Panic on the Streets of London: Police, Crime and the July 2005 Terror Attacks

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Abstract

In this paper we contribute to two relatively small, but growing, literatures of high public policy relevance on the causal impact of police on crime and the economics of terrorism. We bring the two together by looking at what happened to crime before and after the terror attacks that hit central London in July 2005. The attacks resulted in a large redeployment of police officers to central London boroughs as compared to outer London – in fact, police deployment in central London increased by over 30 percent in the six weeks following the July 7 bombings. In this time period, crime fell significantly in central relative to outer London. Study of the timing of the crime reductions and their magnitude, which types of crime were more affected and a series of robustness tests looking at possible biases all make us confident our research approach identifies a causal impact of police on crime. Implementing an instrumental variable approach shows an elasticity of crime with respect to police of approximately -.3, so that a 10 percent increase in police activity reduces crime by around 3 percent.

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1. Introduction

Terrorism is arguably the single most significant topic of political discussion in the current decade. In response, a small economic literature has begun to investigate the causes and impacts of terrorism (see Krueger, 2006, for a summary or Krueger and Maleckova, 2003 for some empirical work in this area). Terror attacks, or the threat of them, have been considered in research on one important area of public policy, namely the connections between crime and policing. Some recent studies (such as Di Tella and Schargrodsky, 2004 and Klick and Taborrak, 2005) have used terrorism-related events to look at the police-crime link since terror attacks induce increased police presence and this can be used to test whether or not increased police reduce crime (the former paper looks at what happened to crime when intensified police presence occurred around religious buildings in Buenos Aires following a terrorist attack, and the latter uses terror alert levels in Washington DC to make inferences about the police-crime relationship).

In this paper we also consider the crime-police relationship before and after a terror attack, but in the very different context of the increased security presence following the terrorist bombs that hit London in July of 2005. Our application is a much more general one than the other studies in that it covers a large metropolitan area following one of the most important and widely known terror attacks of recent years. Indeed, the scale of the security response in London at this time provides a good setting to examine the relationship between police and crime. Moreover, and unlike the other studies in this area, we have very good data on police deployment and can use these to identify the magnitudes of the causal impact of police on crime.[1] In fact the sharp discontinuity in

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[1] Neither Di Tella and Schargrodsky (2004) or Klick and Tabarrok (2005) have data on police activity.

police deployment that occurred immediately after the July bombings means we are able to very precisely pin down this causal relation.

A crucial part of identifying a causal impact in this type of setting is establishing the exclusion restriction which shows that terrorist attacks affect crime through the post-attack police deployment rather than via other observable and unobservable factors correlated with the attack or shock. We are able to make considerable advances on the other terror attack work since we have pre- and post terror attack data (unlike Klick and Tabarrok, 2005) and because we observe data on the change in police activity after the bombings occurred.

The use of this strong research design is important since the crime-police relation has received a lot of attention over the years, and is in many ways a problematic area. Surveys of empirical research on police and crime (e.g. Cameron, 1988; Marvell and Moody, 1996; Eck and Maguire, 2000) report that the majority of studies fail to find any relationship and there is even a positive association between the two that emerges in some studies. This is because most of the existing work faces difficulties in attempting to unravel a causal relationship between police resources and crime.

A small, but growing research area does, however, directly address the question of causality. Probably the best known paper here is Levitt's (1997) study of US cities which tries to resolve the endogeneity issue via an instrumental variable[2] strategy using election years as instrument for police in a crime equation. In doing so he identifies a causal negative effect running from police to crime, but this work is controversial for a

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[2] Identification is achieved by including an election year indicator in the police equation, but by excluding it from the crime equation.

number of reasons (see McCrary, 2002, who discusses some worries with the data and approach used in the Levitt paper and Levitt's, 2002, response).

Some of the other work trying to identify a causal impact of police on crime adopts a quasi-experimental approach looking at what happens before and after a policy or event induced increase in police. The Di Tella and Schargrodsky (2004) paper referred to above shows that motor vehicle thefts fell significantly near to the main Jewish centre in Buenos Aires where a terrorist attack occurred in July 1994 as compared to several blocks away where no extra police were deployed. This piece of research is, of course, highly specific in nature and also is only able to take a reduced form approach since the authors do not have data on police activity before and after the attack.

Another strand of work considers policy initiatives where particular police forces were given more resources to combat crime (the Street Crime Initiative in England and Wales, studied by Machin and Marie, 2004, and the Community Oriented Policing Strategies programme in the US, studied by Evans and Owens, 2006). Both of these studies adopt a treatment-control type programme evaluation approach (where treatment areas received extra resources and control areas did not) and find that extra police resources reduced crime. The difficulty with these papers is that high crime police force areas were selected to be given more resources and it is difficult to be totally confident that the analyses remove all the biases associated with this.

The focus of the current paper is on what happened to criminal activity when a big unanticipated increase in police presence occurred. The scale of the change in police deployment that we study is much larger than any of the other work in the crime-police research field. Indeed, results reported below show that police activity in central London increased by over 30 percent in the six weeks following the July 7 bombings. In this time period, crime fell significantly in central London relative to outer London. The timing of the crime reductions and their magnitude, study of which crimes were more affected and a battery of robustness tests that we present, make us confident that this research approach identifies a causal impact of police on crime. We estimate an elasticity of crime with respect to police of approximately -.3, so that a 10 percent increase in police activity reduces crime by around 3 percent.

The remainder of the paper is organized as follows. Section 2 describes the events of July 2005 and goes over the main modeling and identification issues in the paper. We provide a recap of the endogeneity problem in the police-crime relationship and discuss the problem of correlated shocks in more detail. An important part of this is a consideration of insights from the growing economics of terrorism literature. In section 3 we outline the data used and provide some initial descriptive analysis. Section 4 then presents the statistical results, and a whole range of additional empirical tests. Section 5 concludes.

2. Crime, Police and the London Terror Attacks

The Terror Attacks

In July of 2005, London's public transport system was subject to two waves of terror attacks. The first wave occurred on Thursday July 7th and involved the detonation of four bombs. The 32 boroughs of London are shown in Figure 1. Three of the bombs were detonated 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). The second wave of attacks occurred two weeks later on the 21st of July and involved 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). 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 July 29. As our later descriptive analysis shows the two attack waves were associated with a large increase in police deployment in the affected central London boroughs of approximately 35% in the six weeks following the first attack.

Crime-Police Endogeneity

We use the timing and response to the terror attacks as a means of identifying the impact of police on crime since the weeks following the attacks resulted in a large, unanticipated increase in police presence. In this light it is useful to recall the basic endogeneity problem besetting the police-crime relationship. The problem is straightforward. Basic models of criminal participation (Becker, 1968; Ehrlich, 1973) postulate that crime is a function of opportunities and deterrence (like increased police presence) so that more police deter crime and there should be a negative empirical relationship between the two variables. However, there are many situations where the direction of causation can run in the opposite direction (e.g. when more police are drafted in to high crime areas because crime is high there).[3]

Figure 2 illustrates the problem empirically using data for the police force areas of England and Wales. It shows a regression of the crime rate on police numbers (full-time equivalents) per 1000 population in the financial year (April to March) of 2005-2006. Evidently the cross-sectional relationship is strongly positive. In a regression of log(crime) on log(police) across the 42 police force areas the estimated coefficient (standard error) on the police variable is .81 (.08), showing a strong positive association which is counter-intuitive for the basic economic model of crime. It is therefore very clear that considerable care and attention needs to be placed on issues to do with the direction of causation in the crime-police relation.

Terrorist Attacks, Crime and Correlated Shocks

Di Tella and Schargrodsky (2004) were the first to use police force allocation policies in the wake of terrorist attacks as a source of exogenous variation to circumvent the endogeneity problem described above. Using the case of a July 1994 terrorist attack that targeted the main Jewish institution in Buenos Aires, they show that motor vehicle thefts fell significantly in areas where extra police were subsequently deployed as compared to areas several blocks away which did not receive extra protection. The effect they find is large (approximately a 75% reduction in thefts relative to their comparison group) but also extremely local with no evidence that the police presence reduced crime even one or two blocks away from the protected areas. Another study by Klick and Tabarrok (2005) uses terror alert levels in Washington DC to make inferences about the

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[3] For instance, Levitt (1997) puts it in the following way: 'Higher crime rates are likely to increase the marginal productivity of police. Cities with high crime rates, therefore, may tend to have large police forces, even if police reduce crime [Levitt, 1997, page 270].

police-crime relationship. The deployments they consider cover a more general area but are notional since they are not able to quantify them with data on actual police numbers or hours.

Both of these papers touch on the issue of correlated shocks to observables and unobservables. However, in our London example it would be realistic to think that this could be a larger concern since the terrorist attacks (four detonated bombs and a further four unsuccessful attempts) were a more significant, dislocating event for the city.

In thinking about correlated shocks, it is helpful to first consider a basic equation in levels that describes the determinants of the crime rate in a set of geographical areas (in our case, London boroughs) over time:

$$C_{jt} = \alpha_1 + \delta_1 P_{jt} + \lambda_1 X_{jt} + \mu_j + \tau_t + \upsilon_{jk} + \varepsilon_{jt} \tag{1}$$

where C_{jt} denotes the crime rate for borough j in period t, P_{jt} the level of police deployed in the area and X_{jt} is a vector of control variables that could be comprised of observable or unobservable elements. The next set of terms are : μ_j, a borough level fixed effect; τ_t, a common time effect across boroughs (for example, to capture common weather or economic shocks); and a final term υ_{jk} which represents borough-specific seasonal effects with *k* indexing the season, for example, a number from 1-12 for monthly frequency or 1-52 for a weekly frequency[4].

Now consider a seasonally differenced version of equation (1), where the dependent variable becomes the change in the area crime rate relative to the rate at the same time in the previous year. This is highly important in crime modeling since crime

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[4] These types of effects could prevail where seasonal patterns affect different boroughs with varying levels of intensity. For example, the central London boroughs are more exposed to fluctuations due to tourism activity and exhibit sharper seasonal patterns with respect to crime.

persists very strongly across areas over time. In practical terms, this eliminates the borough level fixed effect and the borough-specific seasonality terms, yielding:

$$(C_{jt} - C_{j(t-k)}) = \alpha_1 + \delta_1(P_{jt} - P_{j(t-k)}) + \lambda_1(X_{jt} - X_{j(t-k)}) + (\tau_t - \tau_{t-k}) + (\varepsilon_{jt} - \varepsilon_{j(t-k)}) \qquad (2)$$

Note that the $\tau_t - \tau_{t-k}$ term can now be interpreted as the year-on-year change in factors that are common across all of the areas. By expressing this equation more concisely we can make the correlated shocks issue explicit:

$$\Delta_k C_{jt} = \alpha_1 + \delta_1 \Delta_k P_{jt} + \lambda_1 \Delta_k X_{jt} + \Delta_k \tau_t + \Delta_k \varepsilon_{jt} \qquad (3)$$

where Δ is the differencing operator with k indexing the order of the seasonal differencing.

In this framework we can carefully consider how a terrorist attack – which we can denote generally as Z - affects the determinants of crime across areas. Following the argument in the papers discussed above the terror attack Z affects ΔP_{jt}, shifting police resources in a way that one can hypothesise is unrelated to crime levels. This hypothesis is, of course, a crucial aspect of identification that needs serious consideration. It is also possible that Z could affect the elements of ΔX_{jt} creating additional channels through which the terrorist attacks could influence the pattern of crime rates.

What are these impacts or channels? The economics of terrorism literature stresses that the impacts of terrorism are sharp but temporary (OECD 2002). Economic activity tends to recover and normalize itself fairly rapidly, with longer term structural impacts occurring in industries such as insurance and international transport. Of course, a sharp, but temporary, shock would still have ample scope to intervene in our identification strategy. In particular, three channels demand consideration. Firstly, there is the physical dislocation caused by the attack. A number of tube stations were closed and many Londoners changed their mode of transport after the attacks (e.g. from the tube to buses or bicycles). This would have reshaped travel patterns and affected the potential supply of victims for criminals in some areas. Secondly, the volume of overall economic activity was affected. Studies on the aftermath of the attack indicate that both international and domestic tourism fell after the attacks, as measured by hotel vacancy rates, visitor spending data and counts of domestic day trips (GLA, 2005). Finally, there is the psychological impact on individuals in terms of their attitudes towards risk. As Becker and Rubinstein (2004) outline, this influences observable travel decisions as well as unobservable behavior.

To summarize, we think of these effects as being manifested in three elements of the X_{jt} vector outlined above:

$$X_{jt} = [X_{jt}^1, X_{jt}^2, \theta_{jt}] \tag{4}$$

In (4), X_{jt}^1 is a set of exogenous control variables, that is, observable factors such as area level labor market conditions that change slowly and are unlikely to be immediately affected by terrorist attacks (if at all). The second X_{jt}^2 vector represents the observable factors that change more quickly and are therefore vulnerable to the dislocation caused by terrorist attacks. As discussed above, we are thinking here primarily of factors such as travel patterns which influence the potential supply of victims across areas. The final element θ_{jt} then captures an analogous set of unobservable factors that are susceptible to change due to the terrorist attack. In the spirit of Becker and Rubinstein's (2004) discussion, the main factor to consider here is fear or how individuals handle the risks associated with terrorism. For example, it is plausible that in the wakes of the attacks commuters in London became more vigilant to suspicious activity in the transport system and in public spaces. This vigilance would have been focused mainly on potential terrorist activity but it is easy to envisage that this type of cautious behaviour could have a spillover onto crime.

The implications of these correlated shocks for our identification strategy can now be clearly delineated. For our exclusion restriction to hold it needs to be shown that the terrorist attack Z affected the police deployment in a way that can be separately identified from Z's effect on other observable and unobservable factors that can influence crime rates. Practically, we show this later in the paper by mapping the timing and location of the police deployment shock and comparing it to the profiles of the competing observable and unobservable shocks.

Displacement Effects and the Response of Criminals

As with other spatially focused analyses, it is possible that the aftermath of the terror attack altered individual behaviour so that criminals, or the potential victims of criminals, geographically altered their activities. The clearest example of this would be displacement of crime away from the areas where increased police presence occurred. We present some tests later on in the paper where we expand the size of the 'terror area' to see if one can uncover any evidence on crime displacement. The other key feature of displacement has already been discussed, namely whether the supply of potential victims alters its geographical configuration. We also devote time to this question in our empirical analysis.

3. Data Description and Initial Descriptive Analysis

Data

We use daily police reports of crime from the London Metropolitan Police Service (LMPS) before and after the July 2005 terrorist attacks. Our data cover the period 1st January 2004 to 31st December 2005 and is aggregated up from ward to borough level and from days to weeks over the two year period. There are 32 London boroughs as shown on the map in Figure 1. There is also monthly borough level data available over a longer time period that we use for some robustness checks.

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 by borough level, supplied by the Office of National Statistics (ONS) online database.

Table 1 (and Appendix Table A1 in more detail) show some basic summary statistics on the crime data. We split the crimes into two groups that we refer to as ‘susceptible’ and ‘non-susceptible’ crimes since there are good reasons to expect potentially different effects on the two. The susceptible crimes we consider are violence and sexual offences, theft and handling and robbery and the non-susceptible crimes are burglary and criminal damage (like vandalism or graffiti). We expect the latter group to be less affected by the increased deployment since they are more prevalent in residential areas or frequently occur at night. The Table shows the breakdown of crime into these different types and the higher crime rate in the central London 'treated' boroughs. This gap is an issue we return to in our empirical specifications below.

The police deployment data are 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 which records hours worked by individual officers on a daily basis. We aggregate to borough-level data on deployment since ward-level data is only recorded for particular programmes and policing initiatives. However, the CARM data contains 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 in the borough adjusted for this reallocation effect.

In addition to crime and deployment, we have also obtained weekly data on tube journeys for all stations from Transport for London (TFL). It is daily borough-level data aggregated up to weeks based on entries into and exits from tube stations. Finally, we also use data from the UK Labour Force Survey (LFS) to provide information on local labour market conditions.

Initial Approach

Our analysis begins by looking at what happened to police deployment and crime before and after the July 2005 terror attacks in London. To do this we start by adopting a difference-in-difference approach, defining a treatment group of boroughs where the extra police deployment occurred and comparing their crime outcomes to the other non-treated boroughs. The police hours data we use facilitates the development of this approach, with two features standing out. Firstly, the data allows us to measure the increase in total hours worked in the period after the attacks. The increase in total hours was accommodated by the increased use of overtime shifts across the police service and this policy lasted approximately six weeks. The deployment policy was stylishly named "Operation Theseus" by the police command structure and we use this title at times in our discussion below. Secondly, the police data contains a special resource allocation code denoted as Central Aid. This code allows us to identify how police hours worked were geographically reallocated over the six-week period. For example, we can identify how hours worked by officers stationed in the outer London boroughs were reallocated to the public security duties in central and inner London. The extra hours were mainly reallocated to the boroughs of Westminster, Camden, Islington, Kensington and Chelsea, and Tower Hamlets. These boroughs either contained the sites of the attacks or featured many potential terrorist targets such as transport nodes or significant public spaces. Using these two features of the data we are able to define a treatment group comprised of the five named boroughs. A map showing the treatment group is given in Figure 1. In most of the descriptive statistics and modeling below we use all other boroughs as the comparison group in order to simplify the analysis.

What did the extra police deployment in the treated boroughs entail? The number of mobile police patrols were dramatically increased and officers were prominently posted to guard major public spaces and transport nodes, particularly Tube stations. In areas of Central London where many stations were located this resulted in a very clear police presence. Table 2 reports the results of a survey of London residents in the aftermath of the attacks. Approximately 70 percent of respondents from Inner London attested to a higher police presence in the period since the attacks. The lower percentage reported by Outer London residents also supports the idea of a differential deployment across areas[5].

Given the high visibility of the deployment we therefore think of it as exerting a deterrent effect on public, street-level crimes such as thefts, violent assault and robbery. We test for this prediction in the later modeling section. As already noted we therefore classify crimes according to whether they were susceptible to a public deterrence mechanism or not.

Basic Differences-in-Differences

We begin our empirical study by looking at what happened to police deployment and to total crime rates before and after the July 2005 terror attacks in the treatment group boroughs as compared to all other boroughs. Police deployment is measured in a similar way to crime rates, that is, we normalize police hours worked by the borough population level. Following the discussion in Section 2 we define the before and after periods in year-on-year, seasonally adjusted terms. This ensures that we are comparing like-with-like in terms of the seasonal effects prevailing at a given time of the year. For example, looking at Table 3 the crime rate of 4.03 in panel B represents the treatment group crime rate in the period from the 8th of July 2004 until the 19th of August 2004. The post-period or "policy on" period then runs from July 7th 2005 until August 18th 2005 with a crime rate of 3.59. Therefore by taking the difference between these "pre" and "post" crime rates we are able to derive the year-on-year, seasonally adjusted change in crime rates

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[5] It must be remembered that the estimates for outer London are biased upwards by the fact that outer London residents commuting into inner London would have witnessed the higher police presence in these locations.

and police hours. These are then differenced across the treatment and comparison groups to get the customary difference-in-difference (DiD) estimate.

The first panel of Table 3 shows the unconditional DiD estimates for police hours. It is very clear that the treatment areas experienced a large relative change in police deployment. Per capita hours worked increased by 34.6% in the DiD (final row, column 3). Arguably, the *composition* of this relative change is almost as important as the scale for our experiment. The relative change was driven by an increase in the treatment group (an additional 72.8 hours per capita) with little change in hours worked for the comparison group (only 2.2 hours more per capita). This was feasible because of the large number of overtime shifts worked. Qualitatively, it means that while there was a diversion of police resources from the comparison boroughs to the treatment boroughs the former areas were able to keep their levels of police hours constant. Obviously, this *ceteris paribus* feature greater simplifies our analysis since we do not have to deal with the implications of a zero-sum shift of resources across areas.

The next panel of Table 3 deals with the crime rates. It shows that crime rates fell by 11.1% in the DiD (final row, column 6). Again, this change is driven by a fall in treatment group crime rates and a steady crime rate in the comparison group. This is encouraging since it is what would be expected from the type of shift we have just seen in police deployment.

A visual check of weekly crime rates and police deployment is offered in Figure 3. Here we do two things. First, we normalize crime rates and police hours across the treatment and comparison groups by their level in week one of our sample (i.e.: January 2004). This re-scales the levels in both groups so that we can directly compare their evolution. Secondly, we take year-on-year seasonal differences so that the graphs show how the levels of crime and police hours compare to the levels at the same time in the previous year. This reveals a clear, sharp discontinuity in police deployment. Police hours worked in the treatment group rise immediately after the attack and sharply fall at the end of the six week Operation Theseus period.

The visual evidence for the crime rate is less decisive. The weekly crime rates are clearly more volatile than the police hours data. This is to be expected insofar that police hours are subject to a large degree of centralized determination and control while crime rates are essentially the outcomes of decentralized activity. However, the fall in treatment group crime rates during the six week policy-on period is not obviously larger than variations at other times. This raises the possibility that the fall in crime rates seen in the Table 3 DiD estimates may simply be due to naturally occurring, short-run time series volatility rather than the result of a policy intervention. After the correlated shocks issue this is probably the big modeling issue in the paper and we deal with it extensively in the next section.

4. Statistical Models of Crime and Police

In this section we present our statistical estimates. We begin with a basic set of estimates and then move on to focus on specific issues to do with different crime types, timing, correlated shocks and displacement effects.

Statistical Approach

The starting point for the statistical work is a DiD model of crime determination. We have borough level weekly data for the two calendar years 2004 and 2005. The terror attack variable (Z as discussed above) is specified as an interaction term $POST_t*T_j$, where POST is a dummy variable equal to one in the post-attack period and T denotes the treatment boroughs.

In this setting the basic reduced form seasonally differenced weekly models for considering police deployment and crime (with lower case letters denoting logs) are:

$$p_{bt} - p_{b(t-52)} = \alpha_1 + \beta_1 POST_t + \delta_1 (POST_t * T_b) + \lambda_1 (x_{bt} - x_{b(t-52)}) + (u_{1bt} - u_{1b(t-52)}) \quad (5)$$

$$c_{bt} - c_{b(t-52)} = \alpha_2 + \beta_2 POST_t + \delta_2 (POST_t * T_b) + \lambda_2 (x_{bt} - x_{b(t-52)}) + (u_{2bt} - u_{2b(t-52)}) \quad (6)$$

Because of the highly seasonal nature of crime (already noted above) the equations are differenced across weeks of the year (hence the t-52 subscript in the differences). The key parameters of interest are the δ's, which are the seasonally adjusted difference-in-difference estimates of the impact of the terror attacks on police deployment and crime.

These reduced form equations can of course be combined to form a structural model relating crime to police deployment, from which we can identify the causal impact of police on crime. The structural equation is:

$$c_{bt} - c_{b(t-52)} = \alpha_3 + \beta_3 POST_t + \delta_3 (p_{bt} - p_{b(t-52)}) + \lambda_3 (x_{bt} - x_{b(t-52)}) + (u_{3bt} - u_{3b(t-52)}) \quad (7)$$

where the variation in police deployment induced by the terror attacks identifies the causal impact of police on crime. The first stage regression is equation (5) above and so equation (7) is estimated by instrumental variables (IV) where the POST*TREAT variable is used as the instrument for the change in police deployment. Here the structural parameter of interest, δ_3 (the coefficient on police deployment), is equal to the ratio of the two reduced form coefficients, so that $\delta_3 = \delta_2/\delta_1$.

Basic Difference-in-Difference Estimates

Table 4 provides the basic reduced form OLS and structural IV results for the models outlined in equations (5)-(7). For comparative purposes, we specify three T*Post-Attack terms to evaluate the interaction term. Specifically, in columns (1) and (5) 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)-(4) and (6)-(8)) 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 long-term trends in the treatment group.

The findings from the unconditional DiD estimates reported earlier are confirmed in the basic models in Table 4. 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 from there is an 11.1% 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. Despite this we include a full set of 32 borough-specific trends in the specifications in columns (7) and (8) to test robustness. The crime rate coefficient for the Operation Theseus period halves but the interaction term is still significant indicating that there was a fall in crime during this period that was over and above that of any combination of trends.

The respective timings of the increase in police deployment and the fall in crime very much suggest that the increased security presence lowered crime. The final three columns of the Table thus show estimates of the causal impact of increased deployment on crime. Column (11) shows the basic IV estimate where the post-attack effects are constrained to be time invariant. Columns (12) and (13) allow 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 (12)) shows an elasticity of crime with respect to police of around -.31 to -.32. This implies that a 10 percent increase in police activity reduces crime by around 3.1 to 3.2 percent.

The magnitudes of these 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). 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.

OLS estimates are reported in columns (9) and (10) for comparison. The column labeled levels estimates a pooled cross-sectional regression resulting in a high, positive coefficient on the police deployment variable. In column (10) 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.

Different Crime Types

In Table 5 we adopt the same framework but break the dependent variable into two variables representing crimes that we think are likely to have been susceptible to the deterrent effect of the police deployment and those that were not. The results in Table 5 confirm this intuition with stronger effects seen for susceptible crimes and virtually none for the non-susceptible crimes. Separate results for each of the eight major crime categories are reported in the Appendix in Table A2.

Timing

The previous section cited the volatility of the crime rates and timing in general as a major issue. Given that we are using weekly data there is a need to investigate to what extent short-term variations could be driving the results for our inferred policy intervention. To test this we take the extreme approach of testing every week for hypothetical policy effects. Specifically, we estimate the reduced form models outlined in equations (5) and (6) defining a single week-treatment group interaction term for each of the 52 weeks in our data. We then run 52 regressions each featuring a different week*T_b interaction and plot the estimated coefficient and confidence intervals. The major advantage of this is that it extracts all the variation and volatility from the data in a way that reveals the implications for our main DiD estimates.

Figure 4 provides a "close-up" of these estimates plotting them for 18 weeks of the data. These weeks are the six weeks of Operation Theseus as well as the six weeks before and after the operation. Figure 4(a) shows the results for police hours repeating the clear pattern seen in Figure 3(a) of the police deployment policy being switched on and off. (Note that precisely estimated treatment effects in this graph are characterized by confidence intervals that do not overlap the zero line). The analogous result for the susceptible crime rate is then shown in Figure 4(b). The falls in crime are less dramatic than the increases in police hours but the two clearly coincide in timing. Figure 5 then shows the same graphs for the full 52-week period we have available. Here it is interesting to note that the pattern of a six consecutive weeks of significant, negative treatment effects in the crime rate are not repeated in any other period of the data except Operation Theseus. This is impressive as it shows that the effect of the policy intervention can be seen "poking through" the noise and volatility of the weekly data.

As a further check on the issue of volatility we also make use of some monthly, borough-level crime data available from 2001 onwards. Results using this data are reported in Table 6. Here we estimate year-on-year, seasonally differenced models for each pair of years going back to 2001-2002. Again we find that a significant treatment effect in susceptible crimes is only evident for the 2004-2005 time period. This gives us further confidence that our estimate for this year is a unique event that cannot be conflated with arbitrary fluctuations in previous years.

Correlated Shocks

The discussion of timing has a direct bearing on the issue of correlated shocks outlined in Section 2. In particular, it is important to examine the extent to which any shifts in correlated observables do or do not coincide in timing with the fall in crime. The major tool we use here is the tube journeys data obtained from Transport for London. This records journey patterns for the main method of public transport around London and therefore provides a good proxy for shifts in the volume of activity around the city. Like the police and crime data, we aggregate the journeys information to borough level and normalize it with respect to population.

Table 7 represents the first step in our analysis of the tube data. Here we define a reduced form model similar those in equations (5) and (6) but using tube journeys as the dependent variable. This specification tests to what extent the fall in tube journeys after the attacks followed the pattern of the police deployment. The estimates indicate that total journeys fell by 11.9% (column 2, controls) over the period of Operation Theseus. However, some of this fall may have been due to a diversion of commuters onto other modes of public transport. This is particularly plausible given that two tube lines running through the treatment group were effectively closed down for approximately four weeks after July 7th. To examine the implications of this we instead normalize journeys by the number of open tube stations with the results reported in panel B of the Table. The fall in journeys is no longer evident after this normalization indicating that station closures could be explaining the findings in the earlier panel. Hence it is plausible that there was a diversion of activity onto other modes of transport rather than a drop in volume.

Further support for this hypothesis is evident in Table 8. Using the Labor Force Survey (LFS) we find no evidence that the work travel decisions of people in Outer London and the South-East were affected by the attacks. Changes in the proportion of commuters before and after the attacks are negligible, lending support to the idea modes of transport activity were affected rather than volume.

However, the issue of work travel decisions uncovers a source of variation that we able to exploit for evaluating the possible effect of observable, activity-related shocks. Specifically, any basic model of work and non-work travel decisions predicts interesting variations in terms of timing. For example, we would expect that faced with the terrorist risks associated with travel on public transport people would adjust their behavior differently for non-work travel. That is, the travel decision is less elastic for the travel to work decision as compared to the non-work travel. As such, we would expect that tube journeys would fall by proportionately more on weekends (when most non-work travel takes place) than on weekdays. The figures in Table 9 suggests that this was the case with tube journeys falling by 7.5% more on weekends that weekdays (final column, second last row).

Hence there is a source of intra-week variation in the shock to observables. If the shock to observables is driving the fall in crime then we would expect this effect to be more pronounced on weekends. Following this, Table 10 then performs the exercise of re-estimating the baseline models of Table 5 but excluding all observations relating to weekends[6]. This results in similar coefficient estimates to Table 5 and only slightly larger standard errors. Practically, this means that our estimates are unaffected even when we drop the section of our crime data that is most vulnerable to the problem of correlated observable shocks.

A similar argument prevails in terms of correlated unobservable shocks. As we have seen from Figures 4a and 4b there is a distinctive pattern to the timing of the fall in crime. For unobservable shocks to be driving our results their effect would have to be large and exquisitely timed. However, basic survey evidence on risk attitudes amongst Inner and Outer London residents, reported in Table 11 suggests that there is significant difference in the types of attitudes that would drive a set of significant, differential

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[6] Recall that our crime, police and tube journeys data is available at daily level for the years 2004-2005. This gives us the flexibility to drop Saturday and Sunday before aggregating to a weekly frequency.

unobservable shocks across our treatment and groups. We thus conclude that the effect of unobservables is likely to be minimal.

Displacement Effects

The final empirical issue we consider is that of crime displacement. We can only do this in a limited manner in that we only have detailed crime data for London boroughs and not for areas outside. Nonetheless, one way of thinking about displacement is by means of the selected set of control areas. Suppose crime was displaced from central London to areas just outside then we would see estimated effects being different if we consider the whole of outer London as a control group (as we have so far) rather than if we focus upon areas that do not stretch all the way to the borders of London.

In Table 12, we therefore consider estimates which only use boroughs geographically closer to the treatment boroughs as controls. We use two sets – those boroughs that are adjacent to the treatment boroughs and a ‘matched’ group of five central boroughs which, in conjunction with the five treatment boroughs, we refer to as the Central Ten sample. If crime was being displaced to these geographically closer boroughs then we would see different estimates from the baseline estimates considered earlier.

As it turns out, using these more matched control boroughs (Adjacent and Central Ten) produces very similar results to the estimates based on using all outer London boroughs. The estimates are shown, separately for susceptible and non-susceptible crimes, in Table 12. The Table gives the crime reduced forms and in each case the estimates are similarly in terms of identifying a crime fall of around 11-13 percent for susceptible crimes in central London relative to the respective control boroughs. As with the earlier baseline results there is no impact on non-susceptible crimes.

5. Conclusions

In this paper we provide new, highly robust evidence on the causal impact of police on crime. Our starting point is the basic insight at the centre of Di Tella and Schargrodsky's (2004) paper, namely that terrorist attacks can induce exogenous variations in the allocation of police resources that can be used to estimate the causal impact of police on crime. Using the case of the July 2005 London terror attacks, our paper extends this strategy in two significant ways. Firstly, the scale of the police deployment we consider is much greater than the highly localized responses that have been previously studied. Together with the unique police hours data we employ this allows us to provide the first new IV-based estimates of the police-crime elasticity since Levitt (1997) and Corman and Mocan (2000). Furthermore, there is a novel *ceteris paribus* dimension to the London police deployment. By temporarily extending its resources (primarily through overtime) the police service was able to keep their force levels constant in the comparison group that we consider while simultaneously increasing the police presence in the treatment group. As a result, this provides a clean setting to test for potential displacement effects arising from the allocation of police.

Secondly, our identification strategy explicitly deals with the problem of what we call "correlated shocks" to observables and unobservables. The growing economics of terrorism literature suggests that terrorist attacks can have a number of (mostly short-run) economic and non-economic impacts in urban areas. In our case, we could realistically expect that the July 2005 attacks affected both police deployment as well as travel patterns and individual behaviour throughout London. Therefore, insofar that the terrorist attack affected these travel patterns and individual behaviours it could have shifted the supply of potential victims in certain areas leading to a fall in crime. Depending on the distribution of these effects and the way that they are correlated with the reallocation of police resources this could bias estimates of the police-crime relationship and undermine the overall identification strategy.

A number of features of our analysis allow us to comprehensively deal with this issue of correlated shocks to observables and unobservables. In practical terms, the payroll-based data on police hours that we use enables us to clearly quantify and map the post-attack police deployment in London. The increase in police presence in London after the July 7th attacks was large, unanticipated and geographically concentrated within five central and inner London boroughs. Furthermore, the increase was limited to a six week period following the attacks thereby creating a clear distinction between the periods when the deployment policy was switched on and off. This allows us to adopt a difference-in-difference strategy to identify the impact of the police deployment on crime In short, because we are able to clearly identify the timing and location of the police deployment we are able to rule out the possibility that correlated observable and unobservable shocks are driving our estimates of the police-crime relationship.

Our identification strategy delivers some striking results. There is clear evidence that the timing and location of falls in crime coincide with the increase in police deployment. Crime rates return to normal after the six week “policy-on” period, although there is little evidence of a compensating temporal displacement effect afterwards.

Shocks to observable activity (as measured by tube journey data) cannot account for the timing of the fall and it is hard to conceive of a pattern of unobservable shocks that could do so as well. Our preferred IV causal estimate of the crime-police elasticity is approximately -0.31, which is slightly below the existing results in the literature, but is very precisely estimated and supportive of the basic economic model of crime where more police reduce criminal activity.

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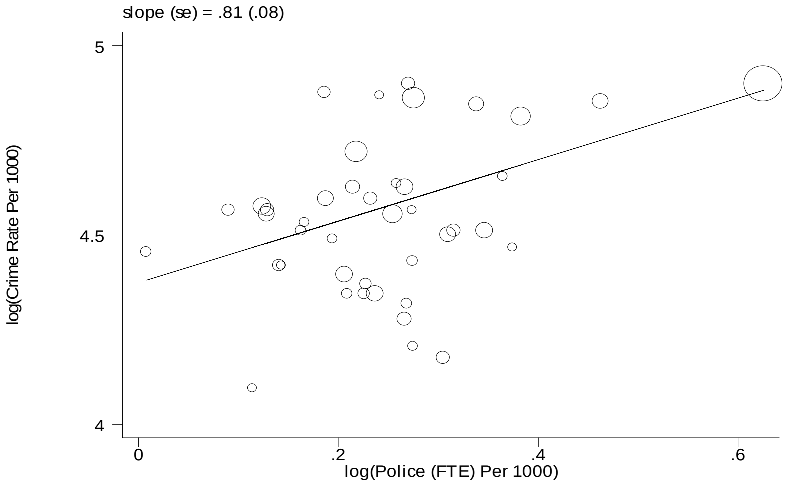
Figure 1: A Map of London Boroughs

Figure 2: Crime Rates and Police, 42 Police Force Areas of England and Wales, 2005 to 2006



Notes:

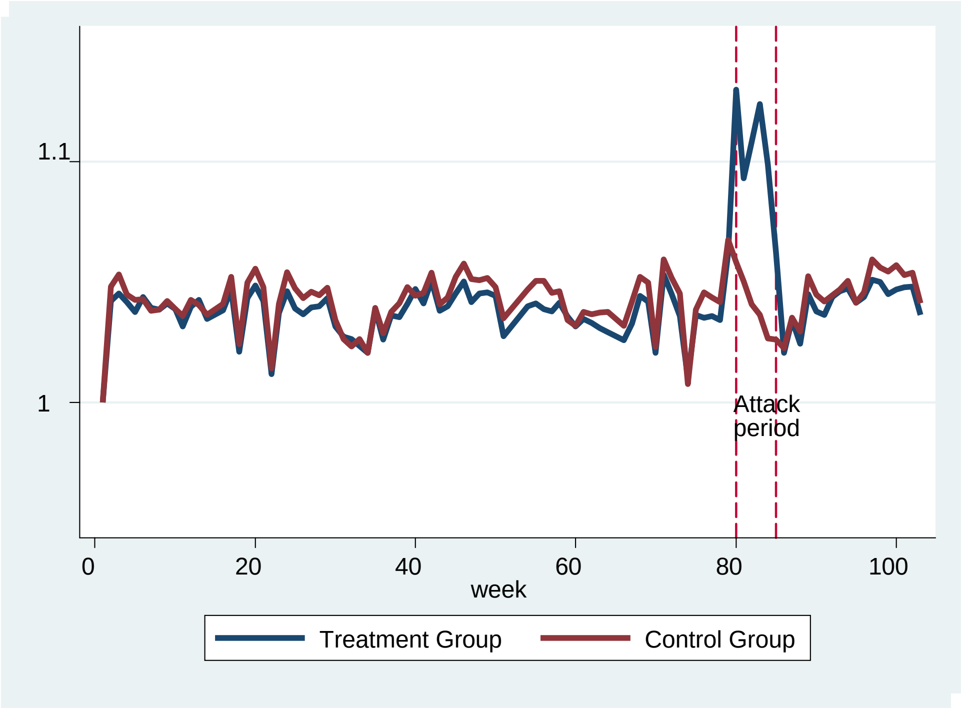
(1) Figure shows the correlation between the log(Crime per 1000 population) and log(Police per 1000 population) for 42 police force areas in England and Wales in 2005-06.

(2) There are 42 areas because the Metropolitan and City of London police are aggregated.

(3) Total crimes are for the whole financial year (April 2005 to March 2006); police numbers are measured in full-time equivalents in September 2005.

Figure 3: Level Graphs 2004-2005, Treatment versus Comparison Groups.

(a) Police Deployment (Normalized at t=1) – (Police Hours/ Population)



(b) Major Crimes – Crimes/Population

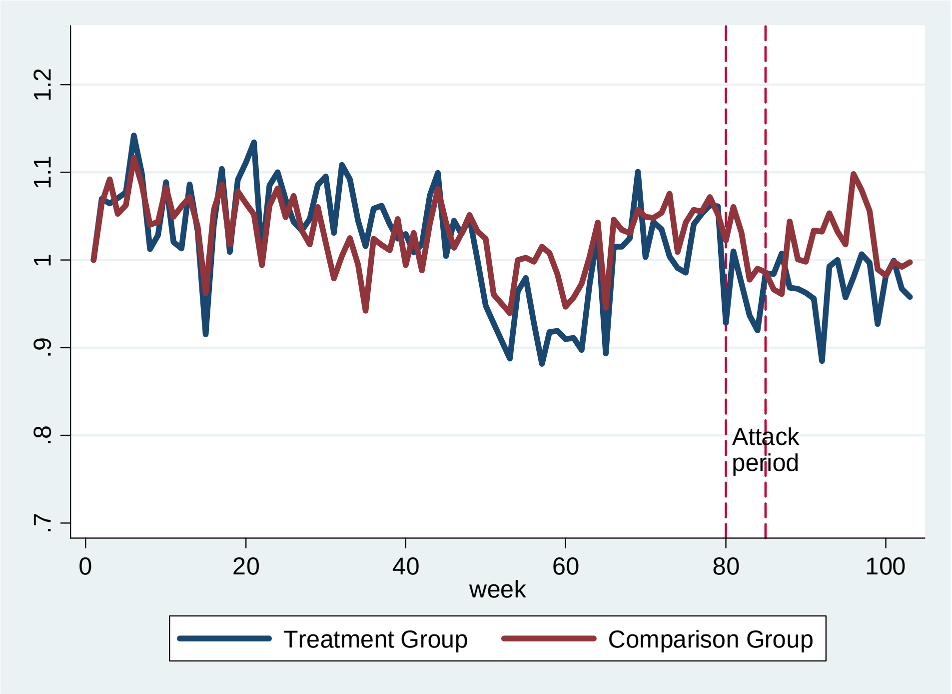
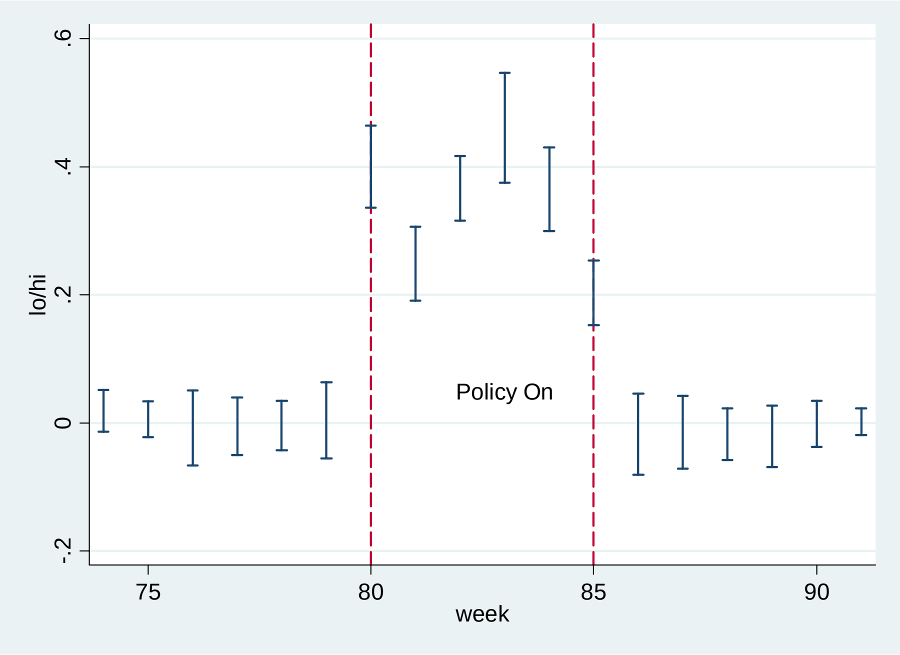


Figure 4: Week-by-Week Policy Effects (Operation Theseus Window), Borough Level Models, 2004-2005.

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



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

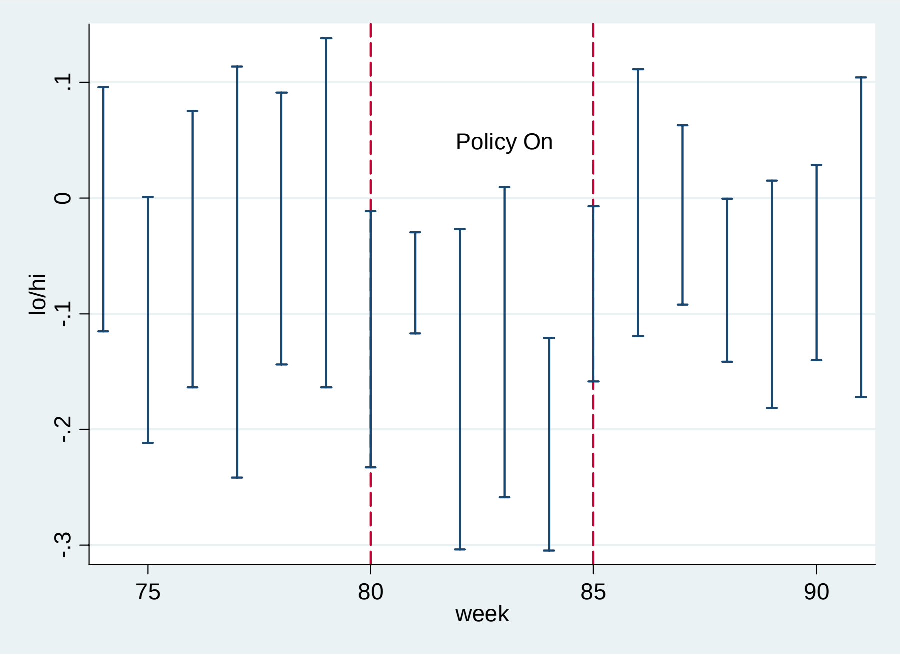
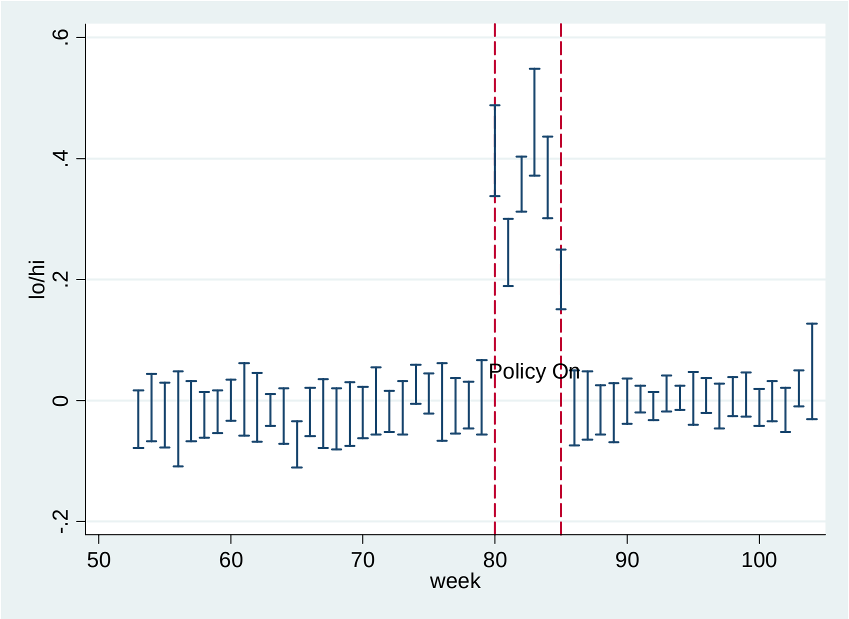


Figure 5: Week-by-Week Policy Effects (Full Period), Borough Level Models, 2004-2005.

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



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

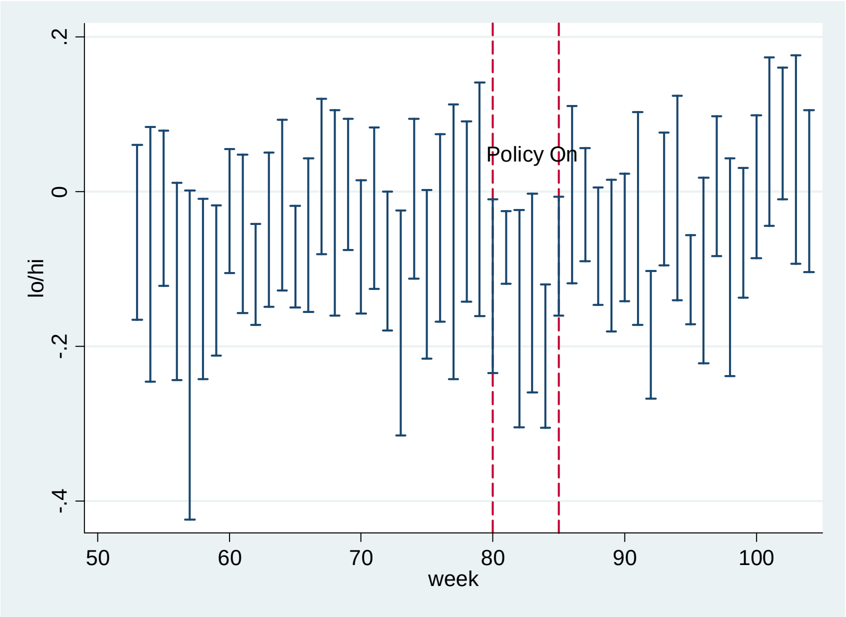


Table 1: Distribution of Crime in London by Major Category, 2004-2005

	(1) % of All Major Crimes	(2) Crime Rate (per 1000 population)	(3) % Occurring in Treatment Group	(4) Crime Rate in Treatment Group (per 1000)
Susceptible Crimes				
Violence and Sexual Offences	23.7	21.3	17.9	40.3
Theft and Handling	44.0	53.1	28.0	117.0
Robbery	4.6	5.5	15.5	6.7
Sub-Total	**72.2**	**79.9**	**23.6**	**164.0**
Non-Susceptible Crimes				
Burglary	12.3	14.8	17.4	20.2
Criminal Damage	15.5	18.7	13.6	20.0
Sub-Total	**27.8**	**33.5**	**15.3**	**40.2**
Total	**100.0**	**113.4**	**25.4**	**204.2**

Notes

a) All major crimes occurring in the 32 boroughs of London between 1st January 2004 and 31st December 2005.
b) Crime rate in column (2) calculated as number of crimes as per 1,000 members of population.
c) Treatment group defined as boroughs of Westminster, Camden, Islington, Tower Hamlets and Kensington-Chelsea.

Table 2: Police Patrols After July 7, 2005

	Inner London	*Outer London*
Q: Have you seen more, less or about the same police patrols across London?		
More (%)	70	62
About the Same (%)	20	27
Less (%)	5	3
Don't Know (%)	5	8
Total Respondents (N)	248	361

Source: IPSOS MORI Survey.
Notes:

(a) Exact wording of question: "Since the attacks in July, would you say you have seen more, less or about the same amount of police patrols across London?"
(b) Interviews conducted on 22-26 September 2005.

Table 3: Police Deployment and Major Crimes

	Police Deployment *(Hours Worked per 1000 Population)*			***Crime Rate*** *(Crimes per 1000 Population)*		
	(1) Pre-Period	*(2) Post-Period*	*(3) %Difference (Post – Pre)*	*(4) Pre-Period*	*(5) Post-7/11*	*(6) % Difference (Post – Pre)*
T = 1	169.46	242.29	+72.83	4.03	3.59	-0.44
T = 0	82.77	84.95	+2.18	1.99	1.97	-0.020
Difference-in-Difference			Levels +70.65 (5.28)			Levels -0.42 (0.11)
			Logs 0.346 (0.028)			Logs -0.111 (0.027)

Notes:

(a) Post-period defined as the 6 weeks following 7/7/2005. Pre-period defined as the six weeks following 8/7/2004. Weeks defined in a Thursday-Wednesday interval throughout to ensure a clean pre and post split in the 2005 attack weeks.

(b) Treatment group defined as boroughs of Westminster, Camden, Islington, Tower Hamlets and Kensington-Chelsea.

(c) Police deployment defined as total weekly hours worked by police staff at borough-level.

Table 4: Difference-in-Difference Regression Estimates, Police Deployment and Major Crimes (2004-2005).

	Police Deployment (Hours Worked Per 1000 Population)				***Total Crimes (Crimes per 1000 population)***				***OLS***		***IV Estimates***		
	Full (1)	Split (2)	+Controls (3)	+Trends (4)	Full (5)	Split (6)	+Controls (7)	+Trends (8)	Levels (9)	Differences (10)	Full (11)	Split (12)	+Trends (13)
T*Post-Attack	0.081 (0.010)				-0.052 (0.021)								
T*Post-Attack 1		0.341 (0.028)	0.342 (0.028)	0.356 (0.026)		-0.111 (0.027)	-0.109 (0.027)	-0.058 (0.029)					
T*Post-Attack 2		-0.000 (0.010)	0.001 (0.010)	0.015 (0.016)		-0.034 (0.027)	-0.032 (0.028)	-0.022 (0.054)					
ln(Police Deployment)									0.738 (0.053)	-0.031 (0.050)	-0.606 (0.273)	-0.318 (0.079)	-0.183 (0.063)
Controls	No	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trends	No	No	No	Yes	No	No	No	Yes	Yes	Yes	No	No	Yes
Number of Boroughs	32	32	32	32	32	32	32	32	32	32	32	32	32
Number of Observations	1664	1664	1664	1664	1664	1664	1664	1664	1664	1664	1664	1664	1664

Notes: All specifications include week fixed effects. Clustered standard errors in parentheses. Boroughs weighted by population.

(a) 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.

(b) 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.

(c) Treatment group defined as boroughs of Westminster, Camden, Islington, Tower Hamlets and Kensington-Chelsea.

(d) Police deployment defined as total weekly hours worked by all police staff at borough-level.

(e) 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 5: Susceptible Crime versus Non-Susceptible Crimes (2004-2005).

Panel (A)	***Susceptible Crimes (Crimes per 1000 population)***				***OLS***		***IV Estimates***		
	Full (1)	Split (2)	+Controls (3)	+Trends (4)	Levels (5)	Differences (6)	Full (7)	Split (8)	+Trends (9)
T*Post-Attack	-0.056 (0.023)								
T*Post-Attack 1		-0.131 (0.031)	-0.131 (0.030)	-0.070 (0.033)					
T*Post-Attack 2		-0.035 (0.029)	-0.035 (0.030)	-0.028 (0.061)					
ln(Police Deployment)					0.897 (0.054)	-0.012 (0.064)	-0.692 (0.288)	-0.382 (0.089)	-0.223 (0.069)
Panel (B)	***Non-Susceptible Crimes (Crimes per 1000 population)***				***OLS***		***IV Estimates***		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
T*Post-Attack	-0.047 (0.024)								
T*Post-Attack 1		-0.031 (0.031)	-0.019 (0.031)	-0.005 (0.036)					
T*Post-Attack 2		-0.049 (0.038)	-0.040 (0.041)	-0.023 (0.054)					
ln(Police Deployment)					0.307 (0.064)	-0.072 (0.100)	-0.443 (0.347)	-0.053 (0.091)	0.006 (0.103)
Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trends	No	No	No	Yes	No	Yes	No	No	Yes
Number of Boroughs	32	32	32	32	32	32	32	32	32
Number of Observations	1664	1664	1664	1664	1664	1664	1664	1664	1664

Notes: All specifications include include week fixed effects. Clustered standard errors in parentheses. Boroughs weighted by population.

(a) Susceptible Crimes defined as: Violence and Sexual Offences; Theft and Handling and Robbery.
(b) Non-Susceptible Crimes defined as: Burglary and Criminal Damage.
(c) Treatment group definitions and T*Post-Attack terms defined as per Table 4. Controls also defined as per Table 4.

Table 6: Extended Time Period Analysis Based on Monthly Data
(Borough Level Models, Monthly Data, Differenced Across Years, 2001 to 2005)

	Change in Log(Crimes Per 1000 Population)			
A. Year on Year Changes, Susceptible Crimes				
	July/August 2001 – July/August 2002	July/August 2002 – July/August 2003	July/August 2003 – July/August 2004	July/August 2004 – July/August 2005
Treatment boroughs	0.030	-0.059	-0.056	-0.097
Control boroughs	0.071	-0.021	-0.026	0.007
T – C Gap	-0.041 (0.030)	-0.038 (0.030)	-0.030 (0.042)	-0.104 (0.030)
B. Year on Year Changes, Non-Susceptible Crimes				
	July/August 2001 – July/August 2002	July/August 2002 – July/August 2003	July/August 2003 – July/August 2004	July/August 2004 – July/August 2005
Treatment boroughs	-0.025	-0.120	-0.120	-0.054
Control boroughs	0.001	-0.065	-0.065	-0.005
T – C Gap	-0.026	-0.055 (0.051)	-0.055 (0.051)	-0.049 (0.033)

Notes: All models estimated in terms of seasonal differences (ie: differenced relative to the same month in the previous year). Clustered standard errors in parentheses. Boroughs weighted by population.

(a) Treatment group defined as boroughs of Westminster, Camden, Islington, Tower Hamlets and Kensington-Chelsea. "Policy-on" period defined as July-August.

(b) Crime defined according to Susceptible and Non-Susceptible categories given in Table 5.

Table 7: Impact on Tube Journeys, Before and After July 7th

	(A) *Unadjusted* *ln (Journeys/Population)*		*(B)* *Adjusted* *Ln (Journeys/Population)*	
	(1) Basic	(2) +Controls	(3) Basic	(4) +Controls
T*Post Attack1	-0.127 (0.080)	-0.119 (0.076)	-0.026 (0.086)	-0.016 (0.084)
T*Post Attack2	-0.059 (0.036)	-0.053 (0.034)	-0.059 (0.036)	-0.053 (0.034)
Controls	No	Yes	No	Yes
Number of Boroughs	32	32	32	32
Number of Observations	1,664	1,664	1,664	1,664

Notes: All specifications include include week fixed effects. Clustered standard errors in parentheses. Boroughs weighted by population.

(a) This table estimates difference-in-difference models of tube journeys per borough. Panel A estimates models using recorded journeys. Panel B normalizes journeys by the total number of open tube stations within a borough.

(b) Tube Journeys measured as total entry and exit for all stations within a borough.

(c) Stations along the Piccadilly Line (Arnos Grove to Hyde Park Corner) and Hammersmith and City Line closed from July 7th to August 2nd, 2005. Note that stations that intersect with other tube lines are not counted as part of this closure.

Table 8: Work Travel Patterns Into Central London

	(1) *Outer London Resident*	*(2)* *Rest of South-East Resident*
(A) Short-Run		
6 Week Before	0.166	0.035
6 Weeks After	0.175	0.037
Difference	+0.005	+0.002
	(0.022)	(0.008)
(B) Medium-Run		
12 Weeks Before	0.145	0.038
12 Weeks After	0.157	0.031
Difference	+0.012	-0.006
	(0.021)	(0.005)
(C) Long-Run		
6 Months Before	0.155	0.034
6 Months After	0.160	0.031
Difference	+0.005	-0.003
	(0.015)	(0.004)
Employment Share (Inner London)	0.448	0.205

Source: Quarterly Labour Force Survey, 2004-2005.
Notes: Standard errors clustered by week.

1. Defined for all employed person aged 18-65 working in Central or Inner London.
2. Column 1 defines all those residing in Outer London and working in Central or Inner London.
3. Column 2 defines all those residing in the South East of England region and working in Central or Inner London.

Table 9: Changes in Tube Journeys – Weekdays Versus Weekends

Attack Period (By Week)	*Weekends*	*Weekdays*	*Difference*
Week 1	-0.409	-0.415	0.006
Week 2	-0.145	-0.120	-0.025
Week 3	-0.425	-0.238	-0.187
Week 4	-0.291	-0.177	-0.114
Week 5	-0.210	-0.126	-0.084
Week 6	-0.147	-0.099	-0.048
Period Means			
Before Attack (6 weeks)	0.106	0.091	0.015 (0.049)
Attack Period (6 Weeks)	-0.196	-0.271	-0.075 (0.030)
Post Attack (6 Weeks)	-0.045	-0.017	-0.028 (0.076)

Notes:

(1) Columns report percentage year-on-year change in total number of tube journeys between 2004 and 2005.
(2) "Attack Period" defined as 6 weeks following 7/7/05.
(3) "Before Attack" defined as 6 weeks immediately before the July 7 terrorist attack.
"Post Attack" defined as the 6 weeks following the attack and police deployment period (ie: 6 weeks following 19/08/05)

Table 10: Estimated Crime Treatment Effects When Excluding Weekends

	(A) *Susceptible Crimes*				*(B)* *Non-Susceptible Crimes*			
	Controls (1)	+Trends (2)	IV (Controls) (3)	IV (Trends) (4)	Controls (5)	+Trends (6)	IV (Controls) (7)	IV (Trends) (8)
T*Post Attack1	-0.138 (0.046)	-0.076 (0.038)			0.022 (0.036)	0.036 (0.042)		
T*Post Attack2	-0.037 (0.030)	0.027 (0.062)			-0.033 (0.047)	-0.015 (0.066)		
ln(Police Deployment)			-0.401 (0.134)	-0.240 (0.096)			0.065 (0.105)	0.115 (0.114)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Boroughs	32	32	32	32	32	32	32	32
Number of Observations	1,664	1,664	1,664	1,664	1,664	1,664	1,664	1,664

Notes: All specifications include include week fixed effects. Clustered standard errors in parentheses. Boroughs weighted by population.

(a) These models estimate similar models to Table 5 but using a count of crimes per 1000 population that excludes all crimes occurring on weekends (ie: using only Monday-Friday).

(b) Treatment groups, T*Post-Attack terms and Crime Categories defined as per Table 5.

Table 11: Survey Evidence on Community Attitudes, Inner versus Outer London.

Question & Response	(1) Inner London (%)	(2) Outer London(%)
(1) As a result of the attacks have you considered moving to live outside London or not?		
Yes	11	11
No	89	89
(*2) How likely do you think it is London will experience another attack in the near future?*		
Very likely	36	48
Somewhat likely	43	37
Not very likely	11	8
Not at all likely	4	3
Don't Know	6	4
(3) As a result of the attacks, have you spent more or less time in Central London?		
More time	2	2
Less time	19	21
Made No Difference	78	76
(4) Since the July attacks have you personally or friends and relatives experienced any hostility on the basis of race or religion?		
Yes: Verbal Abuse	6	6
Yes: Physical Abuse	2	1
Yes: Felt Under Suspicion or Stared At	2	2
Yes: Generally Felt Hostility	2	2

Source: IPSOS MORI Survey.

Table 12: Varying Control Groups to Look for Possible Crime Displacement

	(A) Susceptible Crimes						*(B) Non-Susceptible Crimes*					
	Inner London		Adjacent		Central Ten		Inner London		Adjacent		Central Ten	
	(1) Controls	(2) +Trends	(3) Controls	(4) +Trends	(5) Controls	(6) +Trends	(7) Controls	(8) +Trends	(9) Controls	(10) +Trends	(11) Controls	(12) +Trends
T*Post-Attack1	-0.126 (0.040)	-0.066 (0.043)	-0.131 (0.039)	-0.073 (0.042)	-0.110 (0.049)	-0.057 (0.053)	0.046 (0.050)	0.046 (0.049)	0.038 (0.047)	0.023 (0.041)	0.070 (0.038)	0.024 (0.041)
T*Post-Attack2	-0.042 (0.035)	0.017 (0.069)	-0.030 (0.033)	0.027 (0.067)	-0.011 (0.034)	0.042 (0.075)	-0.003 (0.049)	-0.002 (0.057)	-0.019 (0.051)	-0.034 (0.060)	0.000 (0.060)	-0.045 (0.070)
Trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
No. of Boroughs	13	13	13	13	10	10	13	13	13	13	10	10
No. of Observations	676	676	676	676	520	520	676	676	676	676	520	520

Notes: Clustered standard errors in parentheses. All specification include week fixed effects and time-varying controls.

(1) 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).

(2) Adjacent boroughs defined as: Brent, Hackney, Hammersmith & Fulham, Lambeth, Newham, Southwark and Wandsworth.

(3) Central Ten boroughs defined as: Westminster, Camden, Islington, Kensington and Chelsea, Tower Hamlets (Treatment Group) and Brent, Hackney, Hammersmith & Fulham, Lambeth and Southwark.

APPENDIX

Table A1: List of Minor Crimes by Major Category, 2004-2005.

Major Category	*Minor Category*	*As Proportion of Major Category Crimes (%)*
Violence and Sexual Crimes	Common Assault	30.1
	Harassment	20.4
	Aggravated Bodily Harm (ABH)	32.9
	Grievous Bodily Harm (GBH)	2.6
	Murder	0.1
	Offensive Weapon	3.8
	Other Violence	5.5
	Rape	1.1
	Other Sexual	3.6
Theft and Handling	Picking Pockets	5.2
	Snatches	3.9
	Theft from Shops	10.4
	Theft / Taking of Pedal Cycles	5.2
	Theft / Taking of Motor Vehicles	12.6
	Motor Vehicle Interference and Tampering	0.8
	Theft from Motor Vehicles	23.7
	Other Theft	37.6
	Handling Stolen Goods	0.6
Robbery	Business Property	6.4
	Personal Property	93.6
Burglary	Burglary in a Dwelling	62.9
	Burglary in Other Buildings	37.1
Criminal Damage	Criminal Damage to Motor Vehicles	44.3
	Criminal Damage to a Dwelling	28.7
	Criminal Damage to Other Buildings	14.0
	Other Criminal Damage	13.0

Source: London Metropolitan Police Service (MPS), Ward-level, daily crime

Table A2: Treatment Effects by Major Crime Category

	(A) *Susceptible Crimes*						*(B)* *Non-Susceptible Crimes*			
	Theft and Handling		Violence and Sexual Offences		Robbery		Burglary		Criminal Damage	
	(1) Controls	(2) +Trends	(3) Controls	(4) +Trends	(5) Controls	(6) +Trends	(7) Controls	(8) +Trends	(9) Controls	(10) +Trends
T*Post-Attack1	-0.131	-0.110	-0.138	-0.085	-0.107	-0.007	-0.027	-0.026	-0.032	-0.004
	(0.040)	(0.026)	(0.043)	(0.043)	(0.114)	(0.125)	(0.057)	(0.066)	(0.053)	(0.043)
T*Post-Attack2	-0.064	-0.047	-0.019	0.038	-0.068	0.041	-0.081	-0.074	-0.007	0.025
	(0.024)	(0.047)	(0.039)	(0.083)	(0.113)	(0.142)	(0.060)	(0.077)	(0.051)	(0.064)
Trends	No	Yes	No	Yes	No	Yes	No	Yes	No	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: All specifications include week fixed effects. Clustered standard errors in parentheses. Boroughs weighted by population.

(a) Treatment groups, T*Post-Attack terms defined as per Tables 4 and 5.
(b) Disaggregated minor crime categories listed in Table A5