Crime Displacement and Police Interventions: Evidence from London's "Operation Theseus"

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Abstract

While a small literature in economics has addressed the direct effects of police on crime less attention has been paid to the indirect effects. Such indirect effects could occur in cases where a police intervention changes the relative costs of committing crimes either across areas (spatial displacement) or different time periods (intertemporal displacement). We test for such effects using the case of a major police intervention in London during July-August 2005, following the terrorist attacks that hit the city in July. This intervention involved a 34% increase in police hours worked for a selected set of five London boroughs over six weeks. Furthermore, the change in the relative costs of crime induced by this intervention was a very clean one in terms of measuring possible displacement effects. Despite the clear and well-identified direct effects of the policy intervention, we are unable to find any evidence of significant spatial or intertemporal displacement in crime during or after the intervention.

Keywords: Crime; Police; Terror attacks.
JEL Classifications: H00, H5, K42.

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1. Introduction

Falling crime rates in the US (and some other countries) since the late 1980s have prompted an extensive discussion of the determinants of crime (see Levitt, 2004, and Freeman, 1999, for a summary). The role of police in reducing crime has been a part of this discussion and a series of contributions have sought to estimate the causal impact of police and crime in various settings.[1] This literature in economics has mainly examined the direct effects of police on crime, that is, the impact of additional police resources or interventions on intended crime reduction outcomes.

The indirect effects of police interventions have received less attention. Such indirect effects would occur in cases where an intervention changes the relative costs of different types of criminal activity. For example, if a change in relative costs is large enough, a crime reduction achieved in terms of the intended outcome may be offset by an increase in crime for another related outcome. Most simply, this would occur in cases where crimes are differentiated by time and location thereby creating the possibility of temporal or spatial displacement in criminal activity.

The issue of displacement has been canvassed much more in criminology than economics. Braga (2001) provides a review of five studies in experimental criminology that focused on potential spatial displacement effects across a diverse set of crime reduction programmes. These studies encompassed drug, gun and general crime interventions, and used research designs similar to those employed by empirical economists. There was minimal evidence of spatial displacement across a number of

[1] A long, but not exhaustive, list of papers trying to identify a causal impact of police on crime includes: Levitt (1997, 2002); McCrary (2002); Corman and Mocan (2000); Di Tella and Schargrodsky (2004); Klick and Taborrak (2005); Evans and Owen (2007); and Machin and Marie (2009).

potential outcome variables (including actual crimes committed and service call-outs to police). Overall, these findings are in line with the conclusions of previous surveys of the criminology literature on displacement such as Hessling (1993) and Sherman and Weisburd (1995).

The recent paper by Jacob, Lefgren and Moretti (2007) provides the most comprehensive discussion of displacement issues in the economics literature. This paper focuses on the dynamics of criminal behaviour and uses weather shocks as a source of exogenous variation to evaluate the intertemporal structure of criminal activity. Following this strategy, Jacob et al (2007) *do* find evidence of intertemporal shifts in criminal activity, estimating that a 10% increase in violent crime in a given week is followed by 2.6% reduction in the week after. Similarly, they estimate a 10% increase in property crime was followed by a 2% fall in the following week. In their simple dynamic model the property crime effect works through an income effect, while the violent crime result is due to the diminishing marginal utility of violence (i.e. an offender may "settle a score" one week and derive less utility from using violence in the next week). This is particularly interesting in that it opens up the mechanisms underpinning crime displacement, that is, the specific costs and benefits faced by criminals when making decisions about criminal activity.

Braga (2001) provides a review "hot spot" policing strategies covered in the criminology literature that are relevant to the issue of crime displacement. They discuss risk-focused policing strategies, that is, attempts to target particular high-crime areas with additional police resources. These resources entailed actions such as tailored "problem-oriented" policing responses; patrol programs; and actions based on crackdowns or raids.

The nine main studies they focus on cover the cities of Minneapolis; Jersey (USA); St Louis; Kansas; Houston and Beenleigh (in Brisbane, Australia). Among these, five studies consider possible displacement effects, typically by looking at crime in closely adjacent areas (including the block-level). However, none of these studies were able to uncover systematic displacement effects – one St Louis drug market study did find displacement effects in one location but not in two others.

In this paper we contribute to this topic by considering the displacement effects of a large-scale police intervention that occurred in London in 2005 following the terror attacks that hit the city in July of that year. In contrast to the Jacob et al (2007) study, the change in the relative costs of crime that we consider is based explicitly on a policy intervention. This policy intervention – stylishly dubbed "Operation Theseus" by the London Metropolitan Police – was implemented as part of a general security response to the terrorist attacks that occurred in London during July 2005. The intervention lasted six weeks and involved a major, highly visible police deployment that was geographically concentrated in five central London boroughs. As our paper on the direct crime effects (Draca, Machin and Witt, 2008) establishes the intervention had a clear, direct impact on crime in the boroughs "treated" by the police deployment. The 34% increase in police in these boroughs was accompanied by a 13% fall in susceptible crimes.[2] Furthermore, this fall in crime was not due to other observable and unobservable shocks associated with the terrorist attacks (for example, change in transport usage patterns after the attacks that could have shifted the supply of potential victims for crime).

[2] "Susceptible crimes" in this case are all those crimes that would have been plausibly affected by the public deterrence effects of street-level police deployment. These include all crimes in the major categories of Theft and Handling; Violence and Sexual Offences; and Robbery. See Draca, Machin and Witt (2008) for more details.

Our contribution in this paper is not only to present evidence on the direct connections between crime and police, but also to investigate the indirect effects through potential displacement. We test for spatial displacement of crime from the treated boroughs into neighbouring comparison group boroughs and for intertemporal displacement of crimes within the treatment group by looking at crime patterns in the immediate aftermath of the policy-on period. Despite the clear and well-identified direct effects of the policy intervention, we are unable to find evidence of significant displacement. This suggests that - at least at the geographical level we are considering here - crime displacement effects do not offset the direct effects of police interventions.

The rest of the paper is structured as follows. In Section 2 we discuss the issue of crime modelling with respect to the direct and indirect effects of increased police presence and the big increase in police deployment in London induced by Operation Theseus. Section 3 presents our empirical models of the direct and indirect effects on crime following the increased police presence. Section 4 concludes.

2. Crime, Police and Displacement

In this section we provide a more detailed discussion of crime displacement and give a short overview of the policy intervention at the centre of our analysis. Our paper on the crime and police relation before and after the July 2005 terror attacks (Draca et al, 2008) discusses the estimation and interpretation of the direct effects of this intervention in much more detail and formally analyses the intervention as a quasi-experiment.

Crime and Police

In line with the empirical strategy we adopt below, we discuss modelling issues on the determinants of crime using areas (in our case London boroughs) as the unit of analysis. Consider a general description of an area-level crime function:

$$C_{jt} = C(X_{jt}, P_{jt}, \mu_j, \tau_t, \upsilon_{jk}) \tag{1}$$

where C_{jt} is crime in area j at time t, X_{jt} is a vector of relevant area characteristics for determining crime, and P_{jt} is the level of police resources. The final three terms are μ_j (fixed unobserved area characteristics), τ_t (common time shocks across areas), and υ_{jk} (seasonal shocks specific to the area with k indexing the season).

A regression analogue of (1) can then be written as:

$$C_{jt} = \alpha + \delta P_{jt} + \lambda X_{jt} + \mu_j + \tau_t + \upsilon_{jk} + u_{jt} \tag{2}$$

where the terms are defined as before and u_{jt} is a stochastic error. As is well known crime is highly persistent over time and so it is natural to seasonally difference (2) to give:

$$\Delta_k C_{jt} = \alpha + \delta \Delta_k P_{jt} + \lambda \Delta_k X_{jt} + \Delta_k \tau_t + \Delta_k \varepsilon_{jt} \tag{3}$$

where Δ is the differencing operator with k indexing the order of the seasonal differencing. Note that the $\Delta_k \tau_t$ difference term can now be interpreted as the year-on-year change in factors that are common across all of the areas.

This estimating equation is useful to characterize both the direct and indirect crime effects of an increased in police presence, P_{jt}:

i) Direct effects – The direct effects of an increase in police presence are clear and the parameter δ gives the direct impact of police on crime. If the crime and police variables are specified in logarithms δ is the elasticity of crime with respect to police. The difficult empirical issue in estimating equation (3) is to ensure the causality runs from police to crime (and not vice-versa). Below we not only review estimated elasticities from studies that adopt instrumental variable (IV) strategies to try and ensure that δ picks up the causal impact of police on crime, but also present our own IV estimates using the July 2005 terror attacks of London to identify the crime-police relation.

ii) Indirect effects – The indirect effects are more complex since they rely on displacement of some kind in response to an increased police presence. We consider two possibilities. The first is spatial displacement. As will be made clear below we identify the impact of police on crime by considering what happened to crime in areas where a sizable increase in police presence occurred as compared to areas where this did not happen. If criminals choose to relocate their criminal activities from the first to the second set of areas then spatial displacement will occur. The second possibility is temporal displacement. In this case criminals will still engage in crime in the same areas but will shift their activities to a different time period when the increased police presence does not occur. Thus temporal displacement will occur if this dynamic notion of criminal behaviour applies.

Of course, if these indirect effects do occur then δ will not accurately measure the crime-police relation. Note, however, that if crime rises in an adjacent comparison group area because of spatial displacement we are likely to be under-estimating the direct impact of police on crime. That is, by (indirectly) increasing crime in a comparison area the displacement effect will reduce the empirically measured effect of police on crime. In contrast, temporal displacement is likely to impart an upward bias on the direct estimate of police on crime. In this case, the temporal displacement effect causes an "extra" fall in crime during the policy-on period. This extra fall will then be offset by an increase in crime in subsequent periods when the policy is switched off. Empirically, this offsetting effect could become evident as a significant increase in crime for a treated area in the wake of the policy-on period. It is therefore important to consider possible indirect effects that occur through displacement in evaluating and interpreting a given estimate of δ.

Operation Theseus and the July 2005 Terror Attacks

In practical terms, the δ parameter is estimated by a difference-in-difference strategy centred on a group of London boroughs treated by a heavy police deployment. This deployment occurred in the six weeks following the terrorist attack of July 7th 2005. This attack involved the detonation of three bombs on London Underground train carriages near the tube stations of Russell Square (in the borough of Camden); Liverpool Street (in Tower Hamlets) and Edgware Road (in Kensington and Chelsea). A fourth bomb was detonated on a bus in Tavistock Square, Bloomsbury (in Camden). A second wave of attacks occurred two weeks later on the 21st July and consisted of four unsuccessful attempts at detonating bombs on trains near the underground stations of Shepherds Bush (Kensington and Chelsea); the Oval (Lambeth); Warren Street (Westminster) and on a bus in Bethnal Green (Tower Hamlets). Despite the failure of the bombs to explode, this second wave of attacks caused much turmoil in London. There was a large manhunt to find the four men who escaped after the unsuccessful July 21 attacks and all of them were captured by 29th July.

In response to these attacks the London Metropolitan Police intensified their police patrols and greatly increased their public presence at transport nodes (particularly Tube stations) and other sites of public importance. This extra deployment was achieved in various ways, including extending police overtime for approximately six weeks. Furthermore, the deployment was concentrated in the five boroughs of Westminster, Camden, Kensington and Chelsea, Tower Hamlets and Islington (see Figure 1 for a map). This deployment involved a 34% increase in police hours worked in these boroughs relative to the same period in the previous year.

Figure 2 plots police hours worked for this group of treated boroughs against all other London boroughs (the comparison group used in Draca et al, 2008). Also, in Table 1 we report the changes pre and post-policy levels of police and crime for different groups of boroughs. The striking thing to note from Table 1 is the composition of the relative change in police hours for the treatment group. That is, police hours increased for the treatment group by 37.6% in year-on-year terms but stayed constant for only increased by 3% for all the remaining boroughs. Furthermore, even when we break up the comparison group into smaller sets of boroughs (which is what we do to consider the possibility of spatial crime displacement) it is clear that there the comparison boroughs did not suffer an absolute fall in police resources during Operation Theseus. This was made possible firstly by the increase in overtime hours worked across the Metropolitan Police and secondly by a reallocation of resources across boroughs. Specifically, extra hours worked in the comparison group boroughs were committed to a "central aid" policy where officers assisted in the security operation underway in the treated boroughs.

As a result the Metropolitan Police were able to avoid a situation where resources were allocated on a "zero-sum" basis whereby the absolute levels of resources could have declined in the comparison group. This simplifies our framework in that it represents a much cleaner change in the relative costs of crime than would be the case if the comparison group was subject to absolute falls in police resources.

Data

As in Draca et al (2008) we use daily police reports of crime from the London Metropolitan Police Service (LMPS) before and after the July 2005 attacks. Our crime data cover the period from 1st January 2004 to 31st December 2005 and are aggregated up from ward to borough level and from days to weeks over the two year period.

The basic street-level policing of London is carried out by 33 Borough Operational Command Units (BOCUs), which operate to the same boundaries as the 32 London borough councils apart from one BOCU which is dedicated to Heathrow Airport. The BOCUs are the units that Londoners know as their local police. We have been able to put together a weekly panel covering 32 London boroughs over two years giving 3,328 observations. Crime rates are calculated on the basis of population estimates at borough level, supplied by the Office of National Statistics (ONS) online database.

The police deployment data are recorded at borough level and were produced under special confidential data-sharing agreements with the LMPS. The principal data source used is CARM (Computer Aided Resource Management), the police service's human resource management system. This records hours worked by individual officers on a daily basis. We aggregate to borough-level data on deployment since the CARM data is mainly defined at this level. However, the CARM data contain useful information on the allocation of hours worked by incident and/or police operation. While hours worked are available according to officer rank our main hours measure is based on total hours worked by all officers adjusted for this reallocation effect. Finally, we use data on local labour market conditions from the UK Labour Force Survey (LFS).

3. Empirical Models and Results

Estimating Direct and Indirect Effects

Before discussing the modeling of displacement it is necessary to discuss the identification of δ, the parameter measuring impact of police on crime in equation (3) above. A straightforward OLS estimate of this parameter will be affected by severe endogeneity bias since pre-existing crime patterns influence the allocation of police. In Draca, Machin and Witt (2008) we tackle this problem by using the structure of the Operation Theseus intervention to define an instrumental variable strategy. Specifically, we use the fact that the extra police deployment was concentrated in five central London boroughs to posit a treatment group of heavily affected boroughs, T_b. We then interact this with a "policy on" term ($POST_t$) for the six week duration of the intervention, estimating reduced form equations from police deployment and crime as follows:

$$p_{bt} - p_{b(t-52)} = \alpha_1 + \beta_1 POST_t + \delta_1 (POST_t * T_b) + \lambda_1 (x_{bt} - x_{b(t-52)}) + (u_{1bt} - u_{1b(t-52)}) \quad (4)$$

$$c_{bt} - c_{b(t-52)} = \alpha_2 + \beta_2 POST_t + \delta_2 (POST_t * T_b) + \lambda_2 (x_{bt} - x_{b(t-52)}) + (u_{2bt} - u_{2b(t-52)}) \quad (5)$$

where lower case letters denote logs and the data is seasonally differenced across the same weeks of the year (represented by the t-52 subscript in the differences).

The analogous structural equation for these reduced forms is:

$$c_{bt} - c_{b(t-52)} = \alpha_3 + \beta_3 POST_t + \delta_3 (p_{bt} - p_{b(t-52)}) + \lambda_3 (x_{bt} - x_{b(t-52)}) + (u_{3bt} - u_{3b(t-52)}) \quad (6)$$

The structural parameter δ_3, the causal impact of police on crime, is then recovered from the reduced forms as the ratio of the two reduced form coefficients $\delta_3 = \delta_2/\delta_1$.

Incorporating displacement into this estimating framework basically involves consideration of spatial and temporal effects. In the case of spatial displacement we do this by defining different groups of boroughs immediately around the treatment group as pseudo-treatment groups that could have plausibly been subject to indirect effects. That is, as nearby boroughs these groups would have been most vulnerable to the change in the relative levels of police between the treatment and comparison groups. We therefore interact a dummy for these various definitions of pseudo-treatment group with the six week policy-on term in an extended reduced-form for crime:

$$\Delta c_{b52} = \alpha_4 + \beta_4 POST_t + \delta_4(POST_t{*}T_b) + \theta_{SD}(POST_t{*}SD_b) + \lambda_4(x_{bt} - x_{b(t-52)}) + (u_{4bt} - u_{4b(t-52)}) \quad (7)$$

where SD_b is an indicator for whether a borough is part of the pseudo-treatment group that could be subject to indirect spatial displacement effects of the policy intervention.

In a similar fashion we can test for intertemporal displacement by looking whether crime rose significantly in treatment boroughs in the weeks after Operation Theseus was completed. To do so we use the following equation:

$$\Delta c_{b52} = \alpha_5 + \beta_5 POST_t + \delta_5(POST_t{*}T_b) + \theta_{TD}(TD_t{*}T_b) + \lambda_5(x_{bt} - x_{b(t-52)}) + (u_{5bt} - u_{5b(t-52)}) \quad (8)$$

where TD_t is a dummy variable measuring the weeks after the operation that can be used to look for possible temporal displacement in individual weeks in the post-policy period when police deployment fell back to pre-attack levels.

Descriptive Statistics

Table 1 shows some descriptive statistics on crime and police before and after the terror attacks. It shows a sharp rise in police deployment in the treatment groups in the six weeks following the first round of terror attacks (a 38 percent increase in hours worked per 1000 population, rising from 169.46 to 242.29). In the full comparison group of 27 boroughs there was barely any change (going from 82.77 to 84.95). At the same time crime fell significantly in the five treatment boroughs (by around 13%) whilst there was no change in the comparison boroughs. Thus crime fell significantly in the treatment group relative to the control group (by 12.9% in the difference-in-difference given in the final row of the Table).

For exploring possible spatial displacement effects, the Table also shows what happened to crime and police for three groups of possible pseudo-treatment boroughs – a group for all of Inner London (as per the definition given by the Office of National Statistics, labelled “Inner”); a group of all those boroughs bordering the treatment group (“Adjacent”); and the five boroughs closest to the treatment group (“Central”). These are the different definitions of SD_b from equation (7) that we consider in our empirical analysis. The unconditional pre-period and post-period statistics for these different groups are given in Table 1. As we have already noted, in terms of police hours there was very little change for any of our proposed pseudo-treatment boroughs. Similarly, these unconditional statistics do not show any evidence of the increase in crime that would be expected if the police intervention was displacing criminal activity from the treatment group into nearby boroughs.

Estimates of the Direct Impact of Crime

Table 2 provides the basic reduced form OLS and structural IV results for the causal crime-police models outlined in equations (4)-(6) above. For comparative purposes, we specify three T*Post-Attack terms to evaluate the interaction term $POST_t*T_b$. Specifically, in columns (1) and (4) we include an interaction term that uses the full period from July 7th 2005 to December 31st 2005 to measure the post-attack period. The adjacent columns (i.e. (2)-(3) and (5)-(6)) then split this period in two with one interaction term for the six-week Operation Theseus period (denoted T*Post-Attack1) and another for the remaining part of the year (T*Post-Attack2). The second term is therefore useful for detecting any persistent effects of the police deployment or indeed any longer term trends or possible temporal displacements in the treatment group.

The findings from the unconditional DiD estimates reported earlier are confirmed in the basic models in Table 2. The estimated coefficient on T*Post-Attack1 in the reduced form police equation shows a 34.1% increase in police deployment during Operation Theseus, and there is no evidence that this persists for the rest of the year (i.e. the T*Post-Attack2 coefficient is statistically indistinguishable from zero). For the crime rate reduced form there is an 13.1% fall during the six-week policy-on period with minimal evidence of either persistence or a treatment group trend in the estimates for the T*Post-Attack2 variable.[3]

The coincident nature of the respective timings of the increase in police deployment and the fall in crime suggests that the increased security presence lowered crime.[4] The final two columns of the Table therefore show estimates of the causal impact of increased deployment on crime. Column (9) shows the basic IV estimate where the post-attack effects are constrained to be time invariant. Column (10) allows for time variation to identify a more local causal impact. The Instrumental Variable estimates are precisely determined owing to the strength of the first stage regressions in the earlier columns of the Table. The preferred estimate with time-varying terror attack effects (reported in column (10)) shows an elasticity of crime with respect to police of around -

[3] Whilst we have seasonally differenced the data one may have concerns about possible contamination from further serial correlation. We follow Bertrand et al (2004) and collapse the data before and after the attacks and get extremely similar results.

[4] Draca, Machin and Witt (2008) subject this finding of coincident timing to a number of checks of robustness, including ruling out coincident observable and unobservable correlated shocks that could give concern to interpreting the crime fall as due to increased police presence.

.38. This implies that a 10 percent increase in police activity reduces crime by around 3.8 percent.

OLS estimates are reported in columns (7) and (8) for comparison. The column labelled 'levels' estimates a pooled cross-sectional regression resulting in a high, positive coefficient on the police deployment variable. In column (8) we estimate a seasonally-differenced version of this OLS regression getting a negligible, insignificant coefficient. This reflects the fact there is limited year-on-year change in police hours to be found when the seasonal difference is taken, and also that the causal IV estimates does resolve the issue of upward bias in the estimated police effect resulting from reverse causation in the OLS estimates.

The paper by Draca, Machin and Witt (2008a) discusses potential threats to the identification of this direct effect in more detail. The main issue is the potential role of "correlated shocks" in influencing the fall in crime. In particular, changes in activity around the city (for example, the travel patterns of commuters) could have influenced the supply of victims and opportunities for crime to occur. In this companion paper we consider measures of activity (specifically the volume of Tube journeys in London) and find that the changes in travel patterns do not follow in line with the change in the police deployment or crime. Hence the main argument developed there is that the movements in police and crime match each other in a way that cannot be explained by other observable or unobservable shocks associated with the terrorist attacks.

Estimates of the Indirect Impact of Crime – Spatial Displacement

The results using different control groups to explore possible spatial displacement are reported in Table 3. The possible displacement effects for the six-week period during Operation Theseus are reported in first row as Area*Post-Attack1, where Area is a dummy variable for each our pseudo-treatment SD_b definitions. In the second row we interact the Area dummy with a time dummy for all the weeks after Operation Theseus from late August until the end of 2005. This is done to test for potential long-term persistence effects. Similarly, the direct effects of the intervention are given in the rows labeled T*Post-Attack1 and T*Post-Attack2. It is clear from these conditional estimates that there are no significant, positive displacement effects – in fact, the coefficients are estimated to be slightly negative.

Estimates of the Indirect Impact of Crime – Temporal Displacement

The fact that the timing of the police increases and crime falls go hand in hand is in line with the idea that temporal displacement did not occur. In Table 2 there is a sharp rise in police deployment in the six weeks after the first round of attacks (as shown by the significant positive coefficient on T*Post-Attack 1) which then falls back to pre-attack levels for the rest of 2005 (as shown by the insignificant coefficient on T*Post-Attack 1). The same is true of crime where the estimated coefficient on T*Post-Attack 1 is significant and negative, yet the estimated coefficient on T*Post-Attack 2 is insignificantly different from zero.

The common timing of police increases and crime falls thus seems inconsistent with temporal displacement by criminals. This is considered in more detail in Figure 3 (taken from Draca et al, 2008). In this Figure we estimate the treatment group and week interaction for every week. These week-by-week effects are given in Figure 3(a) for police hours and 3(b) for crime. This is effectively a placebo test for potential "policy" effects outside of the six-week period of the intervention. Following the intertemporal displacement hypothesis a post-policy rebound or "smoothing" response by criminals would be evident in the weeks after Operation Theseus. However, there is no evidence that crime increased significantly in year-on-year terms for the weeks after the policy was switched off. (Note here that significant effects in these figures occur in cases where the standard error bars do not overlap the zero line). This is in contrast to the direct effects during the policy-on period which are significant for each week of the intervention.

Comparison of Estimated Effects With Those in the Literature

Therefore it seems that our estimated direct effects are not contaminated by spatial or temporal displacement. Moreover, the magnitudes of our causal estimates are similar to the small number of causal estimates found in the literature. They are also estimated much more precisely in statistical terms because of the very sharp discontinuity in police deployment that occurred.

Table 4 reports estimates from the other causal studies we know of. For example, Levitt's (1997) study found elasticities in the -0.43 to -0.50 range, while Corman and Mocan (2000) estimated an average elasticity of -0.45 across different types of offences. The papers based upon terror attacks (Di Tella and Schargrodsky, 2004, and Klick and Tabarrok, 2005) report elasticities in this range. Our results are certainly qualitatively similar, with our preferred result being -0.38. This coincidence of estimates in very different contexts is strongly supportive of the external validity of these studies.

4. Conclusions

In this paper we have presented causal estimates of the impact of police on crime and tested for possible spatial and intertemporal displacement effects in the context of a major police intervention in London during July-August 2005. This intervention had clear direct effects on crime in the areas heavily treated by a highly visible police deployment. The structure of the intervention also induced a very clean change in the relative costs of crime – police deployment levels in the comparison group boroughs were held constant thereby avoiding the possibility that crime could have fallen due to an absolute fall in police. However, our tests of spatial and intertemporal displacement deliver an emphatic null result. At least at the level of aggregation we consider here (weekly, borough-level) the Operation Theseus intervention did not generate significant indirect displacement effects in addition to its direct crime reducing effects.

As this last comment implies our results do not rule out the possibility that displacement effects may have had a role at a more disaggregrated level. Our tests for spatial displacement are effectively tests for between-borough displacement. We are unable to test for within-borough displacement arising from the allocation of police inside the treatment group boroughs. For example, less heavily treated parts of the treatment boroughs may experienced increases in crime relative to more heavily treated areas. However, as we point out in Draca et al (2008), this would lead to a downward bias on our estimates of the direct effects of the intervention.

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Figure 1: A Map of London Boroughs

Figure 2: Police Deployment, Year-on-Year Changes 2004-2005, Treatment versus Comparison Group.

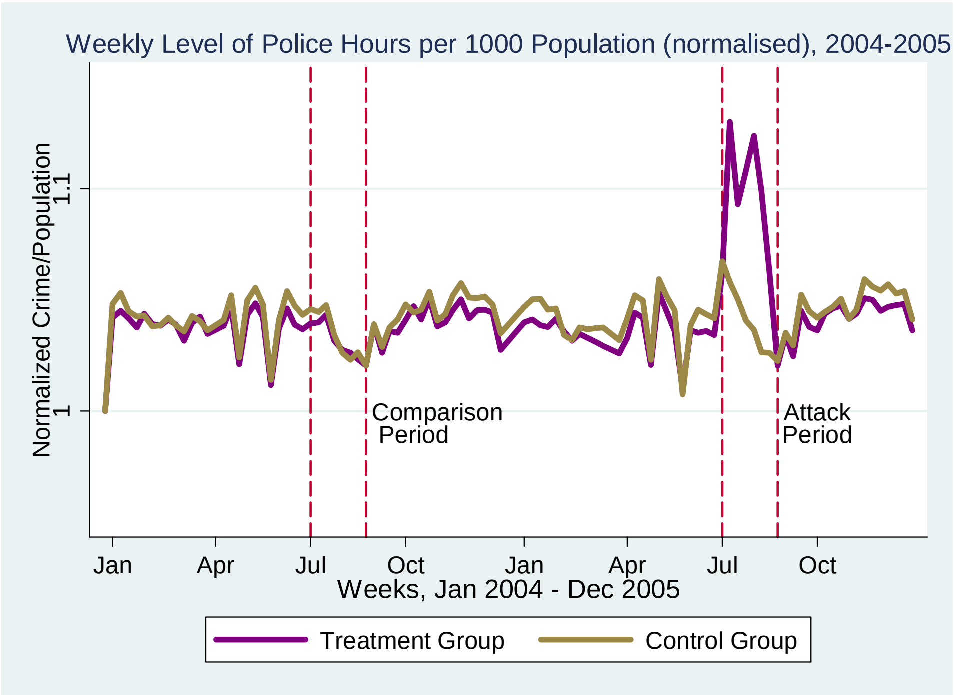
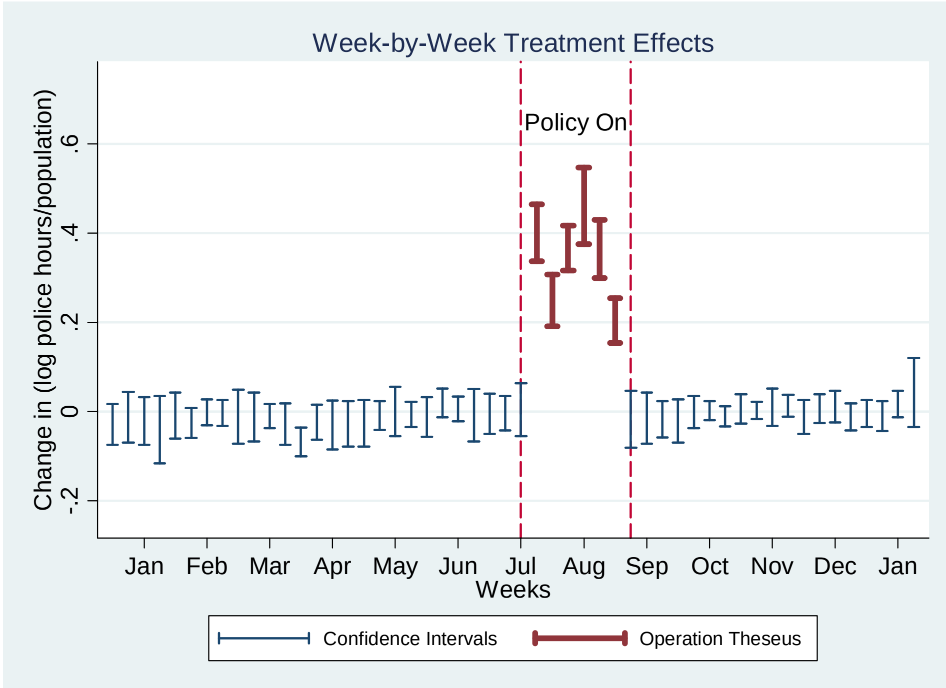


**Figure 3: Week-by-Week Policy Effects,
Borough Level Models, 2004-2005.**

(a) Police Deployment – ln(Police Hours / Population)



(b) Susceptible Crimes - ln(Crimes / Population)

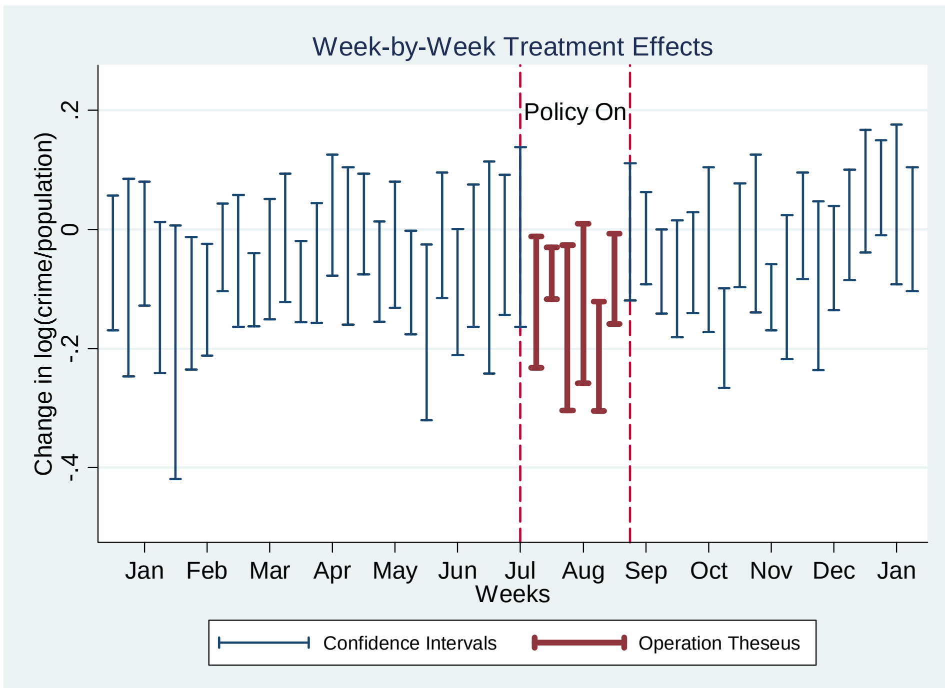


TABLE 1: OPERATION THESEUS – YEAR-ON-YEAR CHANGES IN POLICE AND CRIME,

	(A) Police Deployment (Hours worked per 1000 Population)			*(B) Crime Rate (Susceptible Crimes Per 1000 Population)*		
	(1) Pre-Period	(2) Post-Attack1	(3) Difference (logs)	(4) Pre-Period	(5) Post-Attack1	(6) Difference (logs)
Treatment Group						
Treatment Group (5)	169.46	242.29	0.376 (0.029)	3.30	2.89	-0.134 (0.030)
Comparison Groups						
All Comparison (27)	82.77	84.95	0.030 (0.013)	1.41	1.40	-0.004 (0.015)
Inner (8)	113.71	116.26	0.023 (0.015)	1.85	1.84	-0.008 (0.029)
Adjacent (7)	111.28	112.83	0.014 (0.015)	1.80	1.78	-0.003 (0.029)
Central (10)	121.86	123.75	0.015 (0.026)	1.99	1.93	-0.023 (0.045)
Outer (19)	69.79	71.81	0.033 (0.018)	1.22	1.22	-0.002 (0.019)
Difference-in-Difference (Treatment–All Comparison)			0.346 (0.028)			-0.129 (0.031)

Notes: Treatment group defined as boroughs of Westminster, Camden, Islington, Tower Hamlets and Kensington-Chelsea. Inner London boroughs defined following the ONS classification as: Westminster, Camden, Islington, Kensington and Chelsea, Tower Hamlets (Treatment Group) and Hackney, Hammersmith & Fulham, Haringey, Wandsworth, Lambeth, Lewisham, Southwark and Newham (Comparison Group). Adjacent boroughs defined as: Brent, Hackney, Hammersmith & Fulham, Lambeth, Newham, Southwark and Wandsworth. Central Ten boroughs defined as: Westminster, Camden, Islington, Kensington and Chelsea, Tower Hamlets (Treatment Group) and Brent, Hackney, Hammersmith & Fulham, Lambeth and Southwark. Post-Attack1 represents the six weeks after July 7th 2005 while Pre-Period covers the equivalent weeks 12 months before. "Susceptible crimes" in this case are all those crimes that would have been plausibly affected by the public deterrence effects of street-level police deployment. These include all crimes in the major categories of Theft and Handling; Violence and Sexual Offences; and Robbery.

TABLE 2: DIRECT CRIME EFFECTS OF OPERATION THESEUS.

	(A) Police Deployment (Hours Worked Per 1000 Population)			*(B) Susceptible Crimes (Crime per 1000 Population)*			*(C) OLS*		*(D) IV Estimates*	
	Full (1)	Split (2)	+Controls (3)	Full (4)	Split (5)	+Controls (6)	Levels (7)	Differences (8)	Full (9)	Split (10)
T*Post-Attack	0.081 (0.010)			-0.056 (0.023)						
T*Post-Attack 1		0.341 (0.028)	0.342 (0.028)		-0.131 (0.031)	-0.131 (0.030)				
T*Post-Attack 2		-0.000 (0.010)	0.001 (0.010)		-0.035 (0.029)	-0.035 (0.030)				
ln(Police Deployment)							0.738 (0.053)	-0.031 (0.050)	-0.692 (0.288)	-0.382 (0.089)
Controls	No	No	Yes	No	No	Yes	Yes	Yes	Yes	Yes
No of Boroughs	32	32	32	32	32	32	32	32	32	32
No of Observations	1664	1664	1664	1664	1664	1664	1664	1664	1664	1664

Notes: Taken from Draca, Machin and Witt (2008). All specifications include week fixed effects. Clustered standard errors in parentheses. Boroughs weighted by population. Post-period for baseline models (1) and (5) defined as all weeks after 7/7/2005 until 31/12/2005 attack inclusive. Weeks defined in a Thursday-Wednesday interval throughout to ensure a clean pre and post split in the attack weeks. T*Post-Attack is then defined as interaction of treatment group with a dummy variable for the post-period. T*Post-Attack1 is defined as interaction of treatment group with a deployment "policy" dummy for weeks 1-6 following the July 7th 2005 attack. T*Post-Attack2 is defined as treatment group interaction for all weeks subsequent to the main Operation Theseus deployment. Treatment group defined as boroughs of Westminster, Camden, Islington, Tower Hamlets and Kensington-Chelsea. Police deployment defined as total weekly hours worked by all police staff at borough-level. Controls based on Quarterly Labour Force Survey (QLFS) data and include: borough unemployment rate, employment rate, males under 25 as proportion of population, and whites as proportion of population (following QLFS ethnic definitions).

TABLE 3: SPATIAL DISPLACEMENT EFFECTS OF OPERATION THESEUS.

	(1) Inner	(2) Adjacent	(3) Central
Area*Post-Attack1	-0.007	-0.001	-0.027
	(0.033)	(0.032)	(0.043)
Area*Post-Attack 2	0.011	-0.007	-0.029
	(0.024)	(0.021)	(0.021)
T*Post-Attack1	-0.133	-0.132	-0.138
	(0.032)	(0.032)	(0.031)
T*Post-Attack 2	-0.031	-0.037	-0.041
	(0.031)	(0.031)	(0.030)
Controls	Yes	Yes	Yes
No. of Boroughs	32	32	32
No of Observations	1664	1664	1664

Notes: Standard errors clustered by borough in parentheses. All regression include week fixed effects/ Boroughs weighted by population. Weeks defined in a Thursday-Wednesday interval throughout to ensure a clean pre and post split in the attack weeks. T*Post-Attack is then defined as interaction of treatment group with a dummy variable for the post-period. T*Post-Attack1 is defined as interaction of treatment group with a deployment "policy" dummy for weeks 1-6 following the July 7^{th} 2005 attack. T*Post-Attack2 is defined as treatment group interaction for all weeks subsequent to the main Operation Theseus deployment. Area*Post-Attack1 and Area*Post-Attack2 are dummies for the pseudo-treatment boroughs interacted with the Post-Attack time dummies. Definitions of these areas are given in the notes to Tabl1. Controls based on Quarterly Labour Force Survey (QLFS) data and include: borough unemployment rate, employment rate, males under 25 as proportion of population, and whites as proportion of population (following QLFS ethnic definitions).

TABLE 4: IV ESTIMATES OF THE POLICE-CRIME RELATIONSHIP

Study	IV Strategy	Estimates
Levitt (1997)	Using timing of gubernatorial elections as an instrument for police expenditure and hiring.	Violent Crime: IV estimates of -1.0 compared to OLS estimates of -0.3 (approximate elasticities). Property Crime: IV estimates of -0.4 compared to OLS estimates of -0.2 (approximate elasticities).
Corman and Mocan (2000)	Uses a long 30-year monthly time-series on crime and the number of police officers in New York. Impact of police estimated by using lagged values.	Elasticity of -0.5 for robbery and -0.4 for burglary (approximate).
Di Tella and Schargrodsky (2004)	Used deployments around potential terrorist targets in Buenos Aires as a source of exogenous variation in police presence.	Elasticity of -0.33 with an estimate of -0.17 using the most conservative assumptions.
Klick and Tabarrok (2005)	Use changes in terror alert levels in Washington to infer a variation in police deployment.	Authors estimate a 15% fall in crime in high terror alert periods. Provide an estimate of the police crime elasticity as -0.3 (approximate).