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

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Abstract

In this paper we study the causal impact of police on crime by looking at what happened to crime and police 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 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, before sharply falling back to pre-attack levels. During this time crime fell significantly in central relative to outer London. Study of the timing of the crime reductions and their magnitude, the types of crime which were more likely to be affected and a series of robustness tests looking at possible biases all make us confident that our research approach identifies a causal impact of police on crime. The instrumental variable approach we use uncovers an elasticity of crime with respect to police of approximately -0.3, 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 this 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 thereof, have also 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 relationship since terror attacks often induce an increased police presence in particular locations. This deployment of additional police can, under certain conditions, be used to test whether or not increased police reduce crime.[1]

In this paper we also consider the crime-police relationship before and after a terror attack, but in a very different context to other studies by looking at the increased security presence following the terrorist bombs that hit London in July 2005. Our application is a more general one than the other studies in that it covers a large metropolitan area following one of the most significant and widely known terror attacks of recent years. The scale of the security response in London after these attacks 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 magnitude of the causal impact of police on crime.[2] Thus a major strength of this paper is that we are able to offer explicit instrumental variable-based estimates of the police-crime elasticity – the first since Levitt's (1997) seminal contribution and that of Corman and Mocan (2000). In fact, the sharp discontinuity in police deployment that we able to identify using this data means we are able to pin down this causal relation between crime and police very precisely . The natural experiment that we consider also has some important external validity in the sense that it involves

[1] 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. Both are discussed in more detail below.

[2] Neither Di Tella and Schargrodsky (2004) nor Klick and Tabarrok (2005) had access to data on police activity.

the deployment of a clear "deterrence technology" (that is, more police on the streets) rather than a simple measure of expenditures. Arguably, this type of visible increase in police deployment is the main type of policy mechanism under discussion in public debates about the funding and use of police resources.

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 increase in police deployment, rather than via other observable and unobservable factors correlated with the attack or shock. Again, the police deployment data we use makes it possible to distinguish the impact of police on crime from the general impact of the terrorist attack. In particular, our research design features two interesting discontinuities related to the police intervention. The first is the introduction of the geographically-focused police deployment policy in the week of the terrorist attack. This immediate period surrounding the introduction of the policy was also characterized by a series of correlated observable and unobservable shocks related to the attack. In contrast, the second discontinuity associated with the withdrawal of the policy occurred in a very different context. In this case, the observable and unobservable shocks associated with the attack were still in effect and dissipating gradually. Crucially though, the police deployment was discretely "switched off" after a six week period and we observe an increase in crime that is exactly timed with this change. Thus, we argue that is difficult to attribute this clear change in crime rates to observable and unobservable shocks arising from the terrorist attacks. If these types of shocks were significantly affecting crime rates then we would expect that effect to continue even as the police deployment was being withdrawn. Indeed, an interesting feature of our empirical results is how clear and definitively crime seems to respond to a police presence.

Following similar themes, our research design also allows us to examine how the overall impact of the police intervention may have been mitigated by temporal or spatial displacement effects. Such effects would occur if criminal behavior changed significantly in response to the allocation of police. However, we do not find evidence of serious displacement effects at the between London boroughs level that forms the main part of our analysis.

The use of this strong research design is important since the crime-police relation has received a lot of attention over the years, yet remains a problematic area. For example, a large literature in criminology casts doubt on the effectiveness of police in reducing crime. For example, on the basis of a series of criminological studies from the 1970s and 1980s, Sherman and Weisburd (1995) state that this work "convinced many distinguished scholars that no matter how it is deployed, police presence does not deter".[3] Moreover, 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, with some studies even finding a positive association between the two. This is because most of the existing work faces difficulties in attempting to unravel the direction of causation in the relationship between police resources and crime.

However, a small but growing research area does directly address the question of causality. Probably the best known paper here is Levitt's (1997) study of US cities, which attempts to resolve endogeneity issue through an instrumental variable strategy that uses election years as an instrument for police in a crime equation. In doing so he identifies a negative causal effect running from police to crime, but this work remains controversial for a number of reasons: see McCrary's (2002) comment, which discusses some concerns about the data and the approach used in the Levitt paper, and Levitt's (2002) response.

Some of the other work which attempts 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 presence. The Di Tella and Schargrodsky (2004) paper referred to above shows that motor vehicle thefts fell significantly near the main Jewish centre in Buenos Aires where a terrorist attack occurred in July 1994 compared to the area several blocks away where no extra police were deployed. The

[3] Sherman and Weisburd (1995) review some of the conclusions from this work. Gottfriedson and Hirschi (1990: 270) state that "no evidence exists that augmentation of police forces or equipment, differential police strategies, or differential intensities of surveillance have an effect on crime rates". Felson (1994:10-11) contends that patrols constitute "a drop in the bucket" for dense urban areas. Finally, Klockars (1984:130) stated that using routine police patrols to fight crime was as sensible as having "firemen patrol routinely in fire trucks to fight fire".

later study by Klick and Taborrak (2005) used the case of changing terror alert levels in Washington to test for a possible impact of police on crime. As mentioned above, both of these studies adopted a reduced form approach by necessity as they did not possess the information on police deployment required to provide instrumental variable estimates.

A final strand of related work considers policy initiatives where particular police forces were given more resources to combat crime. Two examples are 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 (2007). Both of these studies adopt a treatment-comparison 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 get more resources and so it is hard to be confident that the analyses remove all the biases associated with this.

In contrast, the focus of the current paper is on what happened to criminal activity following a large and unanticipated increase in police presence. The scale of the change in police deployment that we study is much larger than in 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 as part of a police deployment policy stylishly titled "Operation Theseus" by the authorities. During this time period, crime fell significantly in central London relative to outer London. Both the timing of the crime reductions and the types of crime that were more affected 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. Furthermore, we are unable to find any strong evidence of either temporal or spatial displacement effects arising from the six-week police intervention.

The remainder of the paper is organized as follows. Section 2 describes the events of July 2005 and goes over the main modelling 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 discussion is that it considers insights from the growing economics of terrorism literature. In Section 3 we describe the data used and provide some initial descriptive analysis. Section 4 presents the statistical results, and a range of additional empirical tests. Section 5 concludes.

2. Crime, Police and the London Terror Attacks

The Terror Attacks

In July 2005 London's public transport system was subject to two waves of terror attacks. The first wave occurred on Thursday 7th July 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 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. As our later descriptive analysis shows, the two sets of attacks were associated with an increase in police deployment of approximately 35% in the affected central London boroughs in the six weeks following the first attack.

Crime-Police Endogeneity

We use the police response to the terror attacks as a means of identifying the impact of police on crime since the weeks following the attacks saw a large, unanticipated increase in police presence. Before continuing, it is useful to recall the basic endogeneity problem besetting the police-crime relationship. Standard economic models of criminal participation (Becker, 1968; Ehrlich, 1973) postulate that crime is a function of opportunities and deterrence. Thus more police should deter crime, predicting a negative empirical relationship between the two. However, there are many situations in which the direction of causation seems to run in the opposite direction (e.g. when more police are drafted in to high crime areas because crime is high there).[4]

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 to the causal negative impact of police on crime predicted by the basic economic model of crime. It is therefore clear that considerable care and attention needs to be taken when empirically studying the direction of causation in the crime-police relation.

Terror Attacks, Crime and Correlated Shocks

Di Tella and Schargrodsky (2004) were first to use police allocation policies in the wake of terror attacks as a source of variation to circumvent the endogeneity problem. Using 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 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 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 police-crime relationship. The deployments they consider cover a more general area but (as already discussed) are speculative since they are not able to quantify them with data on police numbers or hours.

[4] 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: 270].

Both of these papers touch on the issue of correlated shocks to observables and unobservables. However, in our London example this could be a greater concern since the terrorist attacks (four detonated bombs and a further four unsuccessful attempts) were a more significant, dislocating event for the city. Therefore, in thinking about the question of 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:

$$C_{jt} = \alpha + \delta P_{jt} + \lambda X_{jt} + \mu_j + \tau_t + \upsilon_{jk} + \varepsilon_{jt} \tag{1}$$

where C_{jt} denotes the crime rate for borough j in period t, P_{jt} the level of police deployed 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 (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 (e.g. from 1-12 for monthly or 1-52 for a weekly frequency).[5]

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 modelling since crime is strongly persistent across areas over time. In practical terms, this eliminates the borough-level fixed effect and the borough-specific seasonality terms, yielding:

$$(C_{jt} - C_{j(t-k)}) = \alpha + \delta(P_{jt} - P_{j(t-k)}) + \lambda(X_{jt} - X_{j(t-k)}) + (\tau_t - \tau_{t-k}) + (\varepsilon_{jt} - \varepsilon_{j(t-k)}) \tag{2}$$

Note that the $\tau_t - \tau_{t-k}$ difference 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 as follows:

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

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

Using 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

[5] 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.

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. For example, it is possible that Z could affect the elements of ΔX_{jt} creating additional channels via which terrorist attacks could influence crime rates.

What are these potential impacts or channels? The economics of terrorism literature stresses that the impacts of terrorism can be strong, but generally turn out to be temporary (OECD, 2002; Bloom, 2007). 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 by affecting crime in a way that is correlated with the police response. In particular, three channels demand consideration. First 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 could have 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 (Greater London Authority, 2005). Finally, there may be a 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 more subtle unobservable behaviour.

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

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

In (4), X^1_{jt} is a set of exogenous control variables (observable to researchers), that is, observable factors such as area-level labour market conditions that change slowly and are unlikely to be immediately affected by terrorist attacks (if at all). The second X^2_{jt}

vector represents the observable factors that change more quickly and are therefore vulnerable to the dislocation caused by terrorist attacks. As discussed above, here we are thinking primarily of factors such as travel patterns which could influence the potential supply of victims to crime 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 wake 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 one might expect 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

Another issue that could potentially affect our identification strategy is that of crime displacement. Since the police intervention that we consider affected the costs of crime across locations and time, it could be expected that criminals would take these changes into account and adjust their behavior accordingly. This raises the possibility that criminal activity was either diverted into other areas (e.g. the comparison group of boroughs) during Operation Theseus or postponed until after the extra police presence was withdrawn. The implication then is that simple differences-in-differences estimates of the police effect on crime would be upwardly biased if these offsetting displacement effects are not taken into account.

As Freeman's (1999) survey notes the work on crime displacement issues in economics is still very limited, with the criminological literature on the topic finding only modest effects. However, the recent paper by Jacob, Lefgren and Moretti (2007) outlines a dynamic framework for understanding crime displacement. Their main focus is temporal displacement. They hypothesize that the rationale for temporal displacement will differ across crime types, with property crimes subject to a potential income effect (in cases where the value of property is high) and violent crimes subject to effects arising from the diminishing marginal utility of violence.[6] Jacob et al (2007) are relatively silent on spatial displacement but there are some obvious points to make. Spatial displacement effects will depend on changes in the relative costs of crime across locations as well as the mobility characteristics of criminals, that is, the extent to which criminals are able to change their location in response to variations in costs.

Following this analysis, we will test for temporal and spatial displacement effects as part of our empirical analysis described below. Specifically, we look at temporal displacement effects in the weeks following Operation Theseus as well as "between London borough" spatial displacement. There are two types of spatial displacement relevant to our quasi-experiment. The first is the displacement of crime from treatment to comparison boroughs during Operation Theseus, while the second is "within-borough" displacement inside treatment boroughs. For example, within-borough displacement would take place in cases where one part of a treatment borough was less heavily treated than another. Although we are not able to specifically test for these within-borough effects using the available data we still discuss the possible biases these effects could create below.

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 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. There are 32

[6] For example, where a violent crime is committed in a given week it is less likely to occur again in the following week. Intuitively, a criminal who "settles a score" in one week derives less utility from repeating the crime soon after.

London boroughs as shown on the map in Figure 1.[7] There are 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. 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.

Table 1 (and Appendix Table A1 in more detail) show some 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 of an increased police presence on the two. The susceptible crimes we consider are violence against the person, theft and handling, and robbery. The non-susceptible crimes are burglary, criminal damage (e.g. vandalism or graffiti) and sexual offences. We expect the latter group of crimes to be less affected by the increased deployment because 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 difference in crime rates is an issue we return to in our empirical specifications below when discussing pre-policy (or more precisely, pre-attack) trends.

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 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

[7] The City of London has its own police force and so this small area is excluded from our analysis.

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 differences-in-differences approach, defining a treatment group of boroughs in central and inner London 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. First, the data allow us to measure the increase in total hours worked in the period after the attacks. The increase in total hours was accomplished through the increased use of overtime shifts across the police service and this policy lasted approximately six weeks. Secondly, the police data contain 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 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, with individual borough allocations being proportional to the number of Tube stations in the borough.[8] 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

[8] We say "mainly reallocated" due to the fact that some mobile patrols crossed into adjacent boroughs and because some bordering areas of boroughs were the site of some small deployments. A good case here is the southern tip of Hackney borough (between Islington and Tower Hamlets). However, the majority of Hackney was not treated by the policy (since this borough is notoriously lacking in Tube station links) so we exclude it from the 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 modelling 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 greatly 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 highly visible 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 hypothesis of differential deployment across areas.[9]

Given the high visibility of the deployment we therefore think of it as potentially exerting a deterrent effect on public, street-level crimes such as thefts and violent assault. We test for this prediction in the later modelling section. As already noted we therefore classify crimes according to whether they are more or less susceptible to a public deterrence mechanism.

Basic Differences-in-Differences

In Table 3 we compare 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. 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

[9] 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.

of 3.59.[10] Thus 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 and police hours. These are then differenced across the treatment (T = 1) and comparison (T = 0) groups to get the customary differences-in-differences (DiD) estimate.

The first panel of Table 3 shows the unconditional DiD estimates for police hours. It is clear that the treatment boroughs experienced a very 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 for our experiment as the scale. 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. In practice, 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 greatly simplifies our later analysis of displacement effects 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 over time. Secondly, we mark out the attack or "policy-on" period in 2005 along with the comparison period in the previous year. This reveals a clear, sharp discontinuity in police deployment. Police hours worked in the treatment group rise

[10] The one day difference in calendar date across years ensures we are comparing the same days of the week.

immediately after the attack and fall sharply at the end of the six week Operation Theseus period.

The visual evidence for the crime rate is less decisive because the weekly crime rates are clearly more volatile than the police hours data. This is to be expected insofar as police hours are largely determined centrally by policy-makers, while crime rates are essentially the outcomes of decentralized activity. This volatility does raise 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 – a classic problem in the literature (Donohue, 1998). After the correlated shocks issue this is probably the biggest modelling 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 $POST_t*T_b$, 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 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 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 differences-in-differences estimates of the impact of the terror attacks on police deployment and crime.

These reduced form equations can 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:

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

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 $\delta_3 = \delta_2/\delta_1$.

Finally, note that in some of the reduced form specifications that we consider below we split the $POST_t*T_b$ into two distinct post 7/7 time periods so as to distinguish the "post-policy" period after the end of Operation Theseus. This term is added in order to directly to test for any persistent effect of the police deployment, and importantly to explicitly focus upon the second 'experiment' when police levels fell sharply back to their pre-attack levels. Thus the reduced forms in (5) and (6) now become:

$$p_{bt} - p_{b(t-52)} = \alpha_1 + \beta_1 POST_t + \delta_{11} (POST_t^1 * T_b) + \delta_{12} (POST_t^2 * T_b) + \lambda_1 (x_{bt} - x_{b(t-52)}) + (u_{1bt} - u_{1b(t-52)}) \qquad (8)$$

$$c_{bt} - c_{b(t-52)} = \alpha_2 + \beta_2 POST_t + \delta_{21} (POST_t^1 * T_b) + \delta_{22} (POST_t^2 * T_b) + \lambda_2 (x_{bt} - x_{b(t-52)}) + (u_{2bt} - u_{2b(t-52)}) \qquad (9)$$

In these specifications $POST_t^1$ represents the six-week policy period immediately after the July 7th attack when the police deployment was in operation while $POST_t^2$ covers the time period subsequent to the deployment until the end of the year (that is, from the 19th of August 2005 until December 31st 2005).[11] Also note that a test of $\delta_{11} = \delta_{12}$ (in the police equation, (8)) or $\delta_{21} = \delta_{22}$ (in the crime equation, (9)) amounts to a test of temporal variations in the initial six week period directly after July 7th as compared to the remainder of the year.

[11] As we discuss later police deployment levels in London boroughs were returned to their pre-attack baselines after the end of Operation Theseus.

Basic Differences-in-Differences Estimates

Table 4 provides the basic reduced form OLS and structural IV results for the models outlined in equations (5)-(9). 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 form 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.[12] 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 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. The final three columns of the Table therefore 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

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[12] 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: the estimate (standard error) based on collapsed data comparable to the T*Post-Attack 1 estimate in column (6) of Table 2 was -.112 (.027).

(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 -.32. This implies that a 10 percent increase in police activity reduces crime by around 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 labelled '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

So far the results we have considered use a measure of total crimes. However, the potential heterogeneity of the overall effect by the type of crime is clearly important to the experiment considered here. The pattern of the impact by crime type is an important falsification exercise. The main feature of Operation Theseus was a highly visible public deployment of police officers in the form of foot and mobile patrols, particularly around major transport hubs. We could therefore expect any police effect to be operating mainly through a deterrence technology, that is an increase in the probability of detection for crimes committed in or around public places. As a result, the crime effect documented in Tables 3 and 4 should be concentrated in crimes types susceptible to this type of technology.

In Table 5 we estimate the reduced form treatment effect across the 6 major categories defined by the Metropolitan Police – thefts, violent crimes, sexual offences, robbery, burglary and criminal damage This Table shows a clear dividing line in the incidence of crime effects by type. Strongly significant effects are found for thefts and violent crimes which are comprised of crimes such as street-level thefts (picking pockets, snatches, thefts from stores, motor vehicle-related theft and tampering) as well as street-level violence (common assault, harassment, aggravated bodily harm). Also of note is the lack of any effect for burglary. As a group of crimes that mainly occurs at night and in private dwellings this is arguably the crime category that is least susceptible to a public deterrence technology. In Table 6 we therefore divide these major categories into a group of crimes potentially susceptible to Operation Theseus (thefts, violent crimes and robberies) and a group of remaining non-susceptible crimes (burglary, criminal damage and sexual offences). The point estimate for our preferred susceptible crimes estimate is -0.131 (column 3, panel (I)) which compares to an estimate of -0.109 for total crimes in column (7) of Table 4, and a much smaller (in absolute terms) and statistically insignificant estimate of -0.033 for non-susceptible crimes (column (3), panel (II)). We therefore use this susceptible crimes classification as the main outcome variable in the remainder of our analysis.

Timing

The previous section cited the volatility of the crime rates and timing in general as an important 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 or "placebo" 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. Practically, this exercise is therefore able to test whether our 6-week

Operation Theseus effect is merely a product of time-series volatility or variation that is equally likely to occur in other sub-periods.

We plot the coefficients and confidence intervals for all 52 weeks in Figure 4. 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. Here it is interesting to note that the pattern of six consecutive weeks of significant, negative treatment effects in the crime rate is 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.

Figure 5 then provides a similar plot for six-week placebo policy periods. That is, we define a set of hypothetical placebo policy periods each lasting six weeks and include the associated interaction terms in our baseline regression for susceptible crimes, plotting the coefficients and confidence intervals for each of these "policy on" periods.[13] The results in Figure 5 highlight the distinctiveness of the policy effect in the Operation Theseus period, which is the only effect significant at the 1% level. Obviously, this extra precision is the result of the six consecutive weekly effects seen in the previous graph.

As a further check on the issue of volatility we also make use of some monthly, borough-level crime data available from 2001 onwards.[14] These data allow us to examine whether there is a regular pattern of negative effects in the middle part of the year. Results using this data are reported in Table 7. 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-

[13] Note that the two placebo periods at either end of our sample run for less than six weeks. The first placebo period in the year has a duration of three weeks while the final period lasts for only one week.

[14] Note that the daily crime data we use to construct our weekly panel is only available since the beginning of 2004.

2005 time period. This gives us further confidence that our estimate for this year is a unique event that cannot be likened to 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 observable variable we consider here concerns transport decisions and we study this using data on tube journeys 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. We aggregate the journeys information to borough level and normalise it with respect to the number of tube stations in the borough.

Figure 6 shows how journeys changed year-on-year terms across the treatment and comparison groups. There is no evidence of a discontinuity in travel patterns corresponding exactly to the timing of the six week period of increased police presence. In fact the Figure shows a smoother change in tube usage, with the number of journeys trending back up and returning only gradually to pre-attack levels by the end of the year, but with no sharp discontinuity like the police and crime series.

Table 8 formally tests for this difference in the journeys across the treatment and comparison groups. It shows reduced form estimates that use 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 22% (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 7th July. 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 effect is now smaller at 13%. Importantly, on timing, notice that the reduced use of the tube persisted and carried on well after the police numbers had gone back to their original levels.

This final point about the *persistent* effect of the terror attacks on tube-related travel decisions is useful for illustrating the correlated shocks issue. As Table 8 shows, tube travel continued to be significantly lower in the treatment group for the whole period until the end of 2005. For example, columns (2) and (4) show that there was a persistent 10.3% fall in tube travel after the police deployment was completed, which is approximately half of the 21% effect seen in the Operation Theseus period. If the change in travel patterns induced by the terrorist attacks was responsible for reducing crime then we would expect some part of this effect to continue after the deployment.

At this point it is worth re-considering the week-by-week evidence presented in Figure 4(b). A unique feature of the Operation Theseus deployment is that it provides us with two discontinuities in police presence, namely the way that the deployment was discretely switched on and off. The first discontinuity is of course related to the initial attack on July 7th. Notably, along with an increased police deployment this first discontinuity is associated with a similarly timed shift in observable and unobservable factors. In particular, this first discontinuity in police deployment was also accompanied by a similarly acute shift in unobservable factors (that is, widespread changes in behaviors and attitudes towards public security risks – “panic” for shorthand). Because these two effects coincide exactly it is legitimate to raise the argument that the reduction in crime could have been partly driven by the shift in correlated unobservables.

However, the second discontinuity provides a useful counterfactual. In this case the police deployment was “switched off” in an environment where unobservable factors were still in effect. Importantly, the Metropolitan Police never made an official public announcement that the police deployment was being significantly reduced. This decision therefore limits the scope for unobservable factors to explicitly follow or respond to the police deployment. It is therefore interesting to compare the treatment effect estimates immediately before and after the deployment was switched off in Figure 4(b). The estimated treatment interaction in week 85 (the last week of the police deployment) was -0.107 (0.043) while the same interaction in the two following weeks are estimated as being -0.040 (0.061) and -0.041 (0.045). This shows that crime in the treatment group increased again at the exact point that the police deployment was withdrawn. Furthermore, this discrete shift in deployment occurred as observable and unobservable factors that could have affected crime were still strongly persisted (for example, recall the -10.3% gap in tube travel evident in Table 8 for the period after the deployment was withdrawn). More generally, this second discontinuity illustrates the point that any correlated, unobservable shocks affecting crime would need to be exactly and exquisitely timed to account for the drop in crime that occurred during Operation Theseus. Our argument then is that such timing is implausible given the decentralized nature of the decisions driving changes in unobservables. That is, the unobservable shocks are the result of individual decisions by millions of commuters and members of the public while Operation Theseus was a centrally determined policy with a clear "on" and "off" date. Indeed, the evidence on the police deployment that we show in this paper indicates that the Metropolitan Police's response was quite deterministic. That is, deployment levels were raised in the treatment group while carefully keeping levels constant in the comparison group. Furthermore, police deployment levels were effectively restored to their pre-attack levels after Operation Theseus.[15]

Further support for the hypothesis that changing travel patterns did not match the timing of change in police presence is presented in Table 9. This Table uses Labour Force Survey (LFS) data to show that there is 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 that modes of transport activity were affected rather than the volume of travel.

The issue of work travel decisions also uncovers a source of variation that we are 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

[15] Our discussions with MPS policy officers indicate that big changes in the relative levels of ongoing police deployment in different boroughs occur only rarely. Relative levels of police deployment are determined mainly by centralised formulas (where the main criteria are borough characteristics) with changes determined by a centralised committee.

risks associated with travel on public transport people would adjust their behaviour differently for non-work travel. That is, the travel decision is less elastic for the travel to work decision compared to that for non-work travel. We would therefore 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 10 suggest that this was the case with tube journeys falling by 28% on weekends as compared to 20% on weekdays.

Thus there is an important 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 to reflect a more pronounced effect of police on crime on weekends. Following this, Table 11 then performs the exercise of re-estimating the baseline models of Table 5 but excluding all observations relating to weekends.[16] This results in very similar coefficient estimates to Table 5 and only slightly larger standard errors. Importantly, 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 to perfectly match the police and crime changes. However, basic survey evidence on risk attitudes amongst Inner and Outer London residents, reported in Table 12 suggests that there is not a significant difference in the types of attitudes that would drive a set of significant, differential unobservable shocks across our treatment and control groups. Indeed, the responses given by Inner and Outer London residents are closely comparable.[17] The attacks almost certainly had an impact on risk attitudes but they seem to be very similar in the treatment and control areas of London that we study. From this we conclude that the effect of unobservables is likely to be minimal.

[16] Recall that our crime, police and tube journeys data are 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.
[17] Note that since the underlying micro-data for these surveys were unavailable we were not able to calculate standard errors for these estimates or conduct any other statistical analysis.

Possible Crime Displacement

The final empirical issue we consider is that of crime displacement. We can only do this in a limited manner in that we 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 different estimated effects from considering 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 13, we therefore consider estimates which only use boroughs which are geographically closer to the treatment boroughs as controls. We consider 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 were displaced to these geographically closer boroughs then we would see different estimates from the baseline estimates considered earlier. In particular, if crime rose in these nearby boroughs as a result of displacement then we would expect a smaller effect in the treatment group.

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 13. The Table gives the crime reduced forms and in each case the estimates are similar, 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. At least according to this simple test, we cannot uncover evidence of important crime displacement effects.

Of course, the test in Table 13 examines the possibility of between borough displacement, that is, the movement of criminal activity across boroughs in response to the prevailing levels of police deployment. As mentioned earlier, it is possible that some within borough displacement occurred and that the distribution of criminal activity may have changed inside the treatment boroughs. This is hard to credibly test for but insofar as it could be an influence it would impart a downward bias on our treatment effect estimates. That is, since our treatment effect estimates essentially pick up the between-borough shift in crime they are estimates that already net out any countervailing, within-borough changes in the treatment group boroughs.

Finally, the issue of temporal displacement can be best addressed by referring back to the week-by-week estimates of treatment effects in Figure 4(b). There is no evidence of a significant positive effect on crime in the periods immediately after the end of Operation Theseus. This would seem to run against the hypothesis of inter-temporal substitution in criminal activity although (as with spatial displacement) the inherent modeling problem here is that displacement effects are diffuse by their very nature. Study of the displacement effects of crime (in temporal, spatial or other dimensions) does, however, seem to be an important research priority for the future.

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. First, the scale of the police deployment we consider is much greater than the highly localized responses that have previously been studied. Together with the unique police hours data we use, 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. This provides a clean setting to test the relationship between crime and 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 this case, we would expect that the July 2005 attacks affected police deployment as well as travel patterns and individual behaviour throughout London. Therefore, insofar as 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. 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 7th July 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 differences-in-differences 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 with other papers like ours that adopt a 'quasi-experimental' approach, one might have some concerns about the study's external validity. However, using a very different approach from other papers looking at the causal impact of crime, our preferred IV causal estimate of the crime-police elasticity is approximately -0.32, which (in absolute terms) is slightly below the existing results in the literature (e.g. those of Levitt, 1997, and Corman and Mocan, 2000), but is very much in the same ballpark as these other studies. Moreover, because of the scale of the deployment change and the very clear coincident timing in the crime fall, this elasticity is very precisely estimated and supportive of the basic economic model of crime in which more police reduce criminal activity.

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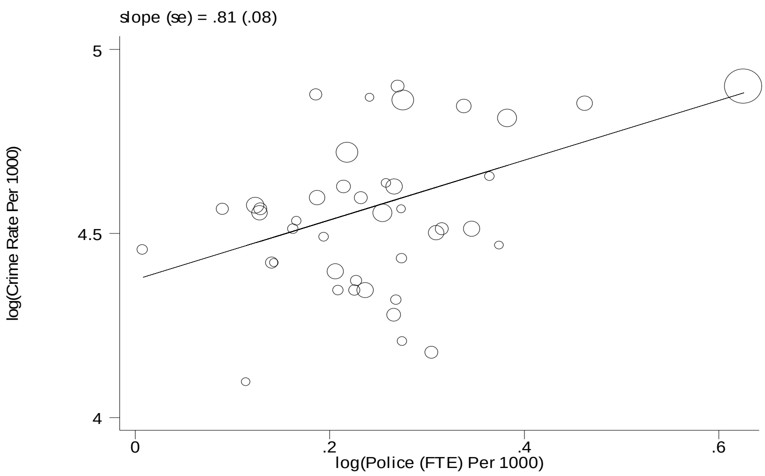
Figure 1: A Map of London Boroughs

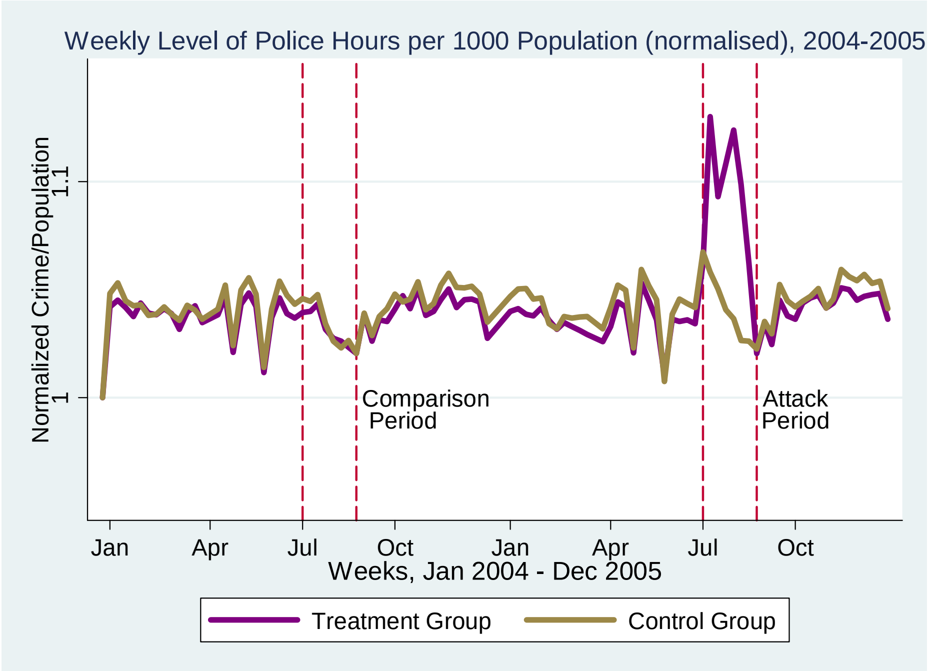
Figure 2: Crime Rates and Police, 42 Police Force Areas of England and Wales, 2005 to 2006



Notes: 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. There are 42 areas because the Metropolitan and City of London police are aggregated. 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: Police Deployment and Crime Graphs 2004-2005, Treatment versus Comparison Groups.

(a) Police Deployment – Hours/Population



(b) Total Crimes – Crimes/Population

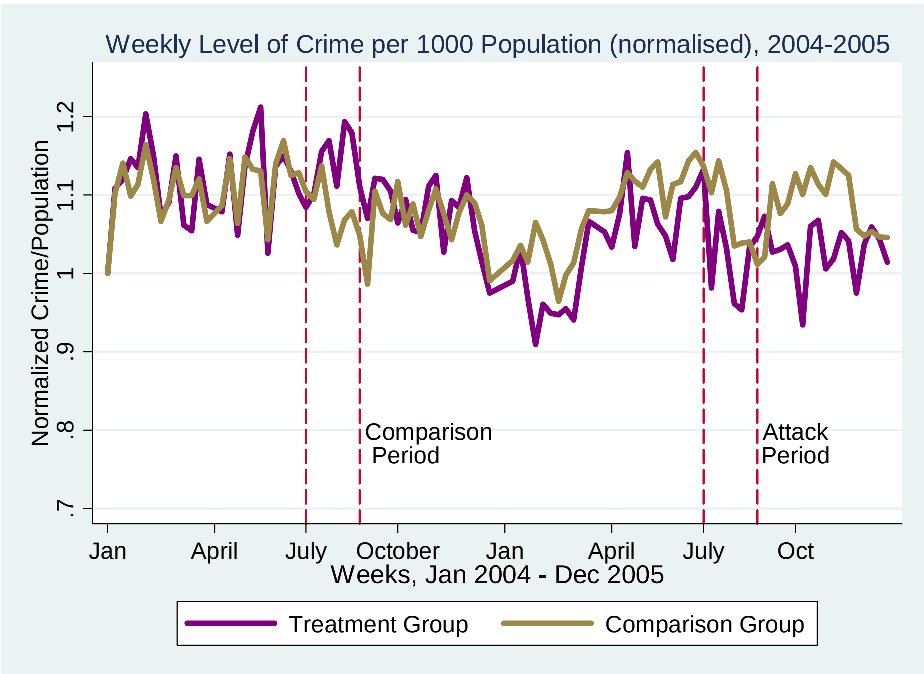
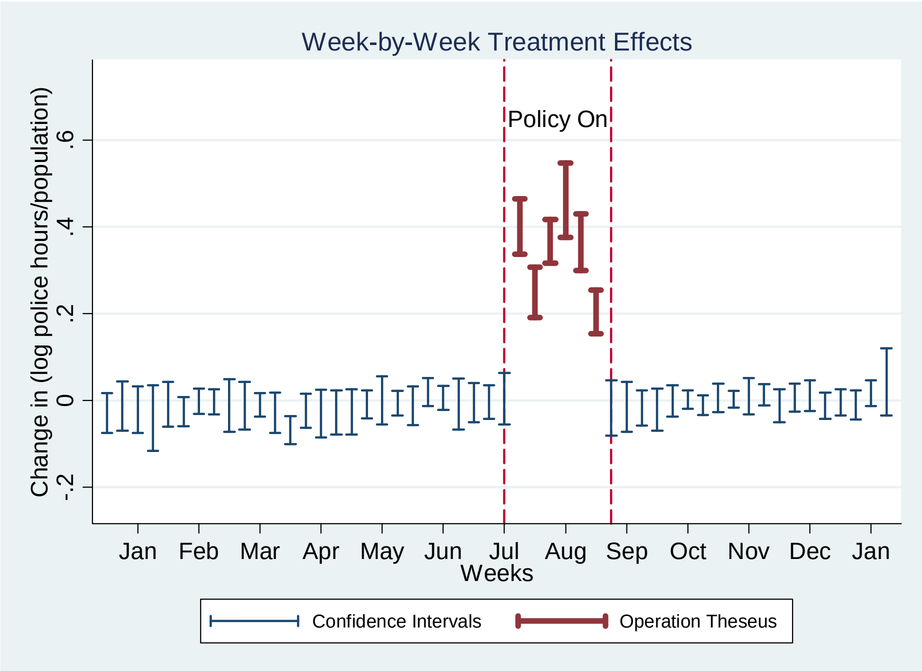


Figure 4: Week-by-Week Policy Effects,
Borough Level Models, 2004-2005.

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



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

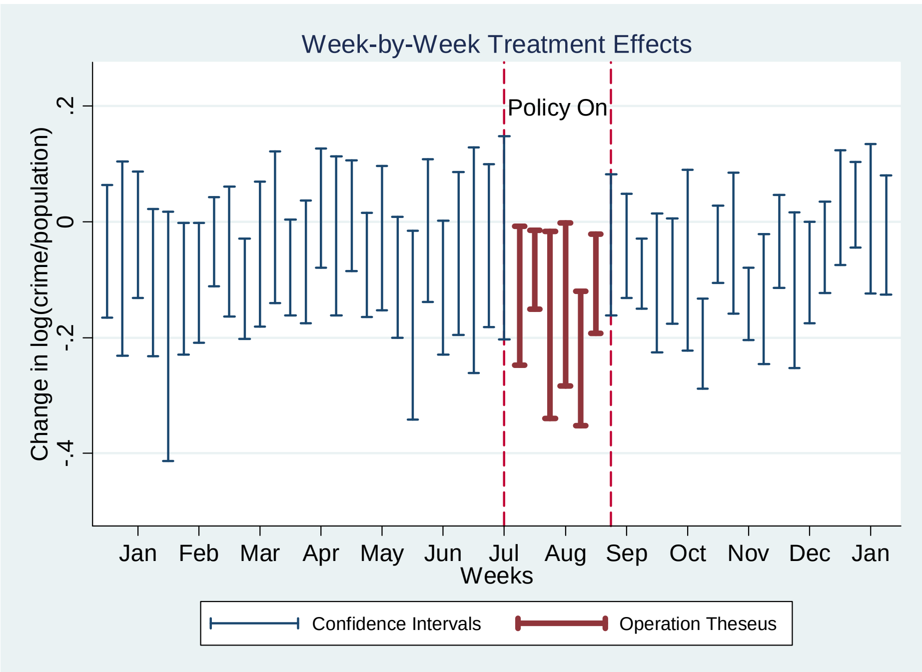


Figure 5: Six-Week Placebo Policy Effects – Susceptible Crimes Borough Level Models, 2004-2005.

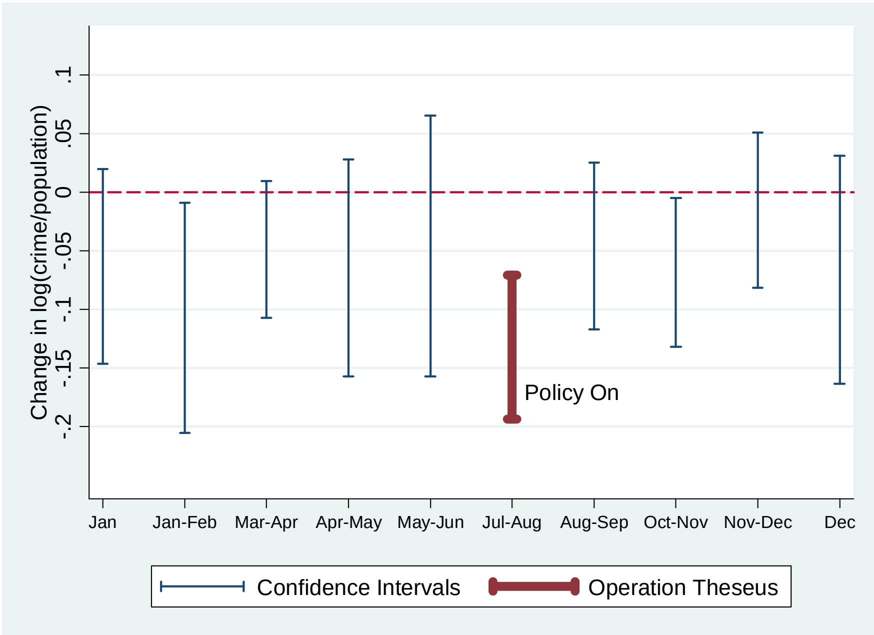


Figure 6: Year-on-Year Weekly Changes in Tube Journeys, 2004-2005.

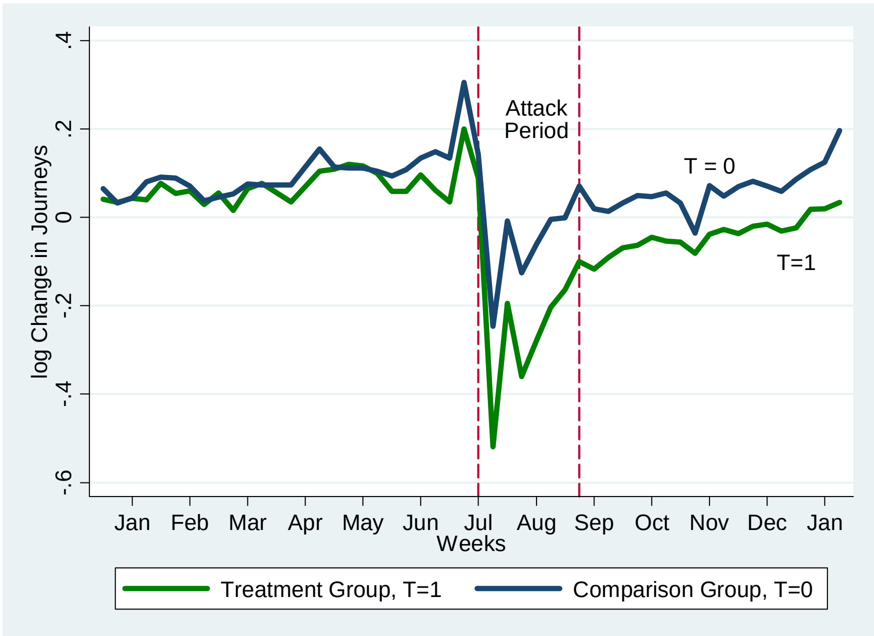


TABLE 1: DISTRIBUTION OF CRIME IN LONDON BY MAJOR CATEGORY, 2004-2005.

	(1) % of All Crimes	(2) Crime Rate (per 1000)	(4) % Occurring in Treatment Group	(5) Crime Rate in Treatment Group (per 1000)
(A) Susceptible Crimes				
Theft and Handling	44.0	53.1	28.0	117.0
Violence Against the Person	22.6	27.2	17.7	38.0
Robbery	4.6	5.5	15.5	6.7
(B) Non-Susceptible Crimes				
Burglary	12.3	14.8	17.4	20.2
Criminal Damage	15.5	18.7	13.6	20.0
Sexual Offences	1.1	1.3	21.8	2.3
Total	**100.0**	**120.6**	**21.3**	**204.2**

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

TABLE 2: POLICE PATROLS AFTER JULY 7TH, 2005.

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

Notes: Source is IPSOS MORI Survey. 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?" Interviews conducted on 22-26 September 2005.

TABLE 3: POLICE DEPLOYMENT AND MAJOR CRIMES, DIFFERENCES-IN-DIFFERENCES, 2004-2005.

	(A) Police Deployment (Hours worked per 1000 Population)			*(B) 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
Differences-in-Differences (Levels)			70.65*** (5.28)			-0.42*** (0.11)
Differences-in-Differences (Logs)			0.346*** (0.028)			-0.111*** (0.027)

Notes: 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. Treatment group (T = 1) defined as boroughs of Westminster, Camden, Islington, Tower Hamlets and Kensington-Chelsea. Comparison group (T = 0) defined as other boroughs of London. Police deployment defined as total weekly hours worked by police staff at borough-level.

TABLE 4: DIFFERENCE-IN-DIFFERENCE REGRESSION ESTIMATES, POLICE DEPLOYMENT AND TOTAL CRIMES, 2004-2005.

	(A) Police Deployment (Hours Worked per 1000 Population)				(B) Total Crimes (Crimes per 1000 Population)				(C) OLS		(D) IV Estimates		
	Full	Split	+Controls	+Trends	Full	Split	+Controls	+Trends	Levels	Differences	Full	Split	+Trends
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
T*Post-Attack	0.081***				-0.052**								
	(0.010)				(0.021)								
T*Post-Attack1		0.341***	0.342***	0.356***		-0.111***	-0.109***	-0.056*					
		(0.028)	(0.029)	(0.027)		(0.027)	(0.027)	(0.030)					
T*Post-Attack2		-0.001	0.001	0.014		-0.033	-0.031	0.024				-0.031	0.026
		(0.011)	(0.010)	(0.016)		(0.027)	(0.028)	(0.054)				(0.029)	(0.054)
ln(Police Hours)									0.785***				
									(0.053)				
Δln(Police Hours)										-0.031	-0.641**	-0.320***	-0.158*
										(0.051)	(0.301)	(0.092)	(0.089)
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	
Number of Observations	1664	1664	1664	1664	1664	1664	1664	1664	3328	1664	1664	1664	1664

Notes: 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 5: TREATMENT EFFECTS BY MAJOR CATEGORY OF CRIMES

PANEL A	**THEFTS, VIOLENCE AND SEX CRIMES**					
Crime Category	Thefts		Violence		Sex Crimes	
	(1)	(2)	(3)	(4)	(5)	(6)
T*Post-Attack1	-0.139***	-0.082*	-0.124***	-0.108***	-0.078	-0.089
	(0.044)	(0.045)	(0.043)	(0.034)	(0.124)	(0.139)
T*Post-Attack2	-0.017	0.044	-0.054	-0.038	-0.080	-0.090
	(0.039)	(0.085)	(0.032)	(0.056)	(0.082)	(0.086)
Trends	No	Yes	No	Yes	No	Yes
Number of Boroughs	32	32	32	32	32	32
Number of Observations	1664	1664	1664	1664	1664	1664
PANEL B	**ROBBERY, BURGLARY AND CRIMINAL DAMAGE**					
Category	Robbery		Burglary		Criminal Damage	
	(1)	(2)	(3)	(4)	(5)	(6)
T*Post-Attack1	-0.132	-0.013	-0.035	-0.029	-0.047	-0.005
	(0.119)	(0.130)	(0.057)	(0.067)	(0.052)	(0.041)
T*Post-Attack2	-0.090	0.023	-0.093	-0.078	-0.018	0.020
	(0.098)	(0.149)	(0.059)	(0.075)	(0.043)	(0.057)
Trends	No	Yes	No	Yes	No	Yes
Number of Boroughs	32	32	32	32	32	32
Number of Observations	1664	1664	1664	1664	1664	1664

Notes: All specifications include week fixed effects. Standard clustered by borough in parentheses. Boroughs weighted by population. T*Post-Attack1 and T*Attack2 defined as per Table 4. Treatment group also defined as per Table 4. List of minor crime categories per major category is given in Table A1.

TABLE 6: SUSCEPTIBLE CRIME VERSUS NON-SUSCEPTIBLE CRIMES, 2004-2005.

(I) SUSCEPTIBLE CRIMES									
	(A) Reduced Forms				*(B) OLS*		*(C) IV Estimates*		
	Full	Split	+Controls	+Trends	Levels	Differences	Full	Split	+Trends
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T*Post-Attack	-0.056**								
	(0.023)								
T*Post-Attack1		-0.131***	-0.132***	-0.067*					
		(0.031)	(0.031)	(0.035)					
T*Post-Attack2		-0.033	-0.033	0.033				-0.032	0.036
		(0.030)	(0.030)	(0.063)				(0.030)	(0.063)
ln(Police Hours)					0.952***				
					(0.056)				
Δln(Police Hours)						-0.019	-0.694**	-0.386***	-0.189*
						(0.063)	(0.336)	(0.105)	(0.105)
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	3328	1664	1664	1664	1664

(II) NON-SUSCEPTIBLE CRIMES									
	(A) Reduced Forms				*(B) OLS*		*(C) IV Estimates*		
	Full	Split	+Controls	+Trends	Levels	Differences	Full	Split	+Trends
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T*Post-Attack	-0.048*								
	(0.024)								
T*Post-Attack1		-0.033	-0.023	-0.015					
		(0.026)	(0.027)	(0.031)					
T*Post-Attack2		-0.053	-0.043	-0.033				-0.043	-0.032
		(0.034)	(0.037)	(0.045)				(0.037)	(0.045)
ln(Police Hours)					0.327***				
					(0.046)				
Δln(Police Hours)						-0.056	-0.597*	-0.068	-0.043
						(0.094)	(0.337)	(0.079)	(0.088)
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	3328	1664	1664	1664	1664

Notes: All specifications include include week fixed effects. Clustered standard errors in parentheses. Boroughs weighted by population. Susceptible Crimes defined as: Violence Against the Person; Theft and Handling; Robbery. Non-Susceptible Crimes defined as: Burglary and Criminal Damage; Sexual Offences. Treatment group definitions and T*Post-Attack terms defined as per Table 4. Controls also defined as per Table 4.

TABLE 7: EXTENDED TIME PERIOD ANALYSIS BASED ON MONTHLY DATA, (BOROUGH LEVEL MODELS, DIFFERENCED ACROSS YEARS, 2001-2005)

(A) Change in log(Susceptible Crimes Per 1000 Population)

	(1)	(2)	(3)	(4)
Year on Year Changes	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 (T)	0.030	-0.059	-0.056	-0.097
Control boroughs (C)	0.071	-0.021	-0.026	0.007
T – C Gap	-0.041	-0.038	-0.030	-0.104***
	(0.030)	(0.030)	(0.042)	(0.030)

(B) Change in log(Non-Susceptible Crimes Per 1000 Population)

	(1)	(2)	(3)	(4)
Year on Year Changes	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 (T)	-0.025	-0.120	-0.120	-0.054
Control boroughs (C)	0.001	-0.065	-0.065	-0.005
T – C Gap	-0.026	-0.055	-0.055	-0.049
		(0.051)	(0.051)	(0.033)

Notes: All models estimated in terms of seasonal differences (i.e. differenced relative to the same month in the previous year). Clustered standard errors in parentheses. Boroughs weighted by population. Treatment group defined as boroughs of Westminster, Camden, Islington, Tower Hamlets and Kensington-Chelsea. “Policy-on” period defined as July-August. Crime defined according to Susceptible and Non-Susceptible categories given in Table 5.

TABLE 8: CHANGES IN TUBE JOURNEYS, BEFORE AND AFTER JULY 7TH 2005.

	(A) log(Journeys/ Number of Stations)		*(B)* log(Journeys/ Number of Open Stations)	
	(1)	(2)	(3)	(4)
T*Post-Attack1	-0.212***	-0.215***	-0.133***	-0.137***
	(0.015)	(0.016)	(0.013)	(0.016)
T*Post-Attack2	-0.105***	-0.103***	-0.105***	-0.103***
	(0.007)	(0.006)	(0.007)	(0.007)
Controls	No	Yes	No	Yes
Observations	104	104	104	104

Notes: Borough level data collapsed by treatment and comparison group, 2 units over 52 weeks. All columns include week fixed effects. Standard errors clustered by treatment group unit in parentheses. All regressions weighted by treatment and comparison group populations. Panel B reports results adjusted for closed 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 9: WORK TRAVEL PATTERNS INTO CENTRAL LONDON, BEFORE AND AFTER JULY 7TH.

	(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

Notes: Source is UK Quarterly Labour Force Survey (QLFS), 2004-2005. Standard errors clustered by week. Defined for all employed person aged 18-65 working in Central or Inner London. Column 1 defines all those residing in Outer London and working in Central or Inner London. Column 2 defines all those residing in the South East of England region and working in Central or Inner London.

TABLE 10: CHANGES IN TUBES JOURNEYS –
WEEKDAYS VERSUS WEEKENDS, BEFORE AND AFTER JULY 7TH 2005.

	(A) Log(Journeys/Stations) No Controls		*(B) Log (Journeys / Stations) Controls*	
	(1) Weekdays	(2) Weekends	(3) Weekdays	(4) Weekends
T*Post-Attack1	-0.196***	-0.281***	-0.197***	-0.294***
	(0.015)	(0.028)	(0.017)	(0.035)
T*Post-Attack2	-0.097***	-0.106***	-0.093***	-0.112***
	(0.008)	(0.026)	(0.008)	(0.024)
Controls	No	No	Yes	Yes
Observations	104	104	104	104

Notes: Borough level data collapsed by treatment and comparison group and split according to weekdays and weekends, 2 units over 52 weeks for each set of days. All columns include week fixed effects. Standard errors clustered by treatment group unit in parentheses. All regressions weighted by treatment and comparison group populations.

TABLE 11: ESTIMATED CRIME TREATMENT EFFECTS WHEN EXCLUDING WEEKENDS.

	(A) Susceptible Crimes		*(B) Non-Susceptible Crimes*	
	(1) Reduced Form	(2) IV	(3) Reduced Form	(4) IV
T*Post Attack1	-0.138***		0.022	
	(0.046)		(0.036)	
T*Post Attack2	-0.037		-0.033	
	(0.030)		(0.047)	
ln(Police Deployment)		-0.401***		0.065
		(0.134)		(0.105)
Controls	Yes	Yes	Yes	Yes
No of Boroughs	32	32	32	32
No of Observations	1664	1664	1664	1664

Notes: All specifications include include week fixed effects. Clustered standard errors in parentheses. Boroughs weighted by population. These models estimate similar models to Table 5 but using a count of crimes per 1000 population that excludes all crimes occurring on weekends (i.e.: using only Monday-Friday). Treatment groups, T*Post-Attack terms and Crime Categories defined as in Table 5.

TABLE 12: 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 13: ALTERNATIVE COMPARISON GROUPS AND CRIME DISPLACEMENT.

(A) Susceptible Crimes	(1)	(2)	(3)
	Inner London	Adjacent	Central Ten
T*Post-Attack1	-0.123**	-0.129***	-0.108*
	(0.041)	(0.040)	(0.051)
T*Post-Attack2	-0.042	-0.030	-0.012
	(0.037)	(0.034)	(0.035)
Controls	Yes	Yes	Yes
Trends	No	No	No
No. of Boroughs	13	13	10
No of Observations	676	676	520

(B) Non-Susceptible Crimes	(1)	(2)	(3)
	Inner London	Adjacent	Central Ten
T*Post-Attack1	0.031	0.025	0.055
	(0.046)	(0.044)	(0.038)
T*Post-Attack2	-0.009	-0.024	-0.007
	(0.045)	(0.047)	(0.057)
Controls	Yes	Yes	Yes
Trends	No	No	No
No. of Boroughs	13	13	10
No of Observations	676	676	520

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

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