango were critical of the way that Smith and Purtell's study addressed 'simultaneity' – that is, the fact that stops are themselves determined in whole or in part by crime rates – simply by lagging the stop rate one month behind the crime rate. This can produce spurious results. They were also critical of a failure to control for other factors, such as:

- deprivation, that might be correlated with changes in crime;
- not controlling for the impact of neighbouring precincts on crime rates;
- not incorporating period fixed effects; and
- not conducting a separate analysis of stops that resulted in arrest.

Rosenfeld and Fornango's own approach was to model the impact of SQF using 'dynamic panel models', which control for other factors that may affect crime rates. Their analysis focused on annual rates of robbery and burglary[7] in New York's 75 police precincts. Despite examining a range of measures, the study found few statistically significant effects of police stops on robbery and burglary rates. When the analysis was repeated to examine the relationship between SQF *arrests* and overall crime, this also failed to yield any statistically significant results. However, as the authors concede, the study is based on annual stop and recorded crime data, and at a fairly large geographic level – police precincts – so may not reveal any short-term, localised effects on crime. In short, the authors concluded that while they cannot be certain that SQF has no impact on crime, if it does, its impact is so localised and dissipates so rapidly that *"it fails to register in annual crime rates"* (Rosenfeld and Fornango, 2014, p 117).

The use of stop and search has also been associated with so-called 'broken windows' policing. The 'broken windows' theory is that signs of disorder attract further disorder and crime, so the police can play a key role in preventing a spiral of decline by focusing on minor disorder and less serious offences in areas that have not yet been overtaken by serious crime. While several papers claim to demonstrate a link between 'broken windows' policing and the fall in crime in New York and other US cities (notably Corman and Mocan, 2005; Sousa and Kelling, 2006; and Worrall, 2006) these claims are disputed (see Sampson and Raudenbush, 2004; and Harcourt and Ludwig, 2006). More pertinently, those studies that find an effect do not use the number of searches as an explanatory variable.

The most recent published work on examining the relationship between SQF and crime in New York has focused on what happens at very small geographic areas (Weisburd *et al.*, 2016). The analysis is based on geo-coded SQFs and non-traffic crime across a five-year period, 2006–11, when crime was falling and SQFs were increasing. The basic analytical approach is to assess the impact on the probability of crime happening in a small area (a street segment and its two intersections) in the week after an SQF has taken place. Once

[7] The authors selected these crime types as they were the two offences that Smith and Purtell had found were most affected by changes in 'stop, question, frisk' in their earlier analysis (2008).

controls are applied, the analysis shows a 2 per cent decrease in the probability of a crime in the week following an SQF in the Bronx. Effects were found to vary across New York City's five boroughs with Staten Island, Brooklyn and the Bronx having the larger effects. Overall it was estimated that the 686,000 SQFs undertaken in New York at their height would yield a reduction of 11,771 crimes at the city level, a 2 per cent decrease. The findings also suggest if that if there is a deterrence effect, the SQFs effect is localised and relatively short term. The effect generally ceases to have an impact on crime four days after the SQF, 300 feet from where it took place.

Apel (2016) has noted several weaknesses that arise from the limitations of Weisburd *et al.*'s data, notably the fact that SQF was just one of a multi-pronged strategy for preventing crime in New York City at this time. Weisburd *et al*. were unable to measure these other strategies. The use of 'all crime' data as the sole outcome variable is also an issue. No allowance is made for the fact that only a proportion of 'all crime' incidents is likely to be reasonably influenced by a potential SQF deterrence (for example, SQF would not be expected to deter most domestic offences). More importantly, SQF will itself uncover offences such as possession of drugs, weapons or stolen property. In other words, the execution of SQF would be expected to generate crime incidents that are covered by the outcome variable. It might be assumed that many of these are likely to be geo-coded and timed with identical coordinates to the SQF that uncovered the incident. Some 7 per cent of SQFs resulted in an arrest in 2010 (Rosenfeld *et al*., 2014), which suggests that SQFs will have 'identified' around 48,000 crimes in New York City in 2010.

UK evidence

In the UK there are several studies that have examined directly or indirectly the effectiveness of stop and search on crime. Penzer (unpublished, cited in Miller *et al*., 2000) assessed the *marginal* deterrent effect of stop and searches. Penzer examined the relationship between numbers of searches and recorded crime, lagged by one month, in London between 1993 and 1999. According to Miller *et al*.'s assessment, Penzer found a statistically significant negative relationship between searches and total crime. However, the relationship ceased to be statistically significant once an unusual shift in the figures in 1999 was allowed for. Between April 1997 and October 1999 there was a marked fall in use of stop and search by the Metropolitan Police. It has been suggested that the drop in stop and searches and the increase in crime at this time reflected a loss of confidence among the police following the publication of the report of the inquiry into the murder of Stephen Lawrence.

Stop and search might also have an impact on crime by it simply having a wider deterrent through the high profile deployment of police at crime 'hotspots'. There is an extensive body of evidence that suggests that focusing police resources in crime hotspots is effective at reducing crime (see, for instance, Braga 2005). However, one UK study of the relationship between the location of stop and search activity and the geographical concentration of crime indicates a far from perfect relationship. Some stop and search activity appears to take place in relatively low crime locations. Conversely some high crime locations appear to have relatively little stop and search activity (Chainey and Macdonald, 2012). However, this pattern may not be universal.[8]

[8] Analysis by Weisburd *et al*. (2014) found a very high correlation between 'stop, quiz, frisk' locations and crime hotspots in New York.

Finally, a detailed study of the execution of Operation BLUNT 2 at the local level was undertaken by O'Brien (unpublished master's thesis). This study focused on the impact on various measures of police recorded crime in 14 hotspot areas within 5 high crime boroughs, on those occasions when there was a marked increase in BLUNT 2 stop and search activity. Although there are some methodological issues with the main analysis, the study provides a helpful insight into the temporal impact of stop and search on police recorded crime close to where the activity is undertaken. On the days when there were five or more searches taking place, most areas recorded localised reductions in crime on the day those searches were undertaken, according to the main outcomes measure used. However, in the three days after the stop search activity, there were statistically significant increases in the hotspot area to levels that were comparable to the period before stop and searches were initiated. Local geographical displacement at the time of the increased stop search activity was not assessed in the study. Nor is it clear whether crime reduced in the short term because of a temporary increase in police visibility or explicitly because of an increase in stop and search activity.

Summary

The research evidence on the links between stop and search and crime reduction is limited and the few available, mainly US, studies offer mixed findings. Stop and search might reduce crime through several mechanisms – direct detection of offences, detection leading to the incapacitation of more prolific offenders and through general deterrence. Arguably the most robust analysis of the impact of a large increase in stop and search in New York City (Rosenfeld and Fornango, 2014) found that it contributed little to marked reductions in robbery and burglary offences. Nor did the authors find evidence of a relationship with crime if the analysis was limited solely to stops resulting in an arrest. Some level of stop and search activity, as opposed to no activity at all – particularly if focused in hotspot locations – might produce a local crime reduction effect, but it may dissipate quickly (Weisburd *et al*., 2016). While the impact of stop and search on specific crime types is unclear, US evidence on more wide-ranging initiatives to tackle illegal firearms – in which stop and search activity features – is a little more positive.

Finally, in a much broader sense, regardless of any direct crime reduction effect there is the potentially adverse impact on the public's willingness to help to tackle crime that may result from negative experiences of stop and search. See for instance Jackson's work on police legitimacy (2013).

3. Assessing the impact of Operation BLUNT 2 on crime

This study seeks to examine the impact of an initiative to tackle knife crime in London – Operation BLUNT 2 – which included a large increase in stop and search. In May 2008 Operation BLUNT 2 was introduced to combat knife crime, serious youth violence and most serious violence. Particular concerns existed over the increase in knife homicides in London involving juvenile victims. London knife homicides involving 10- to 19-year-old victims increased from 6 in 2003/04 to 19 in 2007/08 (Table 2).

Table 2: Metropolitan Police/City of London homicides involving a knife or sharp instrument, victims aged 10–19, 2003/04–2008/09

2003/04	6
2004/05	6
2005/06	8
2006/07	12
2007/08	19
2008/09	18

Source: Homicide Index

Operation BLUNT 2 was run as a central operation within the Metropolitan Police. It provided additional resources to particular operational command units across London.[9] According to a report to the Metropolitan Police Authority's Strategic and Operational Planning Committee (MPA, 2009), Operation BLUNT 2 comprised:

> • *"increased fixed and flexible search deployments and security measures to restrict knife carriage – educational establishments, entertainment and leisure venues, transport infrastructure and public space events;*
>
> • *intelligence-led stop and search operations targeting specific individuals, groups, areas, events, venues, town centres and transport hubs/routes;*
>
> • *targeted enforcement operations to disrupt and prevent violence perpetrated by violent individuals and groups (gangs);*
>
> • *disruption of knife supply through intelligence-led ground searches (neighbourhood weapon sweeps) and enforcement of existing retail sale supply legislation (including internet)."*[10]

Within Operation BLUNT 2, stop and search was seen by the Metropolitan Police to be *"central to the strategy of creating an environment that is hostile for those who choose to routinely carry lethal weapons in a public space"*.[11]

[9] Operational command units are organised on the same lines as London boroughs.

[10] Strategic and Operational Policing Committee, 8 June 2009, Operation BLUNT 2 Report by Temporary Assistant Commissioner Territorial Policing on behalf of the Commissioner. http://policeauthority.org/metropolitan/downloads/committees/sop/090608-20-exemptreport.pdf

[11] *Ibid.*

When first introduced in May 2008, BLUNT 2 had a total of £3.85m resources and 50 dedicated police officers. The additional manpower was divided into 'serials' – one sergeant with seven constables. The Metropolitan Police survey and spending data showed that on average 40 serials were deployed each week in the financial years 2008/09 and 2009/10. In 2010/11 the average weekly number of serials deployed fell to 28. By April 2011 BLUNT 2 was devolved to local areas, meaning that there was no longer any provision of central manpower for BLUNT 2 activities.

In addition to a large increase in weapon-focused stop and searches, other activities undertaken under the umbrella of BLUNT 2 may have had an impact on crime. These other elements, such as knife arches, ground searches and after-school patrols, are unlikely to increase crime,[12] but they might have a crime-reducing effect. As it has not been possible to separate the effect of an increase in stop and searches from the other components of BLUNT 2, the size of any crime-reducing effect identified would necessarily be an upper estimate of the crime-reducing impact of the increase in stop and searches under BLUNT 2.

During Operation BLUNT 2, London boroughs were divided into three tiers. BLUNT 2 resources were disproportionately targeted on Tier 1 boroughs, followed by Tier 2 boroughs, which were 'monitored closely'. Tier 3 boroughs were only required to implement local tactics, with learning from Operation BLUNT 2. Allocation of boroughs to tiers was based on a London-wide analysis of current intelligence.[13]

Table 3: Operation BLUNT 2 operational command units, by tier

Tier 1	Tier 2	Tier 3
Croydon	Barking and Dagenham	Barnet
Enfield	Brent	Bexley
Hackney	Ealing	Bromley
Haringey	Greenwich	Camden
Lambeth	Islington	Harrow
Lewisham	Wandsworth	Hammersmith and Fulham
Newham		Havering
Southwark		Hillingdon
Tower Hamlets		Hounslow
Waltham Forest		Kensington and Chelsea
		Kingston upon Thames
		Merton
		Redbridge
		Richmond
		Sutton
		Westminster

Source: Metropolitan Police

One of the main challenges of evaluating an initiative heavily reliant on an increase in stop and search is the issue of 'reverse causality'. In general, stop and search is used flexibly and responsively in response to crime. Although its use is not perfectly correlated with crime, it is likely to be most frequently used in areas with high crime rates, and its use will also be varied in response to changes in those crime rates. Areas with high urban land use,

[12] Perhaps with the exception of knife arches and weapon possession offences.
[13] http://policeauthority.org/metropolitan/downloads/committees/previous/eodb/080925-07-appendix01.pdf Accessed 30 October, 2015.

deprivation or a large youth population may have higher than average crime rates. As a result, these areas are more likely to have more stop and search activity. Simply comparing these areas with others that have low crime and low stop and search rates would therefore be misleading. In a similar vein, if recorded crime *increases* in an area, the police may respond by increasing the number of stop and searches in that area. If crime subsequently falls, the reduction in crime *could* appear to be the result of the increase in the number of stop and searches, but no such relationship may exist.

To address the problem of 'reverse causality', the impact of stop and search on crime needs to be isolated from other factors. To do this, a change in the use of stop and search that is largely unrelated to time-specific and area-specific changes in crime is required. Unlike the police's more responsive use of stop and search tactics in response to changing crime, making greater use of the power as part of a major policy initiative – such as with Operation BLUNT 2 – is likely to take longer to enact, and be generally more long lasting in its execution. The increase in its use is therefore less likely to be a direct response to changes in crime in one area in the same period. In this sense, Operation BLUNT 2 is a good candidate to evaluate the impact of stop and search on crime. The large increases in stop and searches that formed part of Operation BLUNT 2 were such that, unlike everyday variation in searches, it can reasonably be assumed that they were not driven by changes to crime in that area. For instance, in the period before the introduction of BLUNT 2, recorded robbery offences in Tier 1 boroughs were actually falling.

A second feature that makes Operation BLUNT 2 a good candidate for retrospective evaluation is that it was deployed variably across different London boroughs; 10 of the 32 boroughs accounted for the majority of additional weapons-focused stop and search activity.

Data

A panel dataset was used for this analysis. This contains monthly crime data for 32 London boroughs between 2004 and 2012 supplied by the Metropolitan Police. All boroughs contain the same number of observations for each variable. A panel dataset has particular benefits for this analysis, as it allows changes in crime levels in boroughs that received a large amount of BLUNT 2 resource to be compared with those that did not. Furthermore, a panel dataset enables the use of regression analysis that can control for factors that vary between boroughs, but that are constant over time (for example, the size of a borough). This enables the more effective isolation of the effect of stop and search in high increase (Tier 1) boroughs relative to other boroughs, whilst controlling for borough-specific characteristics.

A range of crimes were used as outcome or dependent variables. These are listed in Table 4. As BLUNT 2 was an intervention primarily intended to reduce serious youth violence and weapons-related offences, its effect should primarily be studied in terms of measures of street-based violent crime. The primary police recorded crime measures are therefore knife crimes (robberies involving knives, assaults involving knives, sexual assaults involving knives) and London Ambulance Service data on calls for assaults involving knives, guns and other weapons. Homicides involving sharp instruments were not analysed separately as there were simply too few cases. However, they are included as part of the generic category 'assaults with knives'. The issue of knife-related homicide is discussed separately in the section 8. In addition, a broader measure of robbery is included in the analysis – robbery

(personal and business) regardless of whether a knife was recorded as being used in the crime, given that it is street/public space offence.

For completeness, the analysis is also extended to three acquisitive crimes which, according to the research evidence, *might* be affected by higher levels of stop and search activity (burglary in a dwelling, theft of a vehicle and theft from a vehicle). Finally, police recorded crime data on weapon and drug possession were included in the analysis. Here a different effect might be expected. Given that weapon and drug possession are by and large offences recorded by the police as a result of their own activity, it might be expected that these offences increased in line with increases in stop and search activity. Analysing these two offence types might cast some light on the way in which changes in stop and search might influence offender behaviour.

There are limitations with using the recorded crime measures, since reporting of these crimes may be influenced by stop and search levels. Increased stop and search may change the willingness of citizens to report crimes. If, for example, community relations are damaged by stop and search then victims may be less likely to come forward. Conversely, any increase in police presence associated with more stop and search activity might provide more opportunities for victims to report crimes. In addition, police recorded crimes may be influenced by wider changes in recording.

To address these issues, the analysis also uses data on London Ambulance Service calls for assaults involving knives, guns and other weapons. Ambulance data should not be affected by either reporting or recording issues. Not all weapons-related assaults will result in ambulance call-outs. Some assaults will not come to the attention of the health services while others may be reported but will not involve the calling of an ambulance. The only way in which the number of stop and searches should be able to influence the number of weapons-related ambulance calls is by reducing the level of knife-related assaults. Hence ambulance data should represent a potentially robust outcome measure for this study.

Table 4: **Crime variables used in the analysis**

	Source	**Case for inclusion**	**Weaknesses**	**Tier 1 boroughs: 12 months data to end April 2008**	**Tier 1 boroughs: % change, 12 months to end April 2009 over 12 months to end April 2008**
Robberies involving knives (a)	Metropolitan Police data (police recorded offences)	Monthly borough-level data on a victim-based crime.	Crime recording by the police could be influenced by stop and search activity. Relatively small numbers.	3,150	-10.1%
Assaults involving knives (b)	Metropolitan Police data (police recorded offences)	Monthly borough-level data on a victim-based crime.	Crime recording by the police could be influenced by stop and search activity. Will include some domestic assaults that will not be influenced by stop and search. Small numbers.	2,710	-19.1%
Sexual assaults involving knives (c)	Metropolitan Police data (police recorded offences)	Monthly borough-level data on a victim-based crime.		650	-33.7%
London Ambulance call-outs for knife-related injuries	London Ambulance Service	Monthly borough-level data. Data are independent of any changes in police recording practices.	Data will include some assault call-outs that are not affected by stop and search activity (for example, domestic assaults). Relatively small numbers.	1,090	-5.6%
London Ambulance call-outs for all weapons-related injuries	London Ambulance Service			1,890	-4.1%

Robbery (personal and commercial)	Police recorded crime	Monthly borough-level data on victim-based street-based violent crime. Accounts for a high proportion of all knife related offences.	Crime recording by the police could still be influenced by stop and search activity. Includes non-weapon offences.	17,000	-13.8%
Burglary	Police recorded crime	Volume crime comparison	Crime recording by the police could be influenced by stop and search activity.	22,300	-1.3%
Theft from a vehicle	Police recorded crime			30,800	-4.8%
Theft of a vehicle	Police recorded crime			13,300	-16.2%
Weapon possession	Police recorded crime	Monthly borough-level data. May expect stop and search to affect (increase) offence levels directly.		3,430	-5.0%
Drugs possession	Police recorded crime	Monthly borough-level data. May expect stop and search to affect (increase) offence levels directly.		28,400	6.9%

Notes:

(a) Robbery involving knives is a subset of 'robbery – personal and commercial'.
(b) Comprising assault with injury, attempted murder, murder, threats to kill and wounding/acts endangering life.
(c) Comprising sexual assault and rape.

Stop and search data obtained from the Metropolitan Police are used to illustrate the level of BLUNT 2 activity in each of the three tiers. Table 5 shows the number of weapons searches in the three tiers in the year immediately before and the year immediately after Operation BLUNT 2 commenced. 'Weapons searches', as defined throughout this report, include

Section 1 Offensive Weapons (Police and Criminal Evidence Act 1984), Section 60 Anticipated Violence (Criminal Justice and Public Order Act 1994) and Section 47 Firearms (Firearms Act 1968) searches. Overall, Tier 1 weapons searches went up by 261 per cent in the first year of Operation BLUNT 2 compared with an increase of 82 per cent in Tier 3. The vast majority of the increase in stop and search under BLUNT 2 was due to Section 60 searches, which went up ninefold under the operation.

Table 5: Weapons-related stop and searches, by tier and search type, 2007–08 to 2008–09

	May 2007 – April 2008 (a)				**June 2008 – May 2009**			
Tier (b)	S1 PACE (c)	S60 CJPO (d)	S47 Firearms	**Total**	S1 PACE	S60 CJPO	S47 Firearms	**Total (% increase)**
1	22,933	8,796	2,425	34,154	39,484	81,620	2,231	123,335 (261%)
2	9,058	4,163	1,014	14,235	16,156	25,294	827	42,277 (197%)
3	15,265	5,396	1,328	21,989	24,420	14,307	1,365	40,092 (82%)

Source: Metropolitan Police

Notes:

(a) This table shows data for the 12 months before and the 12 months after May 2008. May 2008 is excluded because Operation BLUNT 2 started partway through the month.
(b) There were 10 boroughs assigned to Tier 1, 6 boroughs assigned to Tier 2 and 16 boroughs assigned to Tier 3. This table presents the totals, so the numbers do not clearly show the intensity of stop and search activity on a per borough basis. This is more clearly set out in the next section.
(c) Section 1 of Police and Criminal Evidence Act 1984.
(d) Section 60 of Criminal Justice and Public Order Act 1994.

The long-run trend in arrest rates for Section 60 searches for the Metropolitan Police is given in Annex C. The large increase in Section 60 searches that took place in 2008/09, coinciding with the first year of BLUNT 2, resulted in the arrest rate halving from 4 to 2 per cent. Of the 114,316 Section 60 stops in that year, 2,757 yielded an arrest. The arrest rate stayed at 2 per cent for the three years that the operation lasted.

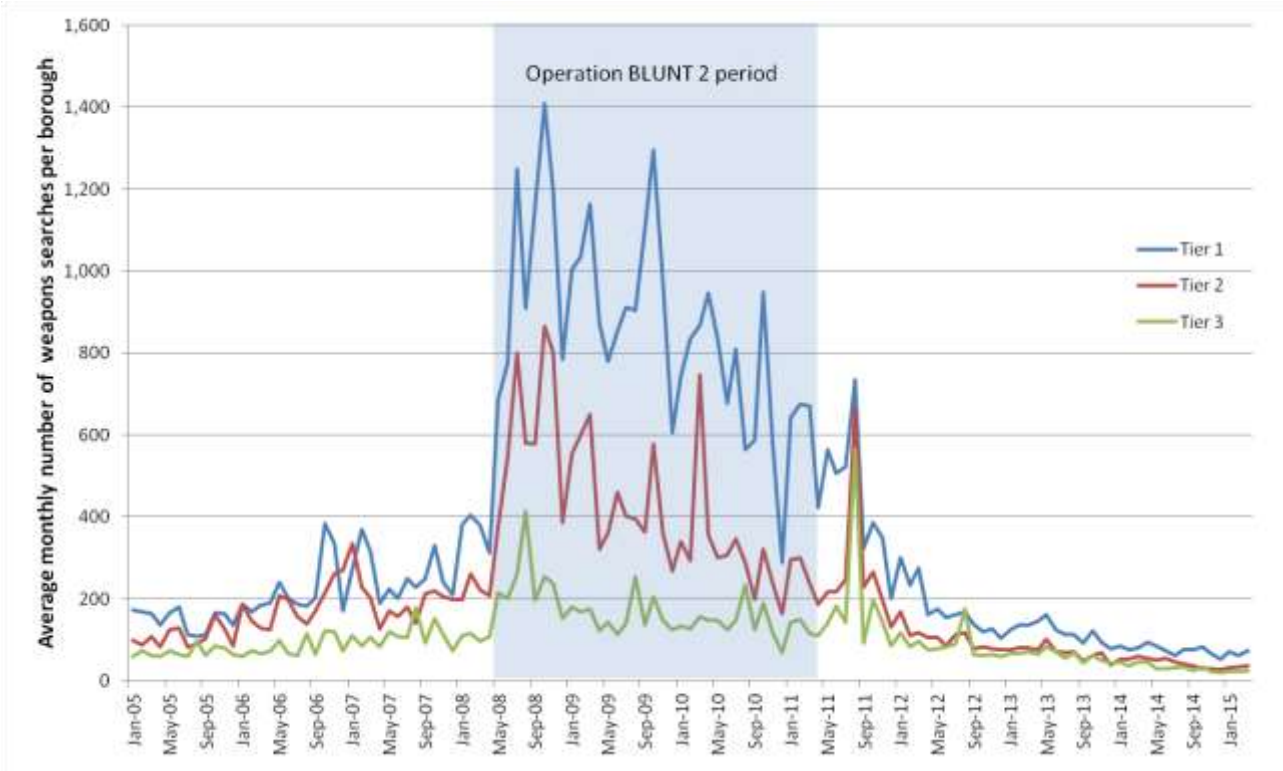
4. Methodology

The analysis that follows compares changes in crime levels in those areas that experienced large increases in weapons searches under Operation BLUNT 2 with areas that received a much lower increase. The former areas make up the 'treatment' group and the latter form the 'comparison' group. Establishing a comparison group is important to be able to estimate the counterfactual, that is what might have happened in the absence of a large increase in stop and search activity. Without a comparison group, there would only be data on London boroughs that experienced a large increase in stop and searches, so it would not be possible to conclude that an overall fall in crime in these boroughs was due to the increase in stop and searches. By establishing a comparison group, the *difference* between changes in crime levels in the 'treated' areas and the non-treated areas can be identified.

Operation BLUNT 2 and the increase in stop and search

As Table 5 shows London boroughs showed a sharp rise in weapons searches during Operation BLUNT 2, but this was not evenly distributed throughout London. BLUNT 2 resources were disproportionately focused on ten Tier 1 boroughs, and to a lesser extent on six Tier 2 boroughs. Figure 1 presents the *average* number of searches per borough for the three tiers, giving a clear indication of how BLUNT 2 resources were disproportionately focused on boroughs in Tier 1 and, to a lesser extent, Tier 2. Annex C presents the average number of non-weapons searches per borough for the three tiers.

Figure 1: Recorded monthly weapons searches, average per borough, by tier, 2005–15



Source: Metropolitan Police Service

In **Tier 1** boroughs, where the operation was concentrated, the average number of weapons searches per month during the BLUNT 2 period was over three times the average in the year before the operation. In the 12 months before the operation the total number of weapons searches in the 10 Tier 1 boroughs averaged at just under 290 per month. During the operation, Tier 1 weapons searches averaged nearly 900 per month, peaking at just over 1,400 in October 2008. In **Tier 2** boroughs, the average number of weapons searches increased by 115 per cent, from an average of just under 200 to an average of around 430 per month in the 6 boroughs. In **Tier 3** boroughs, there was a much smaller increase. The average number of weapons searches increased by 50 per cent from around 120 to an average of around 170 per month in the 16 boroughs.

Tier 1 boroughs saw the vast majority of additional stop and searches, and so these boroughs are assigned to the intervention group. Although Tier 3 boroughs experienced some increase in searches, this was far smaller in both relative and absolute terms than Tier 1, so these boroughs are assigned to the comparison group. In other words, the intervention effect examined is due to the difference between a very large surge in stop and searches and a small increase in stop and searches.

The appropriate treatment of Tier 2 boroughs is less clear. This is because the increase in stop and search activity was less intense than for Tier 1 boroughs, but more pronounced than for Tier 3 boroughs. As a result, three versions of the analysis were carried out. In the 'central' analysis, Tier 2 boroughs are grouped with Tier 1 boroughs as having been part of the intervention. However, Tier 1 boroughs are also compared with the grouping of Tier 2 and 3 boroughs. In the third version of the analysis, Tier 2 is excluded, so that the comparison is between Tier 1 and Tier 3 only, as the best and least resourced boroughs.

The assignment of boroughs to tiers by the Metropolitan Police was on the basis of intelligence on knife crime across London. However, an examination of police recorded crime and London Ambulance call-out data indicates that, measured in terms of the number of knife offences in each area, the tiers were quite mixed. This offers some reassurance for the validity of drawing comparisons between the tiers.

Each borough was ranked from the highest crime rate by population (1) to the lowest crime rate (32), for several crime measures[14] using 2007/08 crime data. Each borough's average rank was calculated. For example, Lewisham was 3rd for violent crimes, 6th for assault with a knife, 8th for ambulance call-outs for knife injuries and 11th for knife possession, giving an average rank of 7.

On this basis only three of the top five boroughs, in terms of knife crimes, were assigned to Tier 1, with one assigned to Tier 2 and the other to Tier 3. Of the top ten boroughs, six were assigned to Tier 1, two to Tier 2 and two to Tier 3. There is an association between the crime ranking and the tier assignment. While the average overall rank for Tier 1 boroughs was 9.2, for Tier 2 boroughs 13.7 and for Tier 3 boroughs 22, it was not the case that all Tier 1 boroughs were high crime areas, and all Tier 3 boroughs were low crime areas. A mix of boroughs existed within the tiers and this strengthened the comparisons across tiers.

[14] Ranked separately for: knife possession; violent crimes; assault with a knife; and ambulance call-outs for knife injuries.

The statistical analysis that follows uses individual borough-level data (rather than data aggregated to the tier) to establish whether there was a statistically significant relationship between changes in the number of stop-and-searches and crime levels.

The statistical modelling is based on a difference-in-difference approach. This relies on two differences being differenced in order to estimate the potential effect of an intervention. The basic principle is described below.

(1) The difference in crime levels in the 'treated' boroughs:
    a) during the 'treatment' (BLUNT 2 period);
    b) outside of the treatment period.
(2) The difference in crime levels in the non-treated boroughs (the comparison group):
    a) during the 'treatment' (BLUNT 2 period);
    b) outside of the treatment period.

By subtracting (1b) from (1a), the change in crime in each of the treated boroughs can be established (1).

By subtracting (2b) from (2a), the change in crime in each of the non-treated boroughs can be established (2).

By subtracting difference (2) from difference (1), the estimated effect of the treatment, or the surge in stop and searches can be established.

The approach does **not** make use of changes in the number of stop and searches as an independent variable. Instead it uses assignment to a tier as a proxy for whether or not a borough was 'treated' with a surge in stop and searches. No allowance is made for how intensive that 'treatment' was, that is how many searches were carried out in each borough.

In practice, the differences described above are not explicitly estimated and presented in this analysis, but the fundamental approach described here underpins the regression models presented below.

The analysis also controls for some socio-economic factors that are known to correlate with crime. A summary of the control variables used is given in Table 6.

Table 6: Control variables

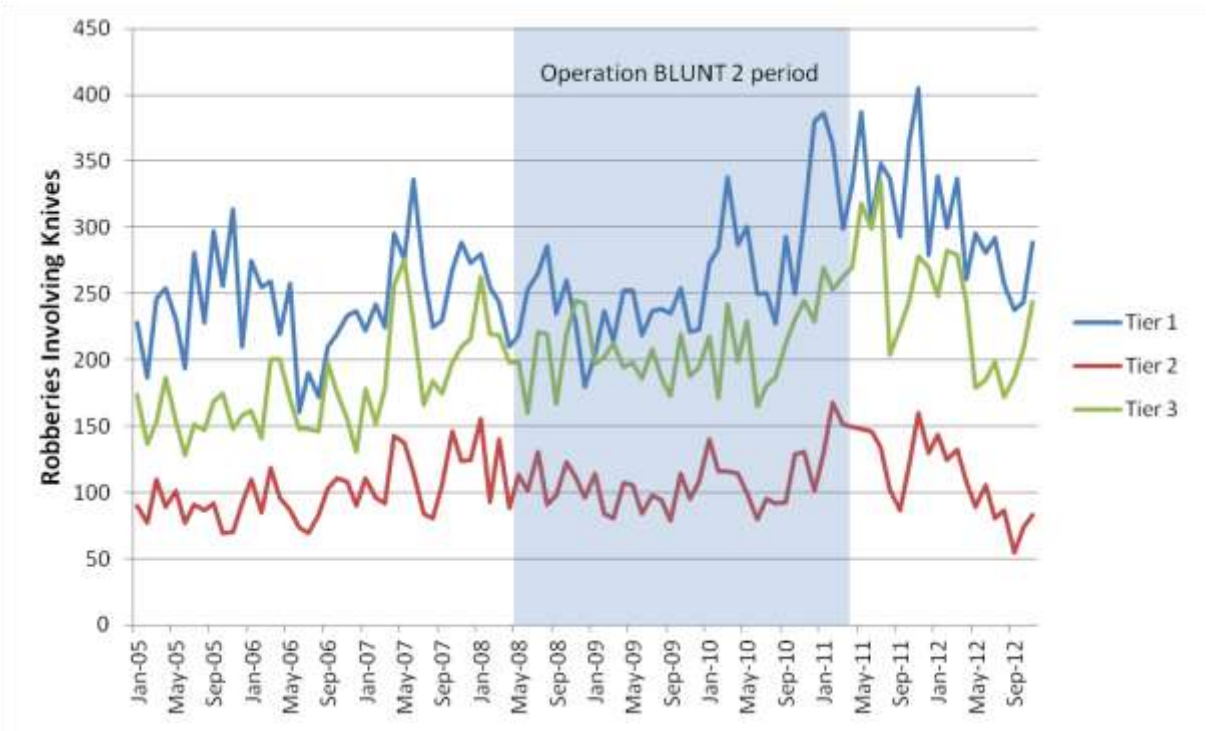
Geographic areas	Time period	Variables	Source
32 London boroughs	103 months Apr 2004 to Dec 2012	Mid-year population estimates and population density.	Office for National Statistics (ONS)
		Unemployment (measured by the proportion of the working age population on out-of-work benefits) Males aged under 25 out of work for more than one year.	Nomis Official Labour Market Statistics, ONS

5. Descriptive analysis

A valid comparison between intervention and comparison groups requires that the underlying trend in the outcome variables – crime – are similar for the different tiers outside of the intervention period. This is so that in the counterfactual case of no intervention it can be assumed that the variables would have followed a similar time trend, regardless of tier. Graphing the outcome variables for each of the tiers helps to show whether the trends pre- and post-BLUNT 2 are aligned across the tiers. This also enables an approximate comparison to be made, since a strong crime-reduction effect of a large increase in stop and searches should result in the Tier 1 crime trend falling visibly during the BLUNT 2 period, relative to the trend in the other tiers.

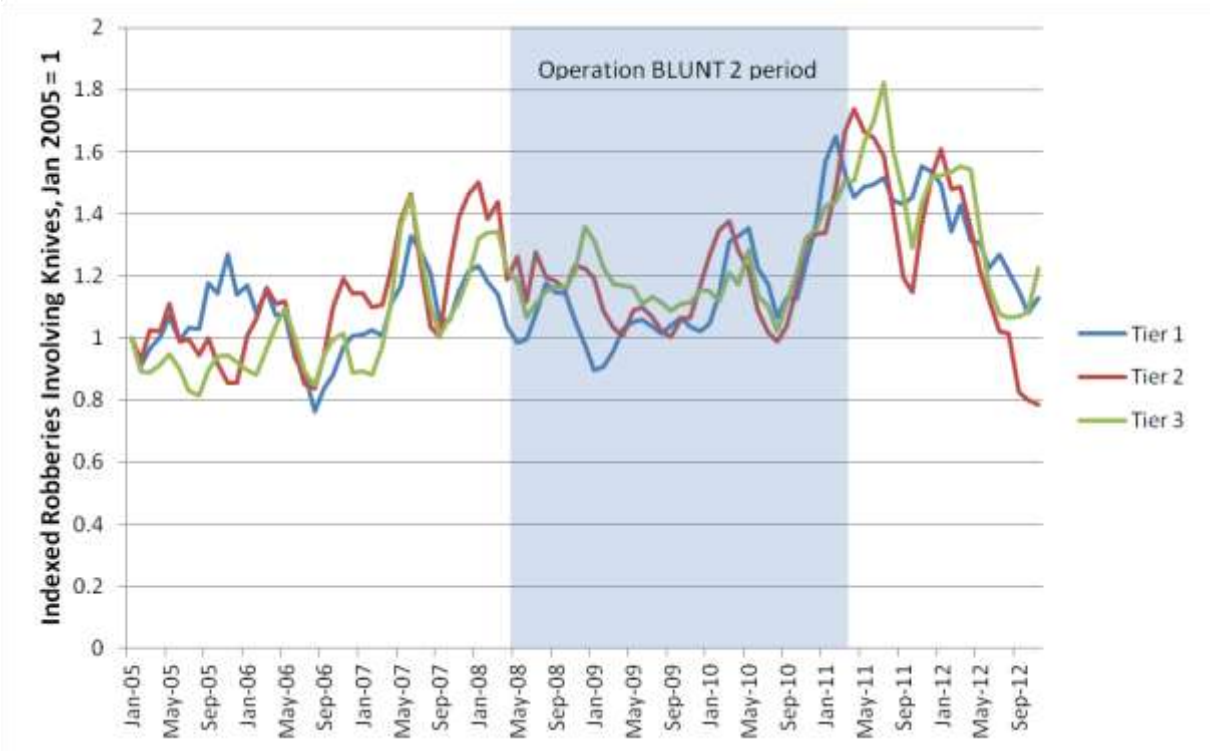
The first crime variable examined is robberies involving knives. Figure 2 presents the monthly data on knife robberies by tier, and Figure 3 gives an index showing the change in total knife robberies in each tier over time, in which three-month rolling averages are each presented relative to the January 2005 values.

Figure 2: Monthly robberies involving knives, by tier, 2005–12



Source: Metropolitan Police Service

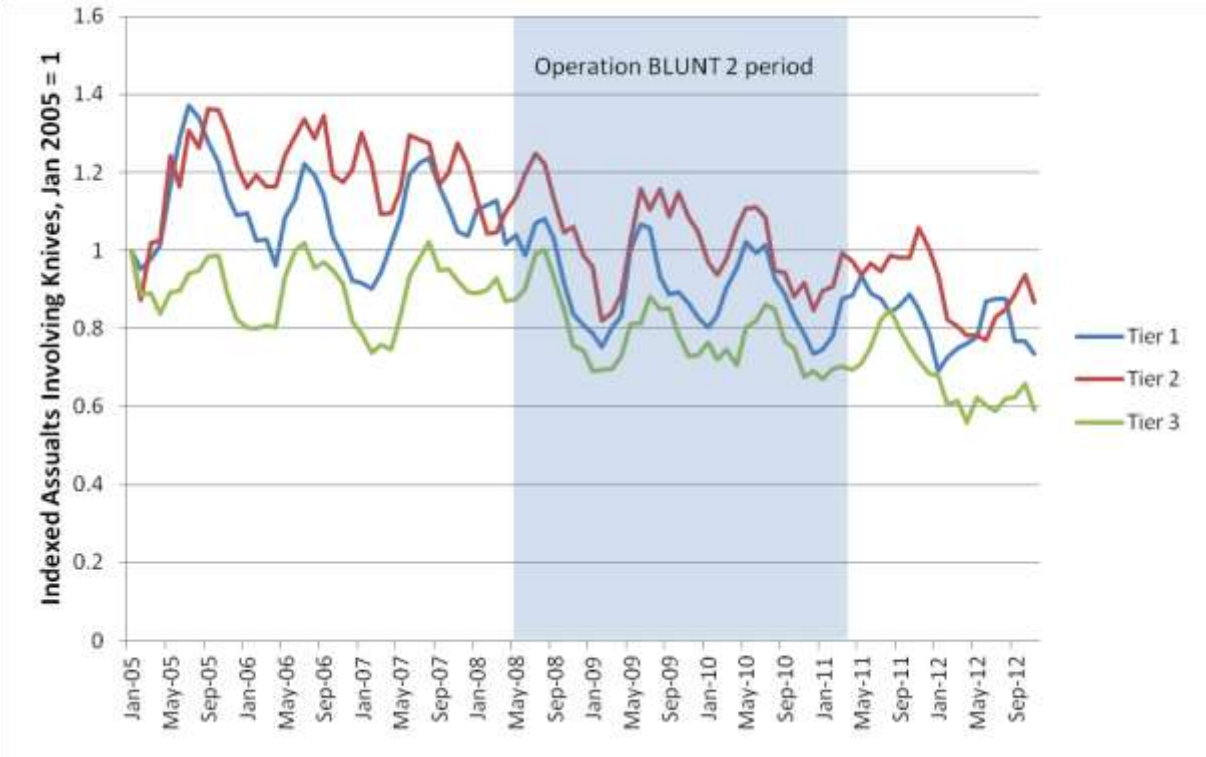
Figure 3: Indexed robberies involving knives, by tier, three-month rolling average, 2005–12



Source: Metropolitan Police Service

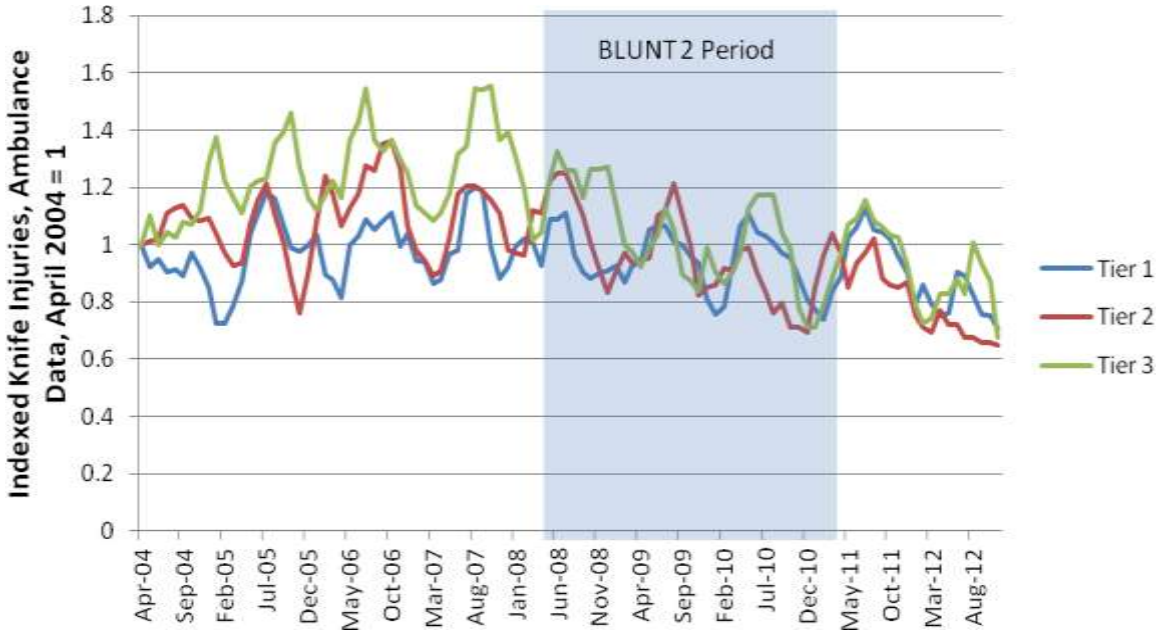
It appears that knife robberies in Tier 1 and Tier 2 might have been reduced, relative to Tier 3, in the first year of the intervention, but this pattern does not appear to persist throughout the operation. Whether or not this apparent initial crime reduction can be said to be a result of Operation BLUNT 2 is the subject of the more formal analysis below. As with most of the crime variables studied, there is evidence of seasonality, which the regression models will capture. Indexed rolling average graphs for some of the other main crime types are presented below, with the remainder presented in Annex B.

Figure 4: Indexed assaults involving knives, by tier, three-month rolling average, 2005–12



Source: Metropolitan Police Service

Figure 5: Indexed ambulance knife injuries call-outs, by tier, three-month rolling average, 2004–12

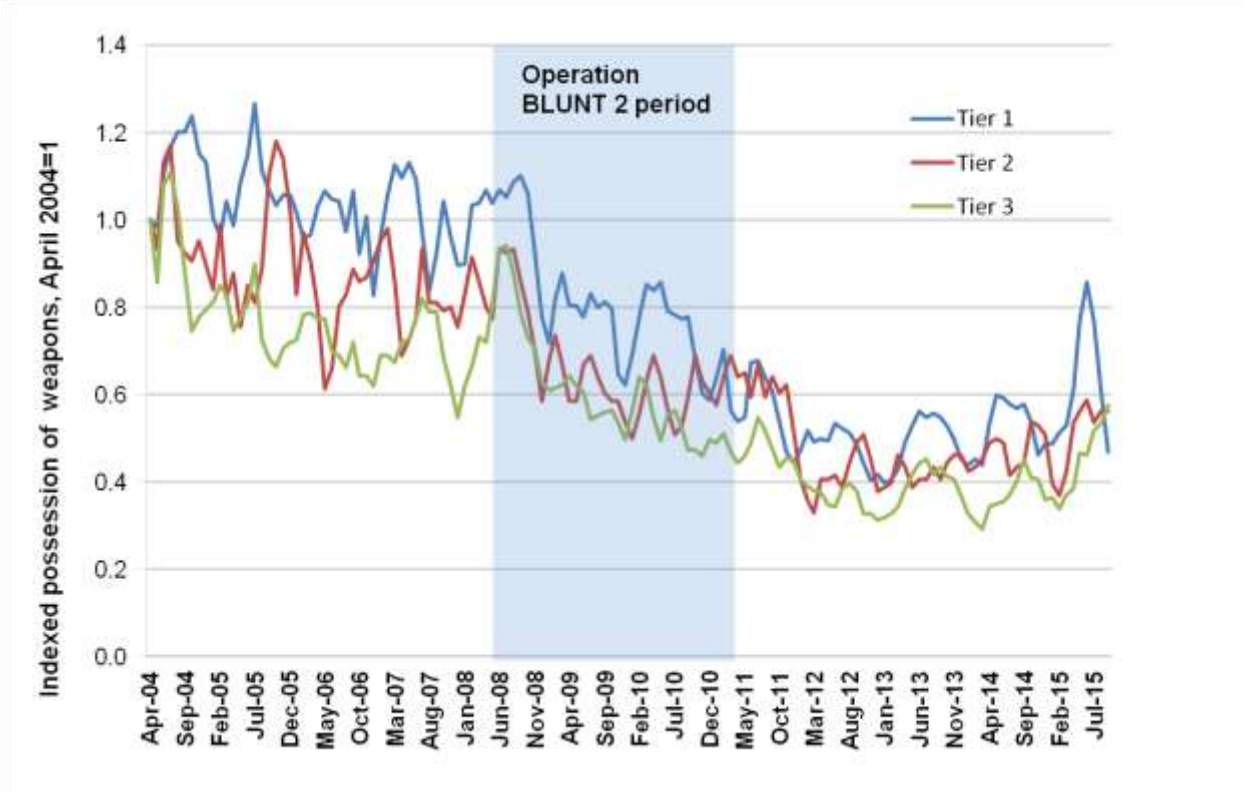


Source: London Ambulance Service

Figure 4 – assaults involving knives – shows a similar pre-BLUNT 2 trend for each of the tiers, each of which are fairly flat (ignoring in-year fluctuations) across the three years leading up to the operation. During Operation BLUNT 2 knife robberies appear to reduce, relative to the pre-2008 levels, but this appears to be the case for all of the three tiers.

Figure 5 shows the data for knife injuries requiring ambulance call-outs in the three tiers. The trends for the different tiers are fairly similar prior to the intervention, although Tier 3 boroughs appear to experience more upwards pressure prior to 2008, with a sustained increase relative to Tier 1 and Tier 2. The gap between Tier 3 and the other two tiers then closes during BLUNT 2, as the indices begin to overlap. This is the opposite effect to what might be expected if BLUNT 2 were to reduce knife crime in the areas where stop and search activity was most focused.

Figure 6: Indexed possession of weapons offences, by tier, three-month rolling average, 2004–15



Finally, it is helpful to examine what happened to possession offences during the BLUNT 2 period. Other things being equal, an increase in the recording of weapon possession offences might be expected as a result of Operation BLUNT 2. But Figure 6 shows a downward trend, regardless of tier, across the period as a whole. This does not appear to be substantially altered by the introduction of BLUNT 2. Only the noticeable peak in Tier 3 offences – in July 2008 – just after the start of BLUNT 2, is possible, but temporary, evidence of a BLUNT 2 effect. However, Tier 3 received little additional resources as a result of BLUNT 2. As before, there needs to be caution in over-interpreting these data.[15] Drugs

[15] Ward *et al*. (2011) examined possession offences in relation to the Tackling Knives and Serious Youth Violence Action Programme. They failed to come to a firm conclusion but note that enforcement activity and education activity could have been pulling in opposite directions.

possession offences increased markedly in late 2000s across all three tiers. Part of this general drift upwards was probably in part due to the existence of the 'Offences Brought to Justice' target, along with the combined effect of a general increase in all types of stop and search, including the large increase in Section 60 stops under Operation BLUNT 2 (see Annex C Table C.1).

A valid comparison between intervention and comparison groups requires that the underlying trend in the outcome variables is similar for the different tiers outside of the intervention period. Reviewing the data in this section and Annex B suggests this assumption can be supported, as there are generally no clear differences in pre-BLUNT 2 trends for the crime types examined. This examination has also provided an initial indication of whether crime rates for the three tiers diverged as a result of the differential increases in stop and searches during Operation BLUNT 2. The descriptive analysis provides little evidence for this.

6. **Statistical analysis**

Unconditional difference-in-difference regression

The starting point for the statistical analysis is a simple comparison of the difference in crime levels before Operation BLUNT 2 and during BLUNT 2 between those boroughs most affected by the operation and those less affected. This is called an unconditional difference-in-difference approach. The regression framework used to implement the difference-in-difference analysis using the panel data is set out below.

The starting point is the following model:

$$Crime_{it} = \beta_1 + \beta_2 Treated_i + \beta_3 During_t + \beta_4 (Treated * During)_{it} + \varepsilon$$

Equation 1

In this model *Treated* is a dummy variable, which is set to 1 if the observation is in the treatment (BLUNT 2 intervention) group and 0 otherwise. In the central analysis, Tier 1 and Tier 2 boroughs are assigned 1 for *Treated*.

The variable *During* is a dummy variable for the treatment period, which is set to 1 for all observations that occur during BLUNT 2 and set to 0 for observations outside that period.

The subscripts *i* and *t* are indices for borough and time respectively.

The parameter of interest is $\boldsymbol{\beta_4}$ – this measures the effect of the treatment (Operation BLUNT 2) on the treatment group, controlling for any changes that might have occurred in the absence of the intervention.

Conditional difference-in-difference regression

The model described above can be augmented to include a matrix of additional variables, $\boldsymbol{X'_{it}}$:

$$Crime_{it} = \beta_1 + \beta_2 Treated_i + \beta_3 During_t + \beta_4 (Treated * During)_{it} + \boldsymbol{\beta X'_{it}} + \varepsilon$$

Equation 2

The augmentation is carried out in the 'conditional difference-in-difference' analysis below. Here, additional variables are incorporated into the estimated model in order to control for factors that vary across boroughs and through time. The parameter of interest is always $\boldsymbol{\beta_4}$.

Given the specific data used in the model, a linear regression model with ordinary least squares (OLS) estimation is not the most appropriate model to use for estimating the size and significance of the BLUNT 2 'treatment effect'. Instead, a negative binomial regression model with maximum likelihood estimation is employed. There are three main reasons for this.

- The dependent variables (crimes) are discrete count variables, and for some crime measures the monthly borough values are small. They can only take on non-negative

integer values, whereas a key implicit assumption in OLS is that the dependent variable is continuous. The negative binomial regression model is most appropriate for modelling count variables.

- Across the various crime and ambulance outcome measures, the data display 'over-dispersion', which is where the variance of a variable exceeds its mean. For all of the crime and ambulance data, the conditional variance is greater than the conditional mean, and in many cases the variance is more than ten times greater than the mean. The negative binomial regression is most appropriate for modelling over-dispersed outcome variables.[16]

- When estimating the above equation using OLS, the residuals are very far from normally distributed – for most of the crime variables there is a strong positive skew.

In the section that follows, the results of different iterations of the negative binomial regression model are presented, with the parameter of interest ($\boldsymbol{\beta_4}$) the focus of the results. If the parameter is statistically significant and less than one, this would indicate that the increase in stop and searches under Operation BLUNT 2 in the intervention tier (or tiers) had a crime reduction effect on the outcome variable of interest.

In all of the models presented below, the dependent variable is simply the monthly count of the variable of interest, for example, knife robberies. To assist with interpreting the results, it is modelled in terms of incidence rate ratios (IRRs) – roughly, a ratio of crime rates, when controlling for other factors, for the 'treated' boroughs with the crime rates for those not 'treated' – on the exponential scale. This means that in the negative binomial models estimated, β_4 is the estimated incident rate ratio for the effect of the intervention.

This allows for the results to be interpreted as follows.

- If a London borough is subject to the intervention, such that it takes on the value 1 for *Treated* rather than 0, then that borough's crime rate (for example, robberies per month) during the intervention would be expected to change by a factor of β_4, while holding all other variables in the model constant.
- For example, if $\beta_4 = 0.9$ and is statistically significant (that is significantly lower than 1) then it might be concluded that the estimated effect of the intervention, holding all other variables in the model constant, was to *reduce* the crime rate by around 10 per cent in the boroughs where the operation was carried out, relative to the boroughs in the comparison group.
- Alternatively, if $\beta_4 = 1.1$ and is statistically significant then it might be concluded that the estimated effect of the intervention, holding all other variables in the model constant, was to *increase* the crime rate by around 10 per cent in the boroughs where the operation was carried out, relative to the areas in the comparison group.

In the central analysis the boroughs are assigned the 'treatment' – a surge in stop and searches under Operation BLUNT 2 – if they are in Tier 1 or Tier 2. The duration of the intervention also needs to be defined. This is defined as the first 2 years of Operation

[16] It has the same mean structure as Poisson regression and an extra parameter to model the over-dispersion.

BLUNT 2, the period when most central resources were allocated to BLUNT 2 (an average of 40 weekly serials, compared with 28 in the final year of the operation). The intervention period will be compared with the two years prior to BLUNT 2 (May 2006 – April 2008). As a robustness check, the regression model is also run on only the first year of the intervention (May 2008 – April 2009), when stop and searches were at their very highest.

7. Results

In this section the incidence rate ratios (IRRs) are presented for each of the crime types, together with p-values. A p-value below 0.05 would indicate that the null hypothesis, in which the IRR for the effect of Operation BLUNT 2 = 1 (no effect of the intervention), can be rejected with 95 per cent confidence. A statistically significant IRR below 1 would indicate a crime-reducing effect of the surge in stop and searches.[17] Table 7 shows the results of unconditional difference-in-difference regressions for six crime-type variables, and a combined crime variable, for the central case (where Tier 1 and Tier 2 boroughs form the treatment group, and Tier 3 boroughs make up the comparison group). The two possession offences are considered separately in this section, whilst the results for other outcome measures are presented in Annex A.

Table 7: Unconditional difference-in-difference regression results, Tier 1 and Tier 2 compared with Tier 3

Crime type	IRR for effect of BLUNT 2	P-value	95% confidence interval	
Knife robbery	0.960	0.555	0.840	1.098
Knife assaults	0.964	0.444	0.878	1.059
Knife sexual assaults	1.069	0.619	0.821	1.393
Ambulance – knife injury	1.181***	0.002	1.063	1.311
Ambulance – all weapons injury	1.107***	0.010	1.024	1.195
Robbery	0.951	0.347	0.855	1.056
Combined crime measure[18]	0.924	0.142	0.831	1.027

* Indicates statistical significance at 90 per cent confidence.

** Indicates statistical significance at 95 per cent confidence.

*** Indicates statistical significance at 99 per cent confidence.

The results here provide no evidence of a crime-reducing effect of a large increase in weapons searches. There is no result in which the IRR is statistically significantly less than 1 at the 95 per cent confidence level. The only statistically significant results are for the two ambulance call-out measures, but these are in the opposite direction to what might be expected if increased stop and searches were effective at reducing crime. This is in line with the descriptive data in Figure 5. Tier 3 ambulance call-outs during BLUNT 2 fall most in the boroughs that did not experience the large surges in stop and search activity. This is perhaps a counter-intuitive finding, but confirms the findings of the other variables analysed here. Overall these results show that without conditioning on other variables, no crime-

[17] To combat heteroskedasticity and serial correlation within boroughs, robust standard errors (clustered by boroughs) are used throughout.

[18] An additive index of police recorded robbery, burglary, assaults involving knives and sexual assaults involving knives.

reducing effect is found for the additional stop and search activity in Tier 1 and Tier 2 boroughs relative to Tier 3 boroughs.

As a robustness check, the results of the two alternative analyses are presented below, in which the treatment is assigned to Tier 1 boroughs only. The effect of the intervention in Tier 1 boroughs is measured firstly relative to the effect of the more modest increases in stop and search activity in Tier 2 and Tier 3 boroughs (Table 8), and then relative only to Tier 3 boroughs (Table 9).

Table 8: Unconditional difference-in-difference regression results, Tier 1 compared with Tier 2 and Tier 3

Crime type	IRR for effect of BLUNT 2	P-Value	95% confidence interval	
Knife robbery	0.992	0.917	0.861	1.144
Knife assaults	0.959	0.445	0.862	1.067
Knife sexual assaults	0.784***	0.000	0.692	0.888
Ambulance – knife injury	1.138***	0.023	1.018	1.272
Ambulance – all weapons injury	1.114**	0.018	1.020	1.218
Robbery	0.966	0.520	0.871	1.073
Combined crime measure	0.950	0.354	0.851	1.059

* indicates statistical significance at 90 per cent confidence.

** indicates statistical significance at 95 per cent confidence.

*** indicates statistical significance at 99 per cent confidence.

Table 9: Unconditional difference-in-difference regression results, Tier 1 compared with Tier 3

Crime type	IRR for effect of BLUNT 2	P-value	95% confidence interval	
Knife robbery	0.970	0.701	0.829	1.134
Knife assaults	0.954	0.418	0.851	1.069
Knife sexual assaults	0.982	0.855	0.804	1.120
Ambulance – knife injury	1.201***	0.003	1.066	1.352
Ambulance – all weapons injury	1.135***	0.004	1.042	1.236
Robbery	0.948	0.386	0.841	1.069
Combined crime measure	0.923	0.183	0.820	1.039

* Indicates statistical significance at 90 per cent confidence.

** Indicates statistical significance at 95 per cent confidence.

*** Indicates statistical significance at 99 per cent confidence.

For the first specification, with one exception, the results are similar to the main analysis, finding no statistically significant crime reduction effect. The exception is a statistically significant crime-reducing effect of the surge in searches under Operation BLUNT 2 found for sexual assaults involving a knife, when Tier 1 boroughs are compared with Tier 2 and Tier 3 boroughs.

Conditional difference-in-difference – results

In this section, the same difference-in-difference regression model is used, but it is augmented to control for factors varying across boroughs, over time, or both.

For each crime type, three specifications of the regression are considered, each building on the previous version, as follows.

(1) **Borough-specific fixed effects.** The unconditional model is extended by including a dummy variable for each borough. This is intended to account for any borough-specific characteristics that might have an effect on crime levels, but that do not vary with time.
(2) **Borough-specific and time-specific effects.** Specification (1) is extended by including dummy variables to account for any time-specific effects across the sample (month effects), for example to reflect seasonal variations in crime.
(3) **Other variables.** Specification (2) is extended by including a small number of regressors that vary both across boroughs and over time. These are:
    - population;
    - population density;
    - unemployment (measured by the proportion of the working age population on out-of-work benefits);
    - males aged under 25 out of work for more than one year.

The results are presented in Table 10 for the central case, in which Tier 1 and Tier 2 boroughs are assigned as the treatment, and Tier 3 boroughs represent the comparison group.

Table 10: Conditional difference-in-difference regression results, main outcome variables, Tier 1 and Tier 2 compared with Tier 3

Crime type	Regression specification	IRR for effect of BLUNT 2	P-value	95% confidence interval	
Knife robberies	(1) Borough effects	0.955	0.475	0.840	1.084
	(2) Borough and time effects	0.952	0.457	0.838	1.083
	(3) Borough, time and other	1.029	0.604	0.923	1.148
Knife assaults	(1)	0.956	0.358	0.869	1.052
	(2)	0.958	0.378	0.871	1.054
	(3)	1.006	0.897	0.916	1.105
Knife sexual assaults	(1)	1.101	0.464	0.851	1.426
	(2)	1.094	0.541	0.820	1.459
	(3)	1.230	0.175	0.912	1.661
Ambulance – knife injury	(1)	1.176***	0.002	1.062	1.304
	(2)	1.177***	0.002	1.062	1.305
	(3)	1.164**	0.013	1.033	1.312
Ambulance – all weapons injury	(1)	1.104**	0.012	1.022	1.193
	(2)	1.105**	0.012	1.023	1.193
	(3)	1.109**	0.040	1.005	1.223
Robbery	(3)	1.109**	0.040	1.005	1.223
	(2)	0.965	0.446	0.880	1.058
	(3)	0.985	0.759	0.895	1.084
Combined crime measure	(1)	0.919	0.141	0.821	1.028
	(2)	0.921	0.147	0.824	1.029
	(3)	0.945	0.244	0.859	1.039

* Indicates statistical significance at 90 per cent confidence.

** Indicates statistical significance at 95 per cent confidence.

*** Indicates statistical significance at 99 per cent confidence.

Again, no statistically significant effect is found for a crime-reducing impact of the intervention on any of the crime outcome measures. As with the unconditional model, knife injury ambulance call-outs were statistically significant, but in a positive direction (that is the rate of knife injury call-outs fell by less in Tier 1 and Tier 2 relative to Tier 3).

To further examine the robustness of these results, these regressions were also run for the two alternative approaches – in which Tier 1 boroughs are considered separately. No statistically significant crime-reduction effect is found under either alternative set-up. This is in contrast to the unconditional difference-in-difference results reported above – the statistically significant result found for sexual assaults involving a knife ceases to be so when conditioning on other variables.

The statistically significant reductions in both knife and all weapons ambulance call-outs in Tier 3 boroughs, compared with the Tier 1 and Tier 2 'treatment' boroughs, is worth discussion. This is something of a counter-intuitive finding since it shows that areas that were outside BLUNT 2 saw much more marked falls in knife crime. In other words, other factors influencing crime trends in Tier 3 boroughs were working to reduce knife crime, which were not evident in Tier 1. Tier 3 boroughs did, of course, see some increase in weapons searches but not on the scale of Tier 1.

One possible hypothesis is that a handful of boroughs were responsible for driving the statistically significant Tier 3 results, and these might have seen more marked increases in their weapons searches *compared with other Tier 3 boroughs*. Further robustness checks were therefore carried out. Firstly, the analysis was re-run to exclude the four Tier 3 boroughs that recorded the greatest increase in numbers of weapons searches within Tier 3 (Westminster, Kensington and Chelsea, Bromley and Camden). Between them, these boroughs accounted for more than half (57%) of Tier 3 increases in weapons searches (year 1 of BLUNT 2 compared with the 12 months before BLUNT 2). Taking the four boroughs out of the analysis had only a small impact on the results. Re-running the analysis with the trimmed group of Tier 3 boroughs still found that the reduction in knife and 'all weapons' ambulance call-outs was still statistically significantly greater than for Tier 1.

Although the coefficients reduce in size, the various conditioned results were still statistically significant (at the 95% level rather than 99%).[19] So on this basis, it might be concluded that there is no evidence to suggest that the reduction in Tier 3 knife crime was being driven by those boroughs that recorded marked increases in weapons searches. Instead there appears to have been a more widespread Tier 3 knife crime effect going on during the time of BLUNT 2. However, the possibility that stop and search did not in some way contribute to this fall cannot be excluded. All Tier 3 boroughs saw some increase in stop and searches, albeit from often very low per head levels, and with small increases compared with those recorded for Tier 1. It might also be that something changed in the way Tier 3 searches were carried out and that they became more intelligence-led. But equally it is possible that other factors unrelated to the police operations were driving down knife crime in these boroughs.

One final robustness check was undertaken to test whether a more extensive re-casting of the composition of the tiers – purely on the basis of the observed change in weapons searches, rather than on the basis of the Metropolitan Police's intelligence-led tier assignment – yielded different results. Four Tier 3 boroughs – Westminster, Kensington and Chelsea, Bromley and Camden – accounted for 57 per cent of the volume increase in Tier 3 weapons searches in the first year of BLUNT 2. Also two Tier 2 boroughs (Greenwich and Ealing) each experienced lower increases on the volume of weapons searches than any of those four Tier 3 boroughs. The revised 'treatment group' contained all of the Tier 1 boroughs, Tier 2 boroughs except Greenwich and Ealing, and the four additional Tier 3 boroughs (Westminster, Kensington and Chelsea, Bromley and Camden). The difference-in-

[19] For ambulance knife injury the coefficients (with p-values) are as follows when excluding Westminster, Kensington and Chelsea, Bromley and Camden: 1.11** (0.042) for specification 1; 1.11** (0.044) for specification 2; and 1.15** (0.024) for specification 3.

difference analysis was then applied to this set-up, comparing the changes in crime rates in the treatment group boroughs with changes in the comparison group boroughs.

Running the final conditional difference-in-difference specification yielded no new statistically significant results, nor did it affect the statistical significance of the results for ambulance call-outs. This gives further assurance about the robustness of the results, as it confirms that any crime-reduction effect at the borough level is unlikely to have been masked by boroughs that could be considered as outliers within their respective tiers.

The results in Table 10 above are based on a comparison of crime levels in the first two years of Operation BLUNT 2 compared with the two years prior to the intervention. However, as Figure 1 shows, stop and searches were not evenly distributed during the operation. The final robustness test involved running the regressions on only the first year of the intervention (May 2008 – April 2009), when stop and searches were at their highest,[20] and comparing this with the two years prior to the intervention. This tests whether there might have been an effect of BLUNT 2 during the period when stop and searches were at their highest. It also tests whether BLUNT 2 might have had a short-term impact that was not sustained over time, which might be obscured by running the analysis over the two years of the operation. Using the shorter time period revealed no change in the overall picture.

Finally, the regression was run for the two possession offences – possession of weapons and possession of drugs. Given that Operation BLUNT 2 focused on addressing knife offences, it would have been reasonable to expect that areas that had the largest increase in weapons searches would see a significant *increase* in weapon possession offences compared with comparison areas.

Only the results for the central analysis are presented, in which Tier 1 and Tier 2 boroughs are compared with Tier 3, although the results, in terms of statistical significance, are robust using the two alternative analyses. No statistically significant effects on possession of weapon offences – or drugs offences – were identified for Operation BLUNT 2.

Table 11: Conditional difference-in-difference regression results, possession offences, Tier 1 and Tier 2 compared with Tier 3

Crime type	Regression specification	IRR for effect of BLUNT 2	P-value	95% confidence interval	
Possession of weapons	Unconditional	0.931	0.138	0.846	1.023
	(1) Borough effects	0.926	0.122	0.840	1.021
	(2) Borough and time effects	0.927	0.125	0.842	1.021
	(3) Borough, time and other	0.932	0.181	0.840	1.034
Possession of drugs	Unconditional	1.002	0.981	0.878	1.142
	(1)	0.977	0.682	0.872	1.093
	(2)	0.974	0.672	0.862	1.100
	(3)	0.914	0.172	0.802	1.040

[20] For Tier 1 boroughs weapons searches averaged 1,020 per borough per month in the first year of the operation, 900 per month in the second year and 640 per month in the third year.

8. Limitations of this study

This study has sought to improve the evidence on the effectiveness of the stop and search evidence base using a quasi experimental-design built around a police operation that had some elements of 'natural experiment'. However, it is not without its limitations and these are summarised in this section.

Operation BLUNT 2 was wider than just stop and search. The focus of this analysis has been the relationship between increases in stop and searches and crime. Operation BLUNT 2 was characterised by the deployment of officers using stop and search powers resulting in large increases in weapons searches in Tier 1 boroughs. But the operation had other components, including knife arches, ground searches for concealed weapons, after-school patrols and some discrete anti-gang activities. Operation BLUNT 2 also sat on top of what might be considered 'normal' policing operations and was supplemented by the more wide-ranging initiatives of the Tackling Knives Action Programme II, which was in operation for much of the time that Operation BLUNT 2 was live.

It has not been possible to disaggregate the various elements of Operation BLUNT 2, but it is clear that stop and searches went up markedly in Tier 1 boroughs and that, overall, stop and search was a cornerstone of Operation BLUNT 2. An overview of the operation by the then Metropolitan Police Authority described stop and search as being *"central to the strategy of creating an environment that is hostile for those who choose to routinely carry lethal weapons in public space"* (MPA, 2009).

Operation BLUNT 2's use of Section 60 searches. Operation BLUNT 2 brought about a surge in weapons searches, but it was most characterised by a ninefold increase in Section 60 searches in Tier 1 boroughs. In year one of BLUNT 2, these accounted for two-thirds of all weapons searches (81,620) those boroughs. It is important to acknowledge that this form of stop and search is now only very rarely used. There were only 1,082 Section 60 searches in 2014/15 in England and Wales as whole. Section 60 stop and searches have a low arrest rate compared with other types of stop and search. See Annex C.

The statistical analysis assumes a uniform deployment, effectiveness and volume of stop and searches in the different tiers. In order to address the problem of 'endogeneity', the analysis has deliberately not allowed for variations in the effectiveness of deployment, targeting, or execution of stop and searches in the Tier 1 areas. Instead the analysis is based on a simple 'policy on' / 'policy off' categorisation of individual boroughs. In a similar vein, it does not allow an assessment of whether arrests from weapons searches is associated with falls in recorded crime. However, on this final point, Rosenfeld and Fornango (2014) tested various measures of stop and search arrests and found no significant effects on crime in New York.

The impact of recording and reporting effects on outcome measures. The importance of recording and reporting effects has been noted elsewhere in the report. The challenge around understanding the reporting effects of stop and search is that they are likely to be operating in both directions. The presence of police undertaking stop and searches may increase the opportunity for victims to report crimes, as well as increasing so-called discovery crimes. But stop and searches, if poorly handled, may discourage cooperation in the short and long term, and possibly reduce reporting rates. All of the recorded crime

outcomes potentially suffer from these weaknesses. Only the London Ambulance Service call-out data are not knowingly affected in this way, and these call-outs actually fell faster in those boroughs that had smaller increases in weapons searches.

Geography. There are two separate issues around the question of geography. First, the allocation of boroughs to tiers. While a good number of high violent crime boroughs feature in Tier 1, the Metropolitan Police's original allocation of boroughs to tiers was based on a London-wide analysis of then 'current' intelligence. Consequently, some high violence / high knife crime boroughs were allocated to Tier 2 and Tier 3, and some low violence / knife crime boroughs were allocated to Tier 1. Arguably there is more diversity in the allocation of boroughs to tiers in terms of their violent crime profiles. This aspect of the Metropolitan Police's allocation process strengthens the study design, since the tier profiles are more heterogeneous than might be expected.

The second geographical issue is more intractable. There is some evidence from other studies that if stop and search does have an effect on crime it is highly localised and short term in its impact (e.g. O'Brien, Weisburd *et al* 2016). The use of borough-level crime data means that any localised effects of stop and search may not be fully captured in the results. Likewise, using monthly data may hide short-term reductions in crime.

Crime-type issues and the question of homicide. A total of 11 outcome measures – specific police recorded crimes or ambulance call-outs – were included in the regression analysis. Since it was weapons stop and searches that increased so markedly under BLUNT 2, the most appropriate outcome measures are a small number of weapons-linked violence offences, and one broader violence offence (robbery). Three property crime offences were included simply to ensure that any collateral crime effects were not overlooked. Two possession offences were also included.

One problem that complicates any interpretation of the relationship between changes in weapons-focused stop and searches and changes in violent crime is the fact that not all violent crimes take place on the street, and some will therefore not be susceptible to the impact of a stop and search – assuming that this police power is 'effective'. According to the 2013/14 Crime Survey for England and Wales (CSEW), 25 per cent of all violent incidents took place in and around the home, 19 per cent took place at work, 22 per cent in the street, 19 per cent in or around a pub or club and 15 per cent elsewhere (Home Office, 2015). Arguably the most robust – though not the most numerous – recorded crime measure used in this study is 'robbery involving a blade or pointed object'. This is likely to be almost exclusively an on-street offence. However, as with all the recorded crime measures, these are susceptible to recording and reporting changes. The only measure that may be immune from reporting and recording issues is London Ambulance Service call-outs, but the publicly available data offer no details on the nature of the offence.

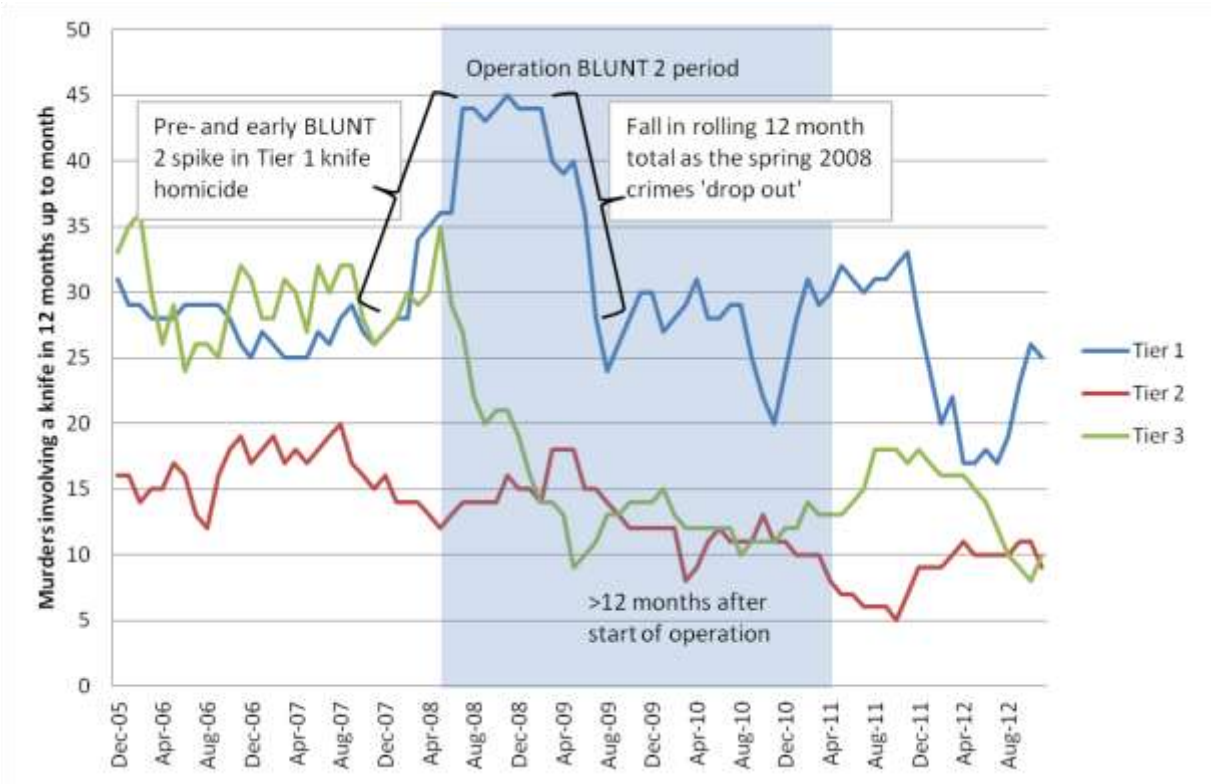
While homicide offences were included under the generic category 'assaults with knives', they were excluded as a discrete crime variable. Although it is possible to identify homicides involving knives, the small numbers of offences makes it difficult to generate meaningful results. If BLUNT 2 did indeed have an effect on knife homicides in Tier 1 boroughs, this might provide a sufficient justification for an increase in stop and searches.

Figure 7 shows rolling 12-month trends in homicides involving a knife or sharp instrument by tier. The figures cover victims of all ages. Several points are worth making. First, although Tier 1 and Tier 3 cover different numbers of boroughs (10 and 16 respectively), up to April 2008, they had broadly similar 12-month rolling totals for knife homicides. In Tier 1, there was a sharp increase in the rolling 12-month knife homicide figures in the run up to the operation starting. This may well help to explain why these boroughs were allocated to Tier 1.

During the operation, the number of knife homicides in Tier 1 returned to historic levels. The apparent sharp fall in the spring of 2009 largely reflects the 'dropping out' of the pre-BLUNT 2 increase from the 12-month rolling total. Tier 3 shows a different pattern – in the years leading up to Operation BLUNT 2 knife homicides were relatively flat, but then fell sharply in the first year of the operation, and remained at a lower level thereafter. Finally, the trend in knife homicides by tier is quite different from the patterns observed for, say, assault with injury. This is likely to be partly a function of small numbers of knife homicides and hence their volatility.

Difference-in-difference analysis cannot be used on such small numbers, so any observations are necessarily tentative. Given that both Tier 1 (high resource) and Tier 3 (low resource) boroughs saw reductions in knife homicides, albeit at different times, it seems unlikely that the falls in Tier 1 boroughs can be attributed to increases in weapons searches under Operation BLUNT 2. But it cannot be ruled out, and on the question of knife homicides the findings are less conclusive.

Figure 7: Knife homicides, by tier, 12-month rolling totals, 2005–2012



9. Concluding observations

Stop and search is a well-established police power, but the research evidence on its effectiveness at reducing crime is less solid. Research on the links between stop and search and crime reduction is limited and the few studies available, mainly in the US, offer mixed findings. The evidence base suggests that some level of stop and search activity, as opposed to no activity at all – particularly if used in hotspot locations – might produce a localised crime reduction effect.

Despite its limitations, this study is able to shed some light on whether the large increase in stop and searches, which were a central part of Operation BLUNT 2, had a discernible effect on knife-crime volumes at the borough level. If an increase in the number of weapons searches is effective for reducing crime then a drop in knife-related offences would be expected in those areas where the number of stop and searches increased the most compared with areas that had smaller increases in stop and search activity. A conditional difference-in-difference regression analysis found no statistically significant crime-reduction effect across 11 offence types from the increase in weapons searches, when comparing boroughs with the biggest increases in stop and search activity with those that had much smaller increases. Perhaps the only exception to this general statement is around homicide, where the small numbers of offences make it difficult to come to a definitive view.

Using borough-level data, with an average population of over 200,000, is not the ideal geography for analyses of the impact of stop and search on crime, especially if the stop and search activity is concentrated in localised crime ‘hotspots’, as in the case of Operation BLUNT 2. So it is possible that there are localised crime-reducing effects of stop and search activity that are masked when analysing data on a large geographic area.

It does not necessarily follow that stop and search activity does not reduce crime. It is possible that a certain ‘base level’ of stop and search activity has an effect, after which there are diminishing (or even zero) returns. This current study has not been able to shed light on what that level would be, or if such a level even exists. What this analysis does show, however, is that there was no discernible crime-reducing effect at the borough level from a large surge in stop and search activity in London during 2008–11.

Regression results for other crime measures

The results for crime measures that are not presented in the main body of the paper are presented in Tables A.1 to A.4.

Table A.1: Unconditional difference-in-difference regression results, Tier 1 and Tier 2 compared with Tier 3

Crime type	IRR for effect of BLUNT 2 (a)	P-value	95% confidence interval	
Domestic burglary	0.936	0.345	0.816	1.074
Theft of vehicle	0.949	0.275	0.864	1.043
Theft from vehicle	1.080	0.169	0.968	1.205

Notes:

(a) IRR = Incident Rate Ratio

* Indicates statistical significance at 90 per cent confidence..

** Indicates statistical significance at 95 per cent confidence.

*** Indicates statistical significance at 99 per cent confidence.

Table A.2: Unconditional difference-in-difference regression results, Tier 1 compared with Tier 2 and Tier 3

Crime type	IRR for effect of BLUNT 2	P-Value	95% Confidence interval	
Domestic burglary	0.972	0.696	0.845	1.119
Theft of vehicle	0.967	0.410	0.894	1.047
Theft from vehicle	1.085	0.158	0.969	1.214

* Indicates statistical significance at 90 per cent confidence.

** Indicates statistical significance at 95 per cent confidence.

*** Indicates statistical significance at 99 per cent confidence.

Table A.3: Unconditional difference-in-difference regression results, Tier 1 compared with Tier 3

Crime type	IRR for effect of BLUNT 2	P-Value	95% confidence interval	
Domestic burglary	0.945	0.459	0.814	1.097
Theft of vehicle	0.949	0.303	0.860	1.048
Theft from vehicle	1.100	0.138	0.970	1.248

* Indicates statistical significance at 90 per cent confidence.

** Indicates statistical significance at 95 per cent confidence.

*** Indicates statistical significance at 99 per cent confidence.

Table A.4: Conditional difference-in-difference regression results, main outcome variables, Tier 1 and Tier 2 compared with Tier 3

Crime type	Regression specification (a)	IRR for effect of BLUNT 2	P-value	95% confidence interval	
Domestic burglary	(1)	0.929	0.315	0.804	1.073
	(2)	0.929	0.314	0.805	1.072
	(3)	0.943	0.341	0.836	1.064
Theft of vehicle	(1)	0.949	0.222	0.872	1.032
	(2)	0.949	0.233	0.871	1.034
	(3)	0.960	0.451	0.864	1.067
Theft from vehicle	(1)	1.063	0.260	0.956	1.091
	(2)	1.062	0.265	0.956	1.180
	(3)	1.086	0.084	0.989	1.192

Notes:

(a) The three regression specifications are the same as described in Section 7.

* Indicates statistical significance at 90 per cent confidence.

** Indicates statistical significance at 95 per cent confidence.

*** Indicates statistical significance at 99 per cent confidence.

Descriptive analysis of other crime measures

Figure B.1– thefts of a vehicle – shows a very similar trend for all three tiers prior to the intervention. During the Operation BLUNT 2 period the indexed number of thefts in Tier 1 and Tier 2 appear to open up a sustained gap below the indexed number of thefts for Tier 3, perhaps suggesting some crime-reduction effect of the additional stop and searches. Whether this is a statistically significant crime-reduction effect of the operation is tested through the formal difference-in-difference analysis, with the results presented in Annex A.

Figure B.1: Indexed thefts of a vehicle, by tier, three-month rolling average, 2004–12

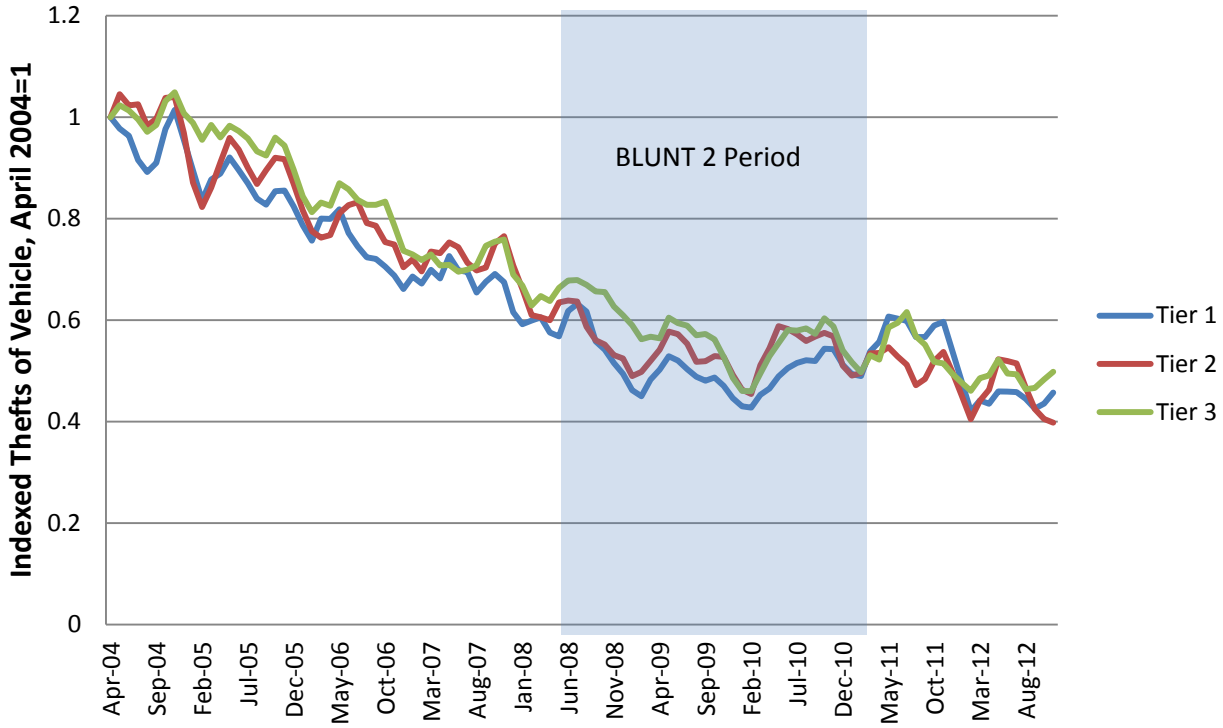


Figure B.2 – thefts from a vehicle – shows roughly similar trends in the years prior to BLUNT 2. However, there is a divergence in the year immediately before the intervention, as thefts in Tier 1 and Tier 2 gradually increase relative to Tier 3. Thefts in Tier 2 boroughs fell rapidly just before and during the first months of Operation BLUNT 2, whilst Tier 1 thefts remained at an elevated level. If it is the case that Tier 1 boroughs had a larger (less negative) underlying time trend for thefts from vehicles prior to BLUNT 2 then a difference-in-difference model might *underestimate* any crime reduction effect of the increase in stop and searches.

Figure B.2: Indexed thefts from a vehicle, by tier, three-month rolling average, 2004–12

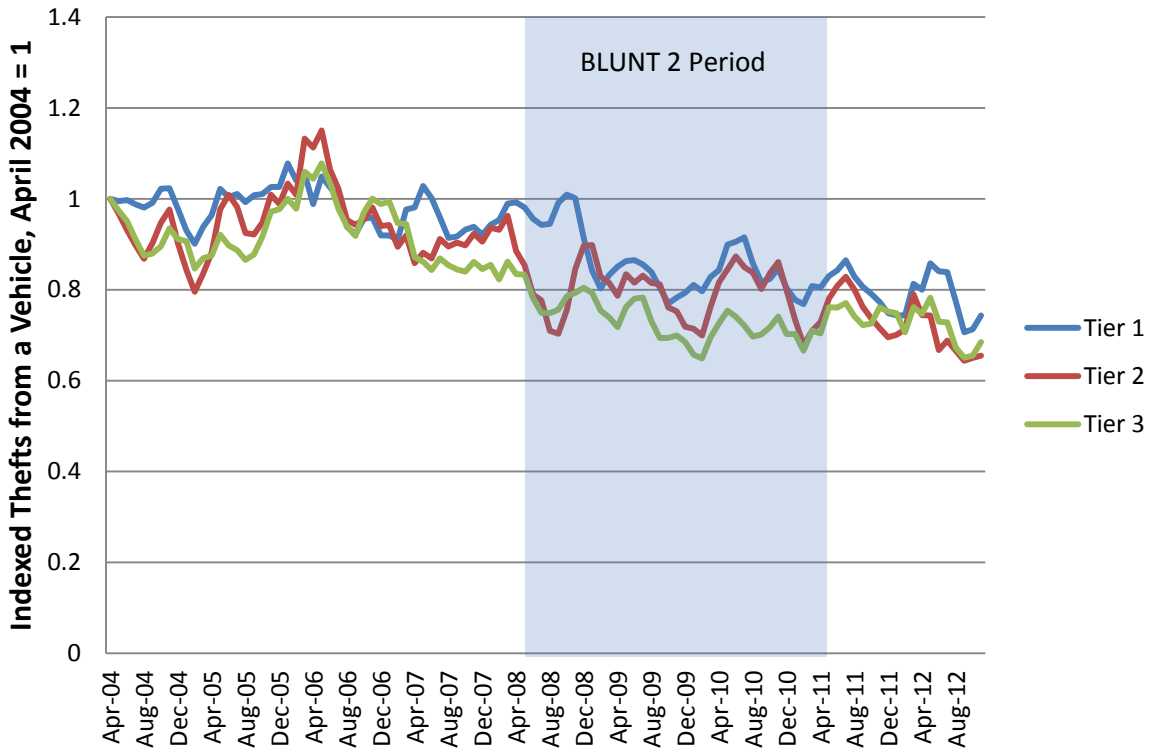
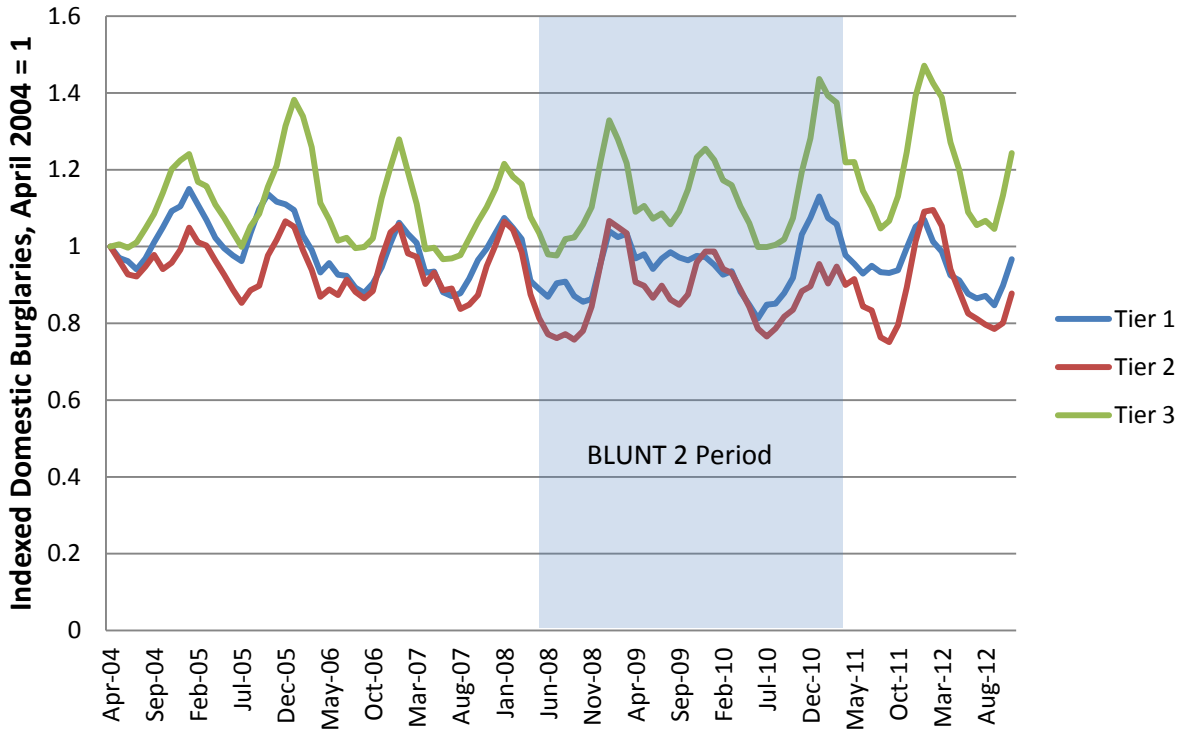


Figure B.3 – domestic burglary – shows a very different picture. Tier 3 had an upward trend prior to the BLUNT 2 period relative to that for Tier 1 and Tier 2. There is therefore a risk that in a difference-in-difference model any burglary-reducing effect of the intervention could be *overstated*. This would be because burglaries in the counter-factual (non-intervention) areas might have remained lower anyway, relative to the comparison group, simply due to different underlying trends. This needs to be considered when interpreting the difference-in-difference results for this crime type. The figure also presents strong evidence of seasonality, which is therefore accounted for in the difference-in-difference regression models.

Figure B.3: **Indexed domestic burglaries, by tier, three-month rolling average, 2004–12**



Figures B.4 and B.5 give charts for robbery and drugs possession.

Figure B.4: Indexed robberies, by tier, three-month rolling average, 2004–12

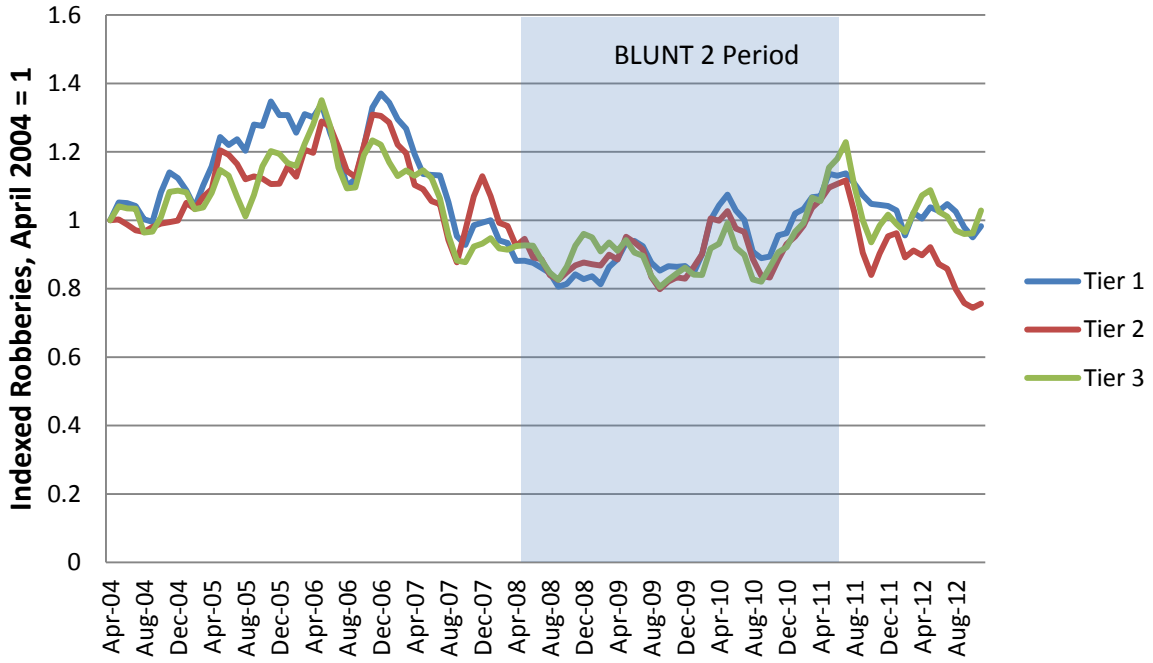
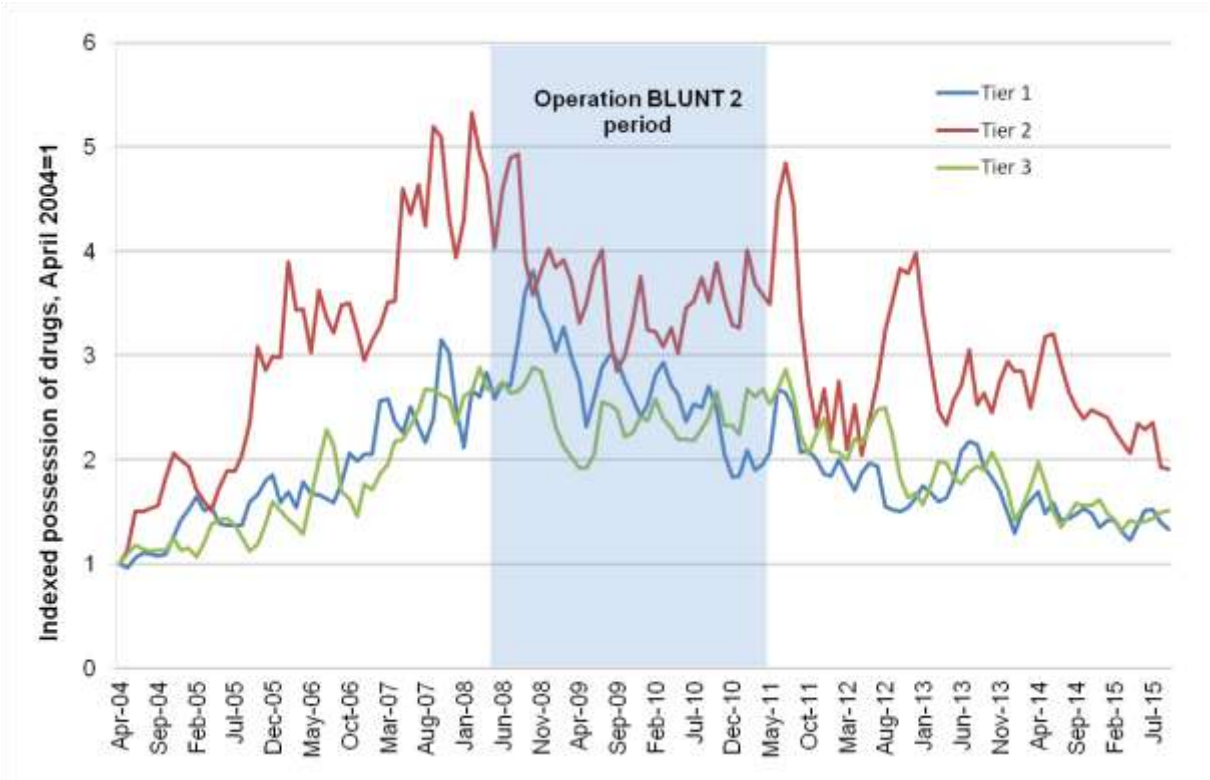


Figure B.5 Indexed possession of drugs offences, by tier, three-month rolling average, 2004–15



Stop and search volumes and arrest rates

Figure C.1: Recorded monthly non-weapons searches, borough average, by tier

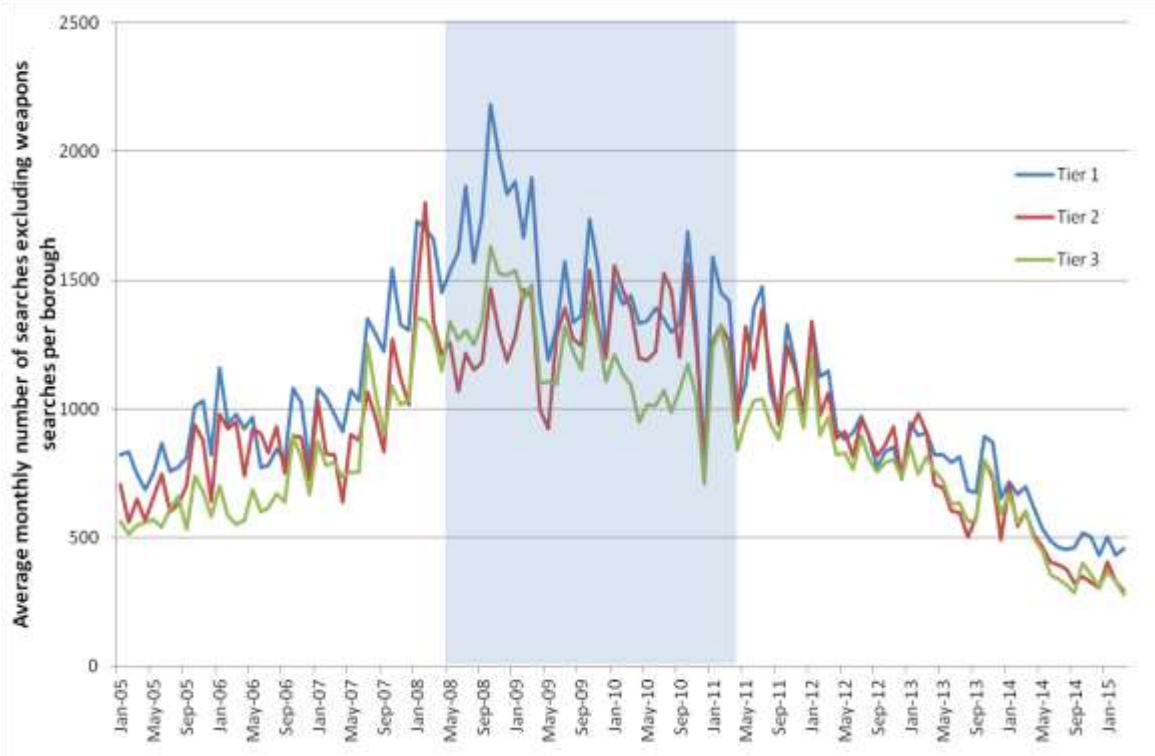


Figure C.2: Recorded monthly searches in London, weapons searches compared with all other searches

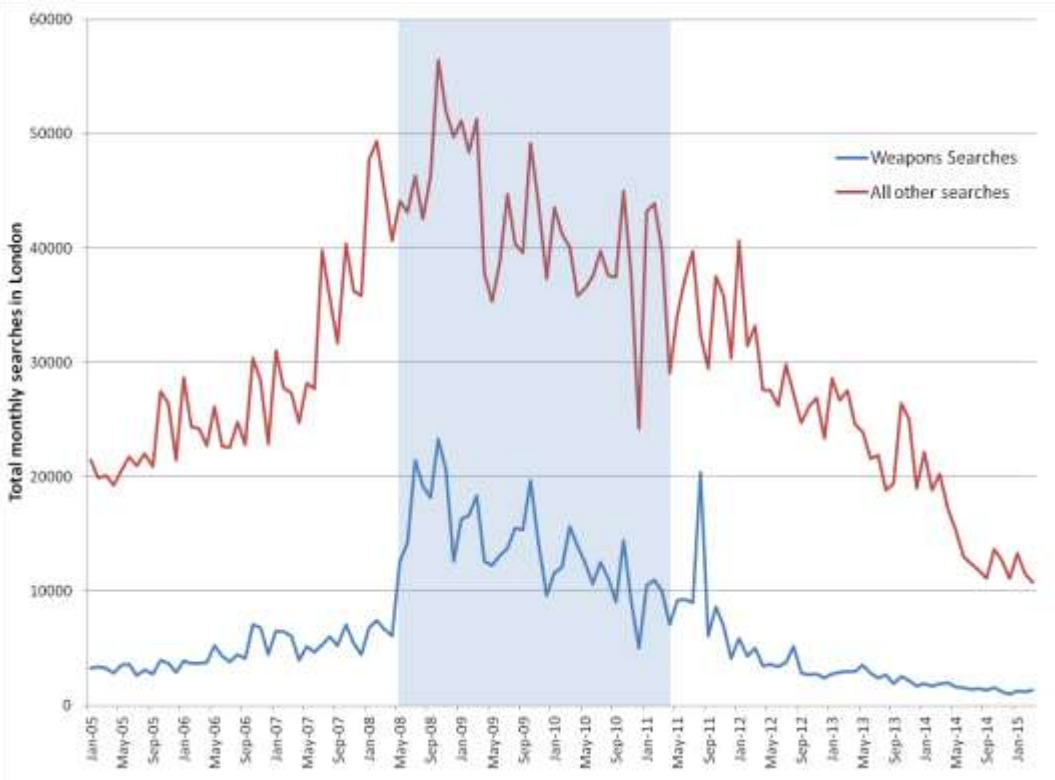
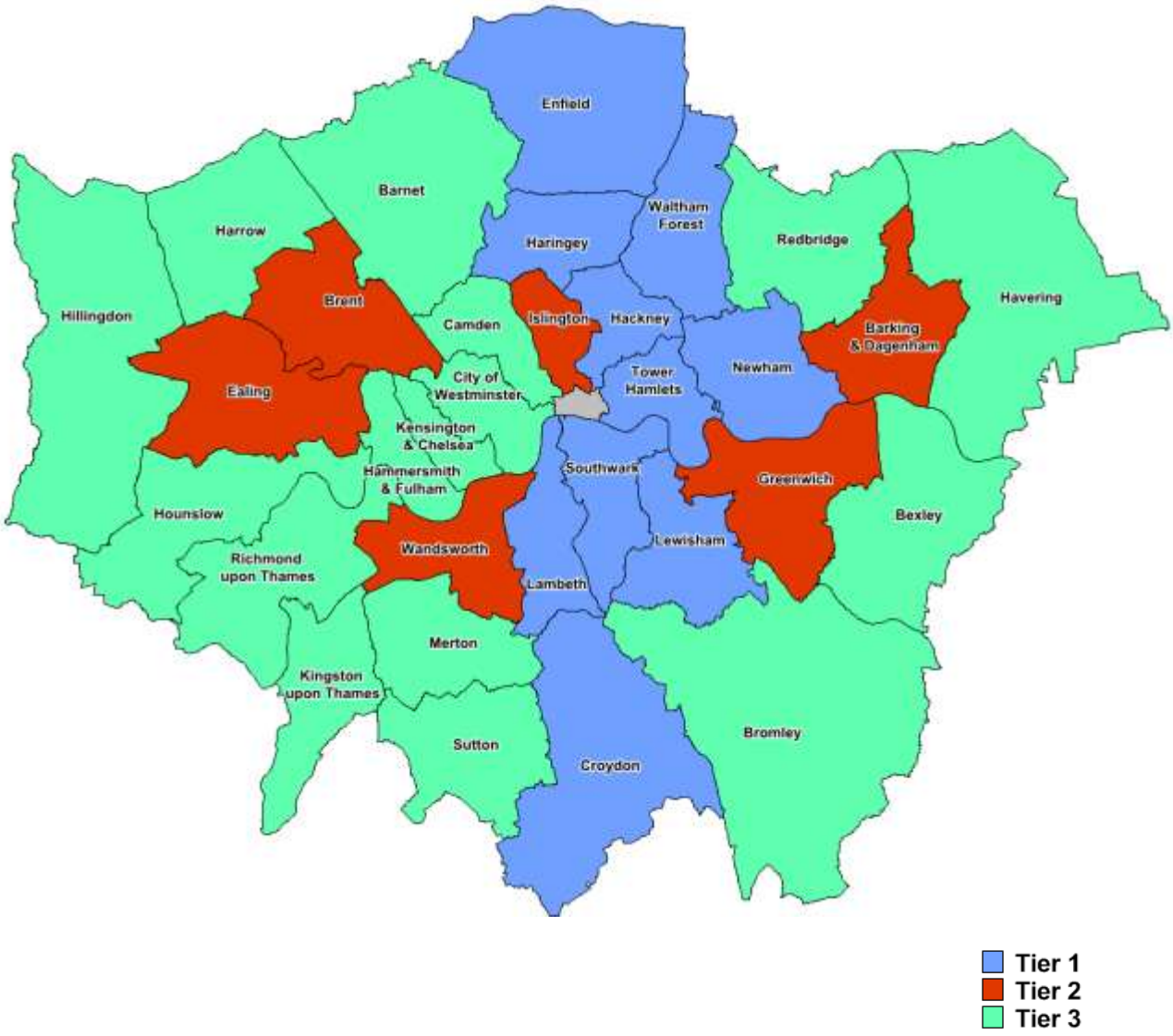


Table C.1: Metropolitan Police Section 60 searches and arrests, 2006/07–2014/15

Financial year	Section 60 searches	Resultant arrests	Arrest rate
2006/07	16,917	621	4%
2007/08	17,653	633	4%
2008/09	114,316	2,757	2%
2009/10	90,869	1,993	2%
2010/11	53,509	1,084	2%
2011/12	39,352	1,017	3%
2012/13	3,162	178	6%
2013/14	1,854	124	7%
2014/15	234	8	3%

Source: Home Office Statistics

Map of London boroughs, by Operation BLUNT 2 tier assignment



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