Does stop and search deter crime? Evidence from ten years of London-wide data

Abstract
Despite recent declines in its use stop and search remains one of the most controversial powers vested in the police in the United Kingdom. Yet there has been surprisingly little research into its effectiveness as a tool of policing, significantly impeding debate around this topic. In this paper we used ten years of police, crime and other data from London to investigate the potential effect of stop and search on crime. Using lagged regression models and the natural experiment created by the sudden increase and subsequent decrease in the use of s60 powers in the capital, we show that the effect of stop and crime is likely to be marginal, at best. While there is some association between stop and search and crime (particularly drug crime), claims that this is an effective way to control and deter offending seem misplaced. We close the discussion, however, by arguing that this is somewhat to miss the point, and for two reasons. In a legal sense the key issue is that each and every stop is justified in itself, not because it has some putative wider effect on crime. In a sociological sense, our findings support the idea that stop and search is a tool of social control widely defined, not crime-fighting, narrowly defined.

Introduction

Use of police powers to stop and search (S&S) members of the public has fallen significantly in England and Wales over the last few years. The number of recorded searches in 2014/15 was approximately 541,000 down by 58% from a peak of almost 1 million in 2008/9 (Home Office 2015). Yet use of the power remains a controversial issue. The decline in recorded searches has not be accompanied by a similar reduction in the ethnic disproportionality in their application; in 2014/15, people who identifying as Black or Black British were still 4 times more likely to be searched than their white counterparts (Home Office 2015). S&S can still trigger significant reactions from individuals and groups who experience or observe its use, and wider social and political debates, as current disproportionalities entrench and interact with previous evidence of bias and discrimination.

Our focus in this paper is not on ethnic disproportionality in S&S, which has been well documented elsewhere (Equality and Human Rights Commission 2010; Bowling and Phillips 2007; Quinton 2015; Bradford and Loader 2016). Nor are we concerned with the wider social and cultural 'meaning' of stop and search – as a tool of social control (Choongh 1997; Bradford and Loader 2016), for example – although we return to this question in the conclusion. Rather, we are concerned with whether S&S deters crime. This is a salient issue for two inter-connected reasons. First, despite reductions in its use, S&S remains one of the most widely used formal powers of the police. Yet very little is known about its effect on crime (Delsol 2015), particularly in a UK context. Second, it remains a commonplace of media and police accounts of S&S – perhaps particularly in relation to the recent reduction in its use – that S&S 'must' have an effect on crime (and therefore that restricting use of the powers risks an increase in crime). While the law governing S&S tends to revolve around the *investigation* of crime, there is no doubt that police officers and many observers of police activity take a broader view, believing and S&S has a *deterrent* effect. This makes consideration of its likely effect on crime a pressing policy concern – yet one which has rarely been addressed with appropriate empirical data.

To address this question we use 10 years' worth of S&S, crime, and other data from London, aggregated at the borough level, and we take two distinct analytical approaches. First, we utilize fixed effect regression models estimating the lagged effect of S&S on crime. Second, interrupted time series analysis is used to explore the potential effect of the sudden rise in the use of 'suspicion-less' or authorised searches that occurred from 2007 to 2011. To anticipate the results, we find that S&S has only a very weak and inconsistent association with crime. While there is some correlation, most notably in relation to drug offences, we conclude that the deterrent effect of S&S is likely to be small, at best.

Setting the scene

The 'power' of S&S in England and Wales actually comprises a range of powers governed by a number of pieces of legislation that enable officers to search for a range of items. Most well-known is section 1 of the Police and Criminal Evidence Act 1984 (hereafter s1), which enables searches for stolen goods and a range of prohibited items (such as offensive weapons). Additional powers include those granted under: section 23 of the Misuse of Drugs Act 1971 (s23) for controlled substances; section 47 of Firearms Act 1968 (s47) for firearms; section 60 of the Criminal Justice and Public Order Act 1994 (s60) to prevent acts of serious violence; and section 44 of the Terrorism Act 2000 (s44, since repealed) to prevent acts of terror. For the purposes of this paper we distinguish between two broad groups of powers – those that require officers to have 'reasonable suspicion' before conducting a search (s1, s23 and S47) and those that do not but, instead, require them to be authorised to carry out searches in a defined area for specified time period (s60 and s44). This is an important distinction in terms of the way the powers are used, their purpose in law, and, more importantly, in terms of the level of debate that surrounds them – s44, in particular, was one of the most controversial powers vested in the police in modern times (Shiner and Delsol 2015).

Although S&S has come be to be one of the most emblematic, indeed arguably foundational powers, of the police in England and Wales, the extent of its uses varies markedly across time and place (Bradford 2017). The data used in this paper cover London during the period 2004 to 2014. By way of an introduction, Figure 1 shows that while recorded crime declined gradually but consistently over this period, S&S showed marked variations month-on-month and over the 10-year study period. The recorded use of reasonable suspicion searches increased between 2004 and 2010 and then went into steady decline. Authorised searches that do not require officers to have reasonable grounds were rarely used before the middle of 2007; use then increased markedly, reaching a peak in 2008, after which they too went into steady decline. After a further peak in August 2011 (which coincided with the London riots), usage again became rare.

Figure 1. Trends in searches and crime (Metropolitan Police, 2004-14)

Note: Susceptible crime = those crimes that are susceptible to detection by S&S.

The deterrent effect of S&S

We are concerned in this paper with the deterrent effect S&S may exert on potential offenders; particularly whether a marginal change in the S&S rate in an area can be linked to a subsequent change in the crime rate in the same area. It is worth reiterating that this is not the legal justification for most searches. While the authorised search powers are framed in legislation explicitly in terms of prevention, the more commonly-used reasonable suspicion powers are framed as investigatory tools aimed at apprehending the perpetrators of crimes that have already been or are in the process of being committed (a point we return to in the conclusion).[1] However, considering the mechanisms by which S&S might reduce crime, and given the significant conceptual overlap between apprehension and deterrence as well as the nature of public and policy debate around S&S (very often framed in terms of deterrence), we believe that this is a justifiable starting point for our analysis.

Deterrence theory outlines distinctions between different 'varieties' of deterrence. In the case of S&S the most pertinent relate, first, to the *certainty* of apprehension as a lever for deterring

[1] PACE Code of Practice A states that the purpose of S&S is "to enable officers to allay or confirm suspicions about individuals without exercising their power of arrest (Home Office 2014: para 1.4).

potential offenders. It is usually argued that what deters people from committing a crime is not the severity of any punishment that may ensue, nor the speed with which it will be delivered (celerity), but their perception of how likely they are to be caught if they were to commit the offence (Apel and Nagin 2011, Nagin 2013; see also Pratt et al. 2009). If it is to have a deterrent effect, S&S, must make acts of crime appear riskier to potential offenders by suggesting that they are likely to be caught if they do break the law.

Second, deterrence theory rests on the contrast between *specific* and *general* deterrence: to whom is the risk of apprehension communicated? Specific deterrence functions at the individual level, referring here to the effect of S&S *experiences* on offenders – and others – who have been interdicted by police. A proportion of those searched will be arrested or handed an alternative sanction such as a cannabis warning or fixed penalty notice. Having been caught 'red-handed', it naturally seems possible that these individuals might be deterred from committing a crime in the future: being searched and possibly receiving a penalty at one time may change their risk calculus in relation to future potential acts. Likewise, simply being stopped and searched, even when one has not broken any law, may have a future deterrent effect on one's behaviour. General deterrence, by contrast, operates at a wider level, and refers to the effect S&S *awareness* might have on the behaviour of the general population – as potential offenders – who see or hear about this type of police activity or who merely know the police can carry out S&S. Witnessing or having some knowledge about S&S may shift people's risk perceptions, leading them to believe, for example, that police are effective 'sentinels' (Nagin et al. 2015) who are capable of apprehending offenders.

Deterrence theory is thus premised on the existence of rational potential offenders who undertake "a conscious weighing of the benefits and costs of offending contingent on and constrained by factors of the environment [and] situation" (Nagin et al. 2015: 79). S&S activity comprises part of the environments and situations within which potential offenders make decisions, and it exerts an influence on their behaviour by making offending riskier. This argument involves two core assumptions: that people are aware of the level of police activity in their environment; and that offenders and members of the wider public update their risk perceptions as a result of experiencing or knowing about such activity, such that an increase in objective risk of sanction (more policing) is linked to an increase in subjective risk. If these assumptions do not hold it is hard to envisage how S&S can have a deterrent effect on crime.

While there is debate within the literature (Apel 2013; Pickett and Roche 2016), the balance of evidence suggests it is unlikely that "criminal justice policies or police activities exert … influence on individual's perceptions of arrest risk" (Picket and Roche 2016: 729) in any widespread or consistent manner (see also Paternoster 2010). There is little reason to believe that there *is* an awareness of the general volume, distribution or nature of police activity such as S&S among potential offenders (or anyone else), casting doubt on the idea that S&S has a *general* deterrent effect on crime. To put it another way "it is not clear how or even if individuals update their subjective probabilities [of sanction risk] in response to changes in objective sanction risk" (Apel 2013: 86). If police change the objective risk of sanction by increasing the level of S&S in an area this will not necessarily lead to changes in the behaviour of people in that area, since they do not 'notice' police activity in such a way that would lead them to reassess how risky they thought certain behaviours were.

There *is* however evidence on *specific* deterrence that suggests people update their risk perceptions as a result of personal and perhaps vicarious experience of arrest or apprehension (Apel 2013). Notably it seems that the extent of such updating (the size of the shift in perceived risk) is greatest among those who commit crime, do not get caught, and consequently lower their risk perceptions, thus making them more likely to offend in the future (Pogarsky et al. 2005; Matsueda et al. 2006). It is therefore possible that a decline in S&S rates, in as much as it results in a reduction in the number of people apprehended after offending[2], may have an upward effect on crime rates because

[2] Recall that a non-trivial proportion of all arrests result from a S&S. For example, in 2014/15 there were 950,000 arrests for notifiable offences in England and Wales; over the same period 75,000 arrests resulted from searches conducted under s1 PACE and associated legislation (Home Office 2015).

it diminishes the perceived risk of sanction among those who 'get away' with crimes (i.e. who could have been searched while intending to or having committed an offence but were not). By contrast it is somewhat less clear that individuals who *are* searched and/or sanctioned by police update their risk perceptions accordingly (i.e. are led to believe they are likely to be searched and/or sanctioned again in the future). Piliavin et al. (1986), for example, found that there was no correlation between their respondents' prior arrest records and a measure of the perceived risk of formal sanction (see also Kleck et al. 2005). There may thus be an asymmetry in the potential effect of S&S on people disposed to offend. Other studies, however, report broadly 'symmetrical' effects of prior arrests on updating (e.g. Lochner et al. 2007; Anwar and Loughran 2011), suggesting that people who are arrested do update their risk perceptions accordingly.

It is also possible that S&S has a 'disruptive' effect that does not fit neatly into deterrence theory, but rather, perhaps, situational crime prevention. First, use of the power(s) in an area may make it harder for offenders to offend – for example by motivating the 'stashing' rather than carrying of knives – and thus disrupt their activity by delaying it, displacing it or reducing its severity. Second, an officer may search someone for 'going equipped'. They had not yet stolen something, but were actively planning to do so. Ignoring for the moment that a search has created a possession offence, the officer has prevented future crime. Such situations are better described as disruption rather than deterrence because the motivation of the offender is unaffected, but, situationally, they cannot commit their planned crime.

A further complicating factor is that not all crimes are equally 'susceptible' to S&S (Miller, Bland and Quinton 2000). At a general level, crimes in the categories of violence, robbery, burglary, theft and handling, drugs and some forms of criminal damage are generally considered susceptible, as they involve the carrying of items related to the offence (e.g. a weapon, stolen property, or drugs). Other important categories, such as fraud, harassment/stalking, and cybercrime are by nature not susceptible to this form of police intervention. One implication here is that if attention is limited to the relationship between S&S and 'all-crime' then analysis may under-estimate its effect, and that consideration of specific crime types is required.

There is an important counterpoint to the discussion thus far. In contrast to the deterrence literature, there is significant evidence that targeted police strategies, most notably hotspots policing, *can* have an effect on crime (Braga et al. 2014; Weisburd and Eck 2004). This is relevant in the current context because such strategies often contain increased use of S&S, either intentionally or simply as a result of deploying additional police officers (see for example, Taylor et al. 2011). While the causal mechanisms behind the observed effects of hotspot policing on crime remain opaque, it seems that targeted S&S activity *may* reduce crime, presumably via some sort of specific deterrent effect. However, such strategies were not in common use in the Metropolitan Police during the period covered in this paper. While small independent efforts may have been implemented at some times and places, we have no reason to believe that S&S activity was actively being targeted towards crime hotspots in a systematic and consistent manner across the police force area. Indeed, evidence suggests that it is people, not places, that are most commonly 'targeted' by officers for S&S (Bradford and Loader 2016; Quinton 2011) and that officers' perceptions of high crime locations may not be accurate (Ratcliffe and McCullagh 2001; Chainey and Macdonald 2011). It seems unlikely then that S&S, certainly when measured at a borough level, might have exerted an effect on crime via a focus on high crime locations.

Findings from existing studies

Given the discussion above it is perhaps not surprising that existing studies of the effect of S&S (and related activities) on crime report very mixed findings. The foundational – and much critiqued – study in the field is Boydstun (1975). A quasi-experiment conducted in the San Diego in the early 1970s found that the suspension of Field Interrogations (FIs) in one beat appeared to lead to an increase in 'suppressible' crime compared to a 'business as usual' control site and a beat where only specially trained officers were allowed to conduct FIs, but that there was no significant change in the total number of arrests across all three sites. Two more recent quasi-experimental studies are reported by

McCandless et al. (2016) and MacDonald et al. (2016). McCandless and colleagues used a retrospective design to explore the effect of Operation BLUNT 2 in London (a knife crime initiative involving a large increase in s60 searches in some Metropolitan Police boroughs). Using difference in differences analysis, which compared change in the boroughs where Operation BLUNT 2 was in place with change in those where it was not, they concluded that the police operation (i.e. a large increase in weapons searches) had no effect on police recorded crime; indeed, ambulance calls fell faster in those boroughs where there were smaller increases in searches.

MacDonald et al. (2016) used a similar design, this time based around Operation Impact in New York, which involved increasing the number of officers, Stop, Question and Frisks (SQFs) and arrests in hotspots ('impact zones'). Different types of SQF appeared to have different effects. While an increase in those SQFs based on reasonable suspicion had no consistent association with crime, the increase in SQFs based on probable cause (a higher legal threshold linked to specific criminal behaviour) was associated with relative reductions in total reported crimes, assaults, burglaries, drug violations, misdemeanor crimes, felony property crimes, robberies, and felony violent crimes in the impact zones. The authors, however, described the results as having "little practical importance" (MacDonald et al. 2016: 9) because of the small size of the reductions and the fact that probable cause SQFs made up a tiny proportion of the overall increase in SQFs. Also, the reductions could not, in the main, be directly attributed to increases in 'investigative stops': MacDonald and colleagues note, echoing the wider literature on hotspots, that the precise cause of the observed effects from Operation Impact remained unclear.

Other studies have used time series and associated techniques to examine observational data. Penzer's (1999) unpublished analysis of Metropolitan Police data looked at whether S&S across London had lagged monthly effects on total recorded crime and street robbery. No underlying associations were found once a sudden upward 'shock' in total crime was taken into account. Smith et al. (2012) used city- and precinct-level data from New York to explore lagged weekly effects of SQF on nine types of recorded crime, concluding that SQF was negatively associated with four (vehicle crime, robbery, assault and rape) but not with the others. Notably, effects, even when statistically significant, were very small. For example, Smith and colleagues estimated that if SQF was 10 per cent higher in week one, robbery would have been 0.09 per cent lower than predicted at the precinct level, and 0.03 per cent lower than predicted at the city level, in week 2 (2012: 32). Rosenfeld and Fornango (2014) also used precinct data from New York, although this time aggregated at an annual level, and concluded that SQF had no significant effect on burglary or robbery once relevant confounds were taken into account. Fagan (2016), again looking at New York, explored whether probable cause SQFs and reasonable suspicion SQFs had different effects on six crime types at the precinct level. He found that aggregates of both SQF types had significant negative two-monthly lagged effects on violent felonies, property felonies, drug crimes, weapon offences, other felonies and misdemeanors, but that the effects were consistently larger when probable cause SQFs were examined on their own. The analysis also suggested the sharpest decreases in crime were associated with the highest concentrations of probable cause SQFs. Fagan thus concluded that targeted use of searches based on reasonable suspicion was unproductive, compared to those based on a higher standard of evidence, and add "nothing to the crime control efforts of law enforcement" (2016: 79)

Finally, Weisburd et al. (2016) used data aggregated at much lower levels temporally (days and weeks) and spatially (street segments) to again explore SQF in New York, in particular its potential contribution to a hotspots policing strategy. Looking at lagged weekly effects across the city, the authors found that SQF had a significant, albeit small, negative association with crime at the street segment level, although the size of the effect was variable across different boroughs (their analysis suggested that an extra 700,000 SQF would reduce crime by 2 per cent – 2016: 47). They also looked at specific instances of SQF in the Bronx and found that these they were negatively associated with crime for up to five days afterwards. Weisburd et al., therefore concluded that SQF had a significant, if small, effect on crime when targeted intensively in high crime locations (albeit this was an approach that had been ruled unconstitutional).

In sum, there is little agreement in the literature as to the existence, or likely size, of any effect of S&S on crime rates. Some studies report modest but significant effects, at least in relation to some crime types, while others report null findings. We cannot in this paper provide a definitive answer to this apparent conundrum. Rather, we use London as a case study from which to add to this growing body of evidence.

Research design

Aims and hypotheses

This paper examines whether the use of S&S by the Metropolitan Police reduced crime via a deterrent effect on potential offenders. Using borough-level data covering a 10-year period, we tested the following hypotheses:

H1. That overall S&S, under any power, was negatively associated with subsequent levels of total recorded crime
H2. That overall S&S, under any power, was negatively associated with subsequent levels of specific types of recorded crime
H3. That S&S under particular powers was negatively associated with subsequent levels of specific types of recorded crimes.
H4. That sudden changes in the use of s60 searches were associated with changes in violent crime.

Data

The Metropolitan Police provided daily counts of recorded searches and particular categories of crime that might be susceptible to detection by S&S for every borough in London from April 2004 to November 2014. While data on all 32 boroughs were provided, Westminster was excluded from the analysis presented here as it was an outlier in terms of its population size and number of recorded searches.[3]

Separate counts were provided for the various powers. Daily crime counts were also provided for recorded drugs offences, non-domestic violent crime, burglary, robbery and theft, vehicle crime and criminal damage, which we aggregated into an overall count of total susceptible crime. To enable us to explore the specific relationship between S&S and violence further, we obtained counts of weapon-enabled non-domestic violent crime from the Metropolitan Police and of ambulance incidents related to 'stab/shot/weapon wounds' from the London Ambulance Service. In theory, the former should have been the sub-category of violence most susceptible to S&S, while the latter should have overcome some of the problems of violence not being reported to the police and not being included in the counts of recorded crime.

The counts were converted into rates per 100,000 residents to control for population and to better reflect the likelihood of an individual being searched by the police or becoming a victim of crime.[4] Table 1 presents summary of these data.

Table 1: Descriptive statistics (per borough, per week)

Variable		*Mean*	*Std. Dev.*	*Min*	*Max*
S&S	Total searches (all powers)	229	169	6	2,574
	s1 (weapons) and s47	32	31	0	323
	s1 (not weapons)	68	48	1	530
	s23	120	96	2	938
	s60	20	73	0	1,720
Crime	Total susceptible crime	409	140	104	1,034
	Non-domestic violent crime	75	29	10	215

[3] All models were reproduced including Westminster, with little effect on the results.

[4] The rates were based on mid-year population estimates for each borough from the ONS and splined to produce monthly and weekly estimates.

	Drug offences	32	23	0	332
	Burglary	55	20	6	244
	Robbery and theft	134	64	21	615
	Vehicle crime	67	28	5	220
	Criminal damage	44	21	3	170

There were two limitations to these data. First, we examine broad categories of crime. While the use of more specific crime types might have provided a better test of S&S, too many boroughs recorded no offences under each category, especially on a weekly basis, which complicated the type of analysis presented here.[5] It was also not possible, for example, to test whether S&S was associated with knife crime for this reason. Second, as the analysis relied mainly on police data, we could only analyse activities and crimes that were recorded by officers. For example, during this period, data were recorded about arrests from searches but not any other 'positive outcome' (e.g. fixed penalty notices).

It is also worth noting that we only look at the quantity of searches, not their 'quality' (e.g. the context in which they were performed or how they were conducted). It is highly likely that policing priorities and practices will have varied by borough and over time, and it is beyond the scope of this paper to relate the results of our analysis to these variations. Our analysis therefore presents an average for the 31 boroughs over the 10-year study period and, as such, should be regarded as a 'real world' assessment of the effectiveness S&S rather than a test under 'ideal conditions'.

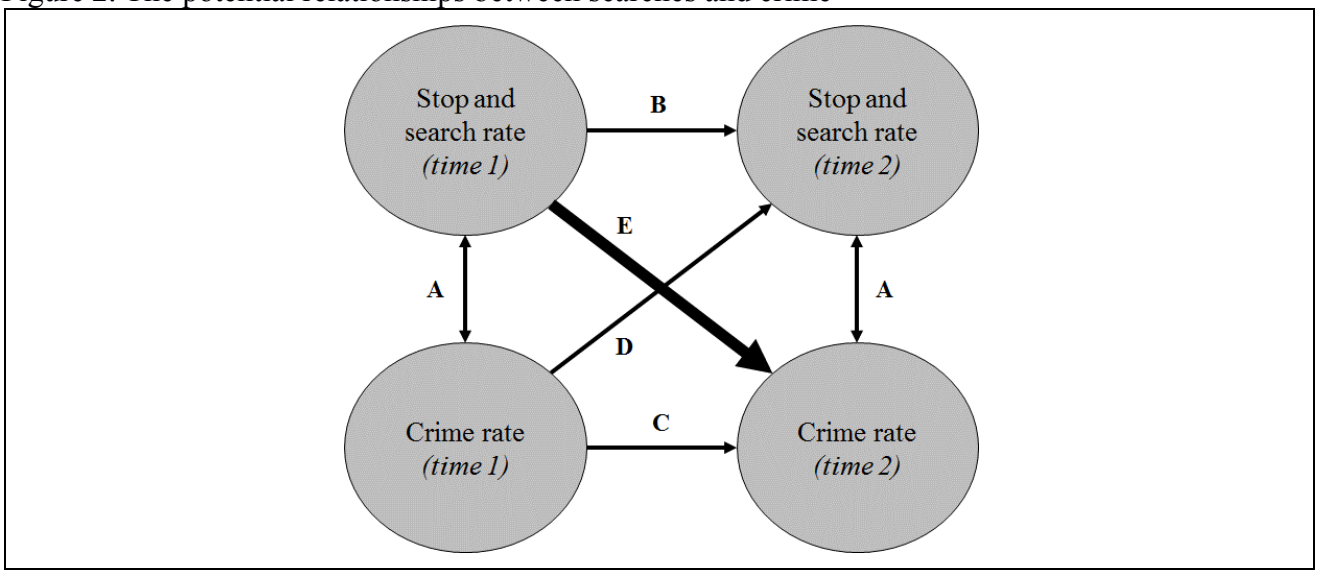
Analysis

We tested H1, H2 and H3 using regression analysis (the fourth was tested via a quasi-experimental design – see below). The regression analysis was complicated by the problem of reverse causality. Searches and crime are associated in many different ways, making it extremely difficult to untangle cause and effect, and as Figure 2 shows there are five relationships of interest:

A. S&S levels and crime might influence one another in the same period (at either time 1 or time 2). For example, S&S might be carried out in response to higher crime, crime might be reduced by S&S, and/or crime might be increased by S&S if the searches led to new offences being discovered and recorded.
B. S&S levels (time 2) might be influenced by S&S in the previous period (time 1).
C. S&S levels (time 2) may respond to crime in the previous period (time 1).
D. Crime levels (time 2) might be influenced by crime in the previous period (time 1).
E. Crime levels (time 2) might be reduced by S&S in the previous period (time 1).

[5] Attempts to use negative binomial models were complicated by the number of variables and, indeed, we could not make those models converge.

Figure 2. The potential relationships between searches and crime



Note: It is possible that searches and crime respond to one another at different speeds. For example, offenders may respond quicker to more S&S than the police do to more crime.

The challenge is, therefore, to show whether S&S had a lagged relationship with crime (E) above and beyond all other possible associations. In order to do this we included the lagged crime rate and the current rate of S&S in all our models (essentially an autoregressive distributed lag (1,1) model). This controls for relationships A-D, but it also creates some statistical challenges which will be considered below.

The other important aspect of research design is the level of aggregation. Studies have examined the impact of searches annually (Rosenfeld and Fornango 2014) and daily (Weisburd et al. 2015). Although daily data were provided (see below) we chose months and weeks as a middle ground, which is justified on theoretical and methodological grounds. Theoretically, it seems somewhat implausible that people will adjust their beliefs about the likelihood of being searched on a daily basis. But equally, it is hard to imagine any deterrent mechanism working on a very long time scale, as people's beliefs are likely to update more often than annually. Methodologically, it is significant that our data were collected at the borough level. Weisburd et al's (2015) study used daily data but on a micro-geographic scale, where it is plausible that a daily surge in S&S could impact crime. With geographical units as big as ours, it would be almost impossible to cut through the noise in daily fluctuations. However, at the other extreme, modelling a yearly effect would require a very different statistical approach and many more control variables than were available to us. We therefore investigated the medium term dynamics of the relationship between searches and crime: looking at the effect over weeks and months.

We developed a series of fixed effects regression models to test our first three hypotheses:

H1. Weekly and monthly models testing whether total susceptible crime was associated with total S&S under any power, on the basis that offenders may not have distinguished between different search powers.

H2 A series of weekly and monthly models exploring whether our six categories of crime were related to total S&S, for the same reason.

H3. A series of weekly and monthly models exploring whether the following crime types were affected by particular search powers, to which they were most likely to be susceptible to detection: drugs offence and s23 searches; non-domestic violent crime and s1 searches and s47 searches ; burglary and s1 searches ; robbery and theft and s1 (non-weapon) searches; vehicle crime and s1 (non-weapon) searches; and criminal damage and s1 (non-weapon) searches.

To enable this analysis, we converted the crime and S&S rates to natural logs to reduce skewness and to allow us to interpret coefficients as a percentage change in crime rate given a 1 per cent change in S&S rate (for clarity results table show the effects of a 10 per cent change in S&S rate). These were then aggregated by week in the first dataset (31 boroughs x 554 weeks = 17,174 observations) and by month in the second (31 boroughs x 127 months = 3,937 observations).

We also controlled for a number of other factors. First, the level of overall police activity using counts of full time equivalent police officers in each borough.[6] We preferred this proxy measure of police activity to a count of total arrests in each borough because, in the arrest data, 'borough' represents the custody suite that the arrestee was taken to, not the place of arrest. The allocation of arrestees to custody suites is organized centrally and depends on multiple factor such as cell availability, special operations and suites dedicated to certain offences. When custody suites are closed for repairs, boroughs register 0 arrests (this happened in six boroughs during our data period, which would have been dropped from the analysis). Data were unfortunately not available on the number of officers deployed flexibly to each borough or on other police activities.

Second, our analysis took account of any unobserved characteristics of each borough (fixed effects). Third, all models included time-period fixed effects. This allowed us to control for seasonality, changes in Home Office counting rules, or any other London-wide shocks to the crime rate. Fourth, we took account of long-term changes in crime rate within each borough by including borough-specific linear time trends. Finally, as we were mainly interested in testing the deterrent effect of S&S and because arrests resulting from searches could have been independently associated with crime[7], we also controlled for the number of search-arrests at both time one and time two and t-1.[8] See the technical appendix for full model specification and estimation strategy.

Quasi-experimental design

We explored our fourth hypothesis with a different approach. The sudden increase and decrease in use of s60 searches in the Metropolitan Police during our data period allowed us to conduct a quasi-experiment comparing the periods before and after s60 searches became common place.[9] We did not expect the increased use of s60 powers to have an instant impact and so it did not make sense to think of this as a one-off 'treatment'. Instead we examined whether the trend in non-domestic violent crime during the period when s60 powers were being used was significantly different to the trend in the preceding period. To be clear: if s60 powers were effective in reducing violence, then we would have expected the rate of decline in non-domestic violent crime to have increased in this period. We, therefore, performed an interrupted time-series analysis using Prais-Winsten regression to test this hypothesis.[10] We did not need to control for seasonality as both periods extended over multiple years. Furthermore, as s60 powers were not used each month in every borough, we had to aggregate the different panels together, looking at the effect across London as a whole.

Results

Regression analysis

Table 2 presents a summary of the results across different crime types for both the monthly and weekly datasets (full tables of results are available from the lead author). We start by looking at the effect of S&S under all powers on total susceptible crime (H1). The results show that a 10 per cent

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6 These data were provided by the Metropolitan Police with yearly totals that were linearly interpolated to monthly and weekly figures.

7 For example, crime levels might have been influenced by search-arrests in the same period (if they resulted in new crimes being discovered and recorded) and by previous search-arrests (if they resulted in offenders being incapacitated)

8 We also experimented with controlling for unemployment but did not find any statistically significant effects and, as it did not affect our results, we have not included it in the models presented here

9 We chose not to extend this analysis into the third period after 2011 (when s60 use subsided) because it would have been almost impossible to decide on a theoretically appropriate end-point for this period. As Figure 3 shows, after the end of 2011, non-domestic violent crime continued to decline and, although it increased from the middle of 2013, it seems unlikely that this was an 18-month lagged effect of reduced s60 usage.

10 The Durbin-Watson statistic revealed positive serial correlation.

increase in S&S was associated with a drop in susceptible crime of 0.32 per cent (monthly) or 0.14 per cent (weekly). Although statistically significant, this effect was extremely small. In addition, most of the effect that searches had on total crime seemed to come from the specific impact of searches on drug offences. When we excluded drug offences from the total crime rate and s23 searches from the S&S rate, the size of the effects halved in both the weekly and monthly models.

Table 2 also shows the results of our tests of H2 and H3 for each crime type. The clearest results were for drug offences: a 10 per cent increase in rates of total S&S per month decreased recorded drug offences by 1.85 per cent. Again, this was stronger than the weekly effect of 0.64 per cent. We also estimated the net effect of s23 searches, controlling for all other searches at time 1 and time 2. This suggested that most of the effect at the monthly level came from s23 searches, although note we did not find corroborating evidence at the weekly level.

We struggled to find evidence of an effect of S&S on violent crime. The only statistically significant result was the net effect of s1 and s47 weapon searches at the weekly level, and the effect here was far smaller than the any of our other findings: a 10 per cent increase in S&S led to 0.01 per cent decrease in non-domestic violent crime. As a test of robustness, we also looked at weapon-enabled non-domestic violence and found similarly nil results: no effect for all searches and a tiny, but statistically significant, effect for s1 and s47 searches. Moreover, when we used ambulance incident data for calls related to 'stab/shot/weapon wounds', we found no statistically significant results at all. Note that, to ensure comparability, we used the same type of linear model for these two alternative measures but, because of the large number of zeros for both (especially in the weekly data), these figures need to be treated with some caution.

The results for burglary were similarly inconsistent. At the weekly level, a 10 per cent increase in total searches seemed to reduce burglary by about 0.17 per cent. However, the effect was non-significant at the monthly level. By contrast, the net effect of s1 searches was only significant at the monthly level (there the effect of a 10 per cent rise in S&S would be a 0.47 per cent decrease). These effects were again very small and inconsistently significant and so must be treated with caution. There was also no evidence of an effect on robbery and theft (separately and together), vehicle crime or criminal damage.

One potential problem with our analysis is that of multiple comparisons. As we tested for so many associations, there was a chance that some of the statistically significant results reported above would have come about purely by chance. See the Technical Appendix for details on how we addressed this issue, which reinforces the sense that the effect of S&S on crime is marginal at best.

Table 2. Summary results

Crime	*Power*	*Lagged effect on crime rate, if S&S was 10% higher*			
		Weekly	*p*	*Monthly*	*p*
Total susceptible crime	Total searches (all powers)	-0.14%	0.01	-0.32%	0.01
Drugs offences	Total searches (all powers)	-0.64%	0.01	-1.85%	0.00
	s23 searches*	-0.21%	0.37	-1.57%	0.00
Non-domestic violent crime	Total searches (all powers)	+0.09%	0.33	-0.14%	0.21
	s1 and s47 (weapon) searches*	-0.01%	0.00	-0.00%	0.17
Burglary	Total searches (all powers)	-0.17%	0.04	-0.21%	0.12
	s1 (non-weapons) searches*	-0.10%	0.10	-0.47%	0.00
Robbery and theft	Total searches (all powers)	-0.03%	0.54	-0.13%	0.35
	s1 (non-weapons) searches*	-0.08%	0.18	-0.04%	0.64
Vehicle crime	Total searches (all powers)	-0.08%	0.21	-0.04%	0.73
	s1 (non-weapons) searches*	-0.03%	0.96	-0.07%	0.58
Criminal damage	Total searches (all powers)	-0.01%	0.88	-0.06%	0.67
	s1 (non-weapons) searches*	-0.05%	0.40	-0.06%	0.66

Notes: All models estimated using fixed effects estimator (OLS) with cluster robust standard errors. Variables not shown: lagged dependent variable, number of full-time equivalent police officers, period fixed effects, borough-specific linear time trends, current rate of S&S, search-arrests in current period (time two) and search-arrests in previous period (time one).

* Net of all other searches.

Quasi-experimental results

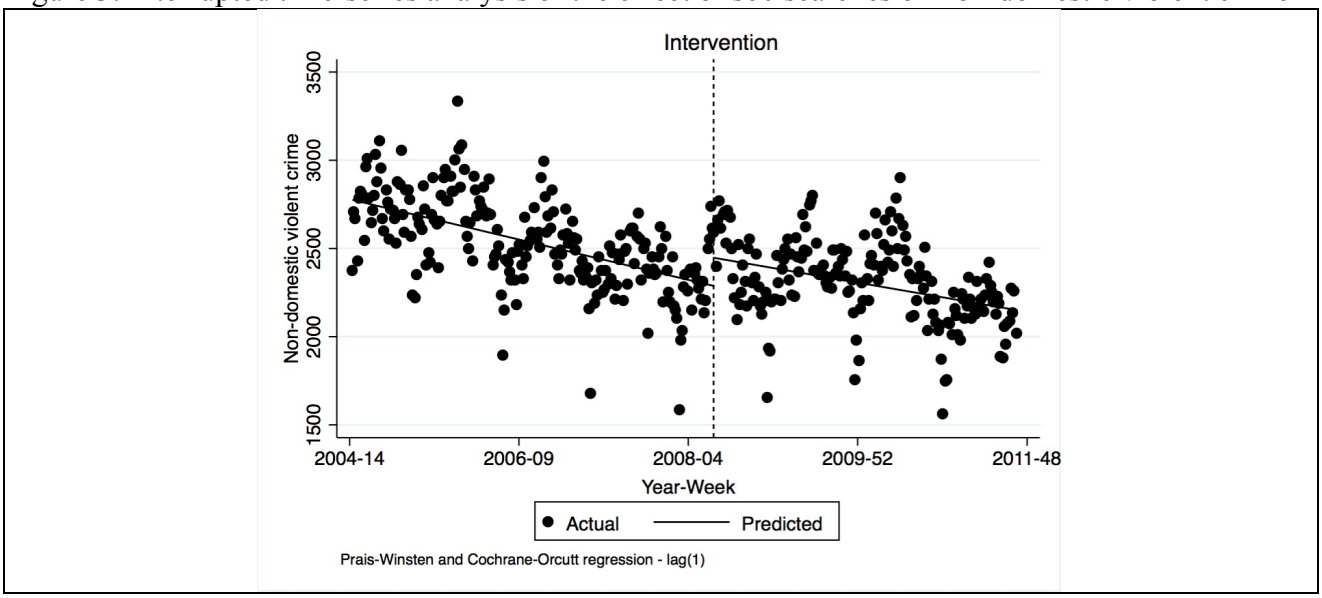
Recall that the sudden increase and decrease in s60 searches allows us to conduct a quasi-experiment comparing the periods before and after s60 searches became common place. As Table 3 and Figure 3 show, there was no statistically significant change in the trend in non-domestic violent crime between the period when s60 searches were used extensively and the period before. This result was robust to the inclusion of population data and officer numbers, and to reasonable changes in the timing of the 'interruption'. In fact, the rate of decline of non-domestic violent crime seemed, if anything, to have slowed (i.e. the coefficient for change to trend was positive and became significant once controls for population are added).

Table 3: The impact of changes in s60 on violent crime

	Coefficient	*Standard error*	*p >t*
Original Trend	-2.28	0.46	0.00
Shock	159.39	80.4	0.05
Change to Trend	0.63	0.77	0.42
Constant	3983.52	66.94	0.00

Notes: Estimated using Prais-Winsten with errors assumed to follow a first-order autoregressive process

Figure 3: Interrupted time-series analysis of the effect of s60 searches on non-domestic violent crime



Discussion and conclusions

Overall, the analysis presented above suggests that, although S&S had a weak association with some forms of crime across London between 2004 and 2014, the effect was at the outer margins of statistical and social significance (H1). We found no evidence for effects on robbery and theft, vehicle crime or criminal damage, and inconsistent evidence of very small effects on burglary, non-domestic violent crime and total crime; the only strong evidence was for effects on drug offences (H2 and H3). When we looked separately at s60 searches, it did not appear that a sudden surge in usage had any effect on the underlying trend in non-domestic violent crime (H4). In other words, we found very little evidence to support any of our hypotheses.

The relationship between S&S and drugs, however, stands out in terms of its relative strength and consistency. This might be thought to provide compelling evidence of S&S having had a deterrent effect on this form of offending. However, there are several other plausible causal mechanisms that might explain the relationship we observe. A deterrent-based explanation assumes that drug users and dealers stop offending when perceived risk reaches a certain level. However, another possibility is that higher rates of S&S prompt people to change their behaviour to make it harder for officers to uncover drugs (e.g. being more cautious, by carrying smaller amounts and hiding them more carefully)

or that people carrying drugs – especially hard drug users – are simply displaced to nearby areas that are less 'hot' in terms of police activity (Small et al. 2006; Wood 2004). Furthermore, police recorded crime data are unlikely to be the most reliable measure of drug crime. The number of recorded drugs offences will depend largely on police activity that discovers people in possession of drugs, and not the underlying prevalence of drug use. This is not to say that S&S has no deterrent effect on drug crime, but clearly more research is needed to fully understand the mechanisms behind the association described above.

It was also notable that the month-on-month relationship between S&S and crime was consistently stronger than the week-on-week relationship. One possible explanation is that people need to be exposed to higher levels of S&S for a longer period of time (particularly at the borough level) before they update their beliefs about the likelihood of being apprehended. As noted above, Weisburd et al. (2015) have suggested that SQF has a deterrent effect over a matter of days, but their study was focused at a very local level where it might be more reasonable to expect that people notice, and respond to, daily fluctuations in police activity.

Our results would seem to support, therefore, the idea that police activity – at least in the form of S&S – has relatively little deterrent effect. Indeed, assuming that longer-term incapacitation effects from S&S are minimal, since we find only very weak associations between S&S and susceptible crimes it appears that this form of policing has little effect on crime via other routes (e.g. disruption). Taken together, therefore, our results suggest that we should stop thinking about S&S as a *policy tool* or *tactic,* whereby its overall level can be deliberately increased with a view to reducing crime, and focus instead on the appropriateness and legal justification of individual uses of the powers.

This last point goes to the crux of the matter. The downward trend in S&S that started in the second half of our study period has since continued; reasonable suspicion searches fell by 46% between 2013/14 and 2015/16, a reduction that was echoed nationally (Hargreaves et al. 2016). While there are many explanations for this drop, it is plausible that increased scrutiny had some effect on police practices. Her Majesty's Inspectorate of Constabulary (2013) and the Home Secretary (2014) both criticised the misuse of S&S and their assessments led to the introduction of the voluntary Best Use of S&S Scheme and national training (Quinton and Packham 2016). Within the Metropolitan Police, the incoming Commissioner – Sir Bernard Hogan-Howe – expressed concerns about the overall level of S&S, its contribution to the 2011 riots and the low arrest rate, and oversaw the introduction of the StopIt programme, which consisted of performance targets and other interventions aimed at making searches more effective and fair (Equality and Human Rights Commission 2013). The tone of this narrative, however, started to shift in response to increased knife crime in London from around 2015. On his retirement, Hogan-Howe questioned whether there might be a 'floor' in the reductions of S&S, after which it ceased to be effective in reducing crime: "…it's possible we got too low in stop/search – a 70% [reduction] is a very big change" (2017). More recently, his replacement Cressida Dick has, along with the Home Secretary, sought to encourage officers to have greater confidence in their powers, while emphasising the need for them to act lawfully (Dick 2017).

Our analysis concurs with most other studies in the field in concluding that increasing levels of S&S is likely to have *at best* a very marginal effect on emerging crime problems. This is not to claim, however, that individual searches do not produce useful 'results' (e.g. uncovering contraband and/or preventing a potential crime). It seems to us that the debates that swirl around S&S could usefully be refocused on the particular instances when the power is used, rather than on its alleged effect on crime in a general sense. As per most of the legislation governing S&S, the police should ask not whether S&S – as a tactic – contributes to crime reduction, but whether each and every search – as a power – is legally and operationally justified.

This is not to say that overall volume is unimportant, and the relationship between drugs and searches is again interesting here. The overall level of S&S is, to a large extent, determined by the number of searches for drugs – they represented over 60% of all searches in 2015/16 (Hargreaves et al. 2016) – which provides something of a conundrum in the light of the results described above. On the one hand, drugs offences seem to be the crime type most affected by the volume of S&S. On the other hand, most drugs searches are for cannabis possession (Quinton et al. 2017), the reduction of

which is unlikely to be a priority for any police force (and which comes with a range of well-known net-widening effects – Release 2013). While it is beyond the scope of the current article to consider this question in any depth, it does underline that understanding the reasons for individual searches is essential to understanding the use of the power as a whole. Moreover, the effectiveness of the power needs to be considered in light of the 'usefulness' of the ends towards which it is directed. Even if S&S is effective in deterring minor drug offending, is this reason enough for its continued use at current or indeed raised levels?

Finally, there is a deeper question that bears some reflection. We have little reason to believe that the results we have presented differ from what would have been found at other times and places in England, Wales and across the UK. It seems likely that S&S has never been particularly effective in controlling crime. Why, then, is the power still so commonly used? On one level the answer is simple – police officers believe that S&S is, indeed, a useful tool of crime control. Yet it is equally important to recognise that S&S is not solely about crime. As research over three decades has suggested, it is also a tool of order maintenance, used by police officers seeking to assert power and control in a particular situation or over a particular locale (Smith and Gray 1985; Choongh 1997; Quinton 2011). S&S may also play a structural role linked to the basic function of police as an institution of social ordering (Bradford and Loader 2016); a way for police to discipline and ascribe identity to the populations they police. The extent and distribution of its use may be affected by the location of the *policed* within social, economic and other hierarchies that, for example, predispose some to be far more exposed to this form of state power than others (c.f. Waddington et al. 2004), or which mark them as out of place, troublesome, or threatening (Bradford 2017). The benign interpretation of this wider function of S&S is that it can be a useful way for police to establish authority, a sense of control, and maintain order. Here, it might be argued that S&S has positive effects above and beyond the detection or deterrence of crime. A less benign interpretation is, of course, that this is a power directed disproportionately toward people from marginal and excluded social groups, and which serves only to deepen their marginality (Bradford 2017; McAra and McVie 2005). On both accounts, the question as to whether S&S has crime control properties is rather beside the point, as this is in some fundamental sense not what the power is 'about'.

Our findings offer support to these arguments, at least in as much as they add to the weight of evidence that S&S is, at least in the aggregate, not about crime. If it were, we would expect a much stronger association between levels of S&S and crime than that which we identify in our models. Or, perhaps, for its general *ineffectiveness* as a tool of crime control to have become much more widely accepted than currently seems to be the case. Instead, police, politicians and public hold on to the idea that this is a useful way for police to 'fight crime', something that may have as much to do with the cultural image of the police as the only institution that can effectively do this. Indeed, for some at least S&S may be seen as effective *because* it is done by police, and police action, as opposed to inaction, is by definition effective in controlling crime (and S&S is, if nothing else, a visible way of 'doing something about crime'). Research that considers the 'effectiveness' of stop and search, such as the present study, can therefore only address one part of a much bigger picture. But, we feel, an important part, not least because it might help to dispel some of these myths and refocus attention where it should properly be – on the fact that S&S should concern only investigation of specific crimes, and on the need to limit its use to appropriate situations to avoid damage to public trust and police legitimacy.

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