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 subsequent sharp return back to pre-attack crime levels, 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 of the past 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 can 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.

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 central 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.[1] Thus a major strength of this paper is that we are able to offer explicit instrumental variable-based estimates of the police-crime elasticity which can be compared to other estimates like Levitt's (1997) contribution, that of Corman and Mocan (2000) and Di Tella and Schrgrodsky's (2004) implied elasticity. 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 the deployment of a clear "deterrence technology" (that is, more police on the streets) rather than a simple measure of expenditures (e.g. as in Evans and Owens, 2007, or Machin and Marie, 2009). Arguably, this type of visible increase in

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

police deployment is the main type of policy mechanism under discussion in public debates about the funding and use of police resources.

Furthermore, the effectiveness of police is important in the context of a large criminological literature that has generally failed to find significant impacts of police on crime, even in quasi-experimental studies. 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". Similar emphatic arguments are made by Felson(1994) and Klockars(1984).

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. This police intervention represented the deployment of a very strong deterrence technology. The coverage of police was more sustained, widespread and complete than in any of the studies analysed in the existing literature. We therefore view the scale of this change as important in addressing the paradox of the criminology literature discussed above where it proves hard to detect crime reductions linked to increased police presence. This is particularly the case since during the time period when police presence was heightened, 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. Moreover, when police deployments returned to their pre-attack levels some six weeks later, the crime rate rapidly returned to its pre-attack level. Exploiting these sharp discontinuities in police deployment we estimate an elasticity of crime with respect to police of approximately -.3 to -.4, so that a 10 percent increase in police activity reduces crime by around 3 to 4 percent. Furthermore, we are unable to find evidence of either temporal or spatial displacement effects arising from the six-week police intervention.

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. This is important to generate credibility that the findings inform the crime-police debate rather than being just about an episode where a terror attack occurred. The police deployment data we use are invaluable here as (under certain conditions) their availability make it possible to distinguish the impact of police on crime from any 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 just how clearly and definitively crime seems to respond to a police presence.

The rest 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 Section 3 we describe the data and provide an 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 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 (Tower Hamlets) and Edgware Road (Kensington and Chelsea). A fourth bomb was detonated on a bus in Tavistock Square, Bloomsbury (Camden). The second wave of attacks occurred two weeks later on the 21st July, consisting 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 attacks and all of them were captured by 29th July.

Terror Attacks, Crime and Correlated Shocks

Di Tella and Schargrodsky (2004) were first to use police allocation policies in the wake of terror attacks to circumvent the endogeneity problem of crime and police. Using a July 1994 terrorist attack that targeted the main Jewish center 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. Their effect is large (approximately a 75% reduction in thefts relative to the 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.

Both of these papers touch on the issue of correlated shocks to observables and unobservables. However, in our case of London this could be a greater concern since the terrorist attacks 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:

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

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).[2]

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

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

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:

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

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 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, 2009) in that economic activity tends to recover and normalize itself fairly rapidly. Of course, a sharp but temporary shock would still have

[2] 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.

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 behavior.

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.

Possible Displacement Effects

Another issue that could potentially affect our identification strategy is that of crime displacement. Since the police intervention affected the costs of crime across locations and time, it may be that criminals take these changes into account and adjust their behavior. 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 spatial displacement effects are not taken into account. Temporal displacement can have the opposite effect and we discuss this more in the final empirical section.

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 London boroughs as shown on the map in Figure 1.[3] 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

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

population estimates at borough level, supplied by the Office of National Statistics (ONS) online database.[4]

The police deployment data are at borough level and were produced under special confidential data-sharing agreements with the LMPS. The main data source used is CARM (Computer Aided Resource Management), the police service's human resource management system. This records hours worked by individual officers on a daily basis. We aggregate the deployment data to borough-level since the CARM data is mainly defined at this level. However, there is also useful information on the allocation of hours worked by incident and/or police operation.[5] 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 using a differences-in-differences approach. This rests upon 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.

[4] Web Appendix Table A1 shows some summary statistics on the crime data.

[5] Since the CARM information is also used for calculating police pay it is considered a very reliable measure of police activity. We gained access to this data after repeated inquiries to the MPS. The main condition for access was that we not reveal any strategic information about ongoing or individual, borough-specific police deployment policies.

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.[6] 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 greatly increased and officers were 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 and this is confirmed by public surveys conducted at the time.[7] Given this high visibility we 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 empirical work.

Basic Differences-in-Differences

In Table 1 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 1 the crime rate of 4.03 per 1000 population 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-

[6] 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.

[7] Table A2 of the Web Appendix 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.

period or "policy on" period then runs from July 7th 2005 until August 18th 2005 with a crime rate of 3.59.[8] 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 1 shows 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 (of 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 1 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 police deployment and crime rates is offered in Figures 2a and 2b. 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. As Figure 2a shows, this reveals a clear, sharp discontinuity in police deployment. Police hours worked in the treatment group rise immediately after the attack and fall sharply at the end of the six week Operation Theseus period.

[8] The one day difference in calendar date across years ensures we compare the same days of the week.

The visual evidence for the crime rate in Figure 2b 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 1 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 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 T_b*$POST_t$, where T denotes the treatment boroughs and POST is a dummy variable equal to one in the post-attack period.

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

$$\Delta_{52}p_{bt} = \alpha_1 + \beta_1 POST_t + \delta_1(T_b{*}POST_t) + \lambda_1\Delta_{52}x_{bt} + \Delta_{52}\varepsilon_{1bt} \quad (5)$$

$$\Delta_{52}c_{bt} = \alpha_2 + \beta_2 POST_t + \delta_2(T_b{*}POST_t) + \lambda_2\Delta_{52}x_{bt} + \Delta_{52}\varepsilon_{2bt} \quad (6)$$

Because of the highly seasonal nature of crime noted above, the equations are differenced across weeks of the year (hence the k = 52 subscript in the Δ_k 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:

$$\Delta_{52}c_{bt} = \alpha_3 + \beta_3 POST_t + \delta_3 \Delta_{52} p_{bt} + \lambda_3 \Delta_{52} x_{bt} + \Delta_{52}\varepsilon_{3bt} \quad (7)$$

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 T_b*$POST_t$ 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:

$$\Delta_{52}p_{bt} = \alpha_4 + \beta_4 POST_t + \delta_{41}(T_b{*}POST_t^1) + \delta_{42}(T_b{*}POST_t^2) + \lambda_4 \Delta_{52} x_{bt} + \Delta_{52}\varepsilon_{4bt} \quad (8)$$

$$\Delta_{52}c_{bt} = \alpha_5 + \beta_5 POST_t + \delta_{51}(T_b{*}POST_t^1) + \delta_{52}(T_b{*}POST_t^2) + \lambda_5 \Delta_{52} x_{bt} + \Delta_{52}\varepsilon_{5bt} \quad (9)$$

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).[9] Also note that a test of $\delta_{41} = \delta_{42}$ (in the police equation, (8)) or $\delta_{51} = \delta_{52}$ (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.

Basic Differences-in-Differences Estimates

Table 2 provides the basic reduced form OLS and structural IV results for the models outlined in equations (5)-(9). For comparative purposes, we specify three terms to uncover the differences-in-differences estimate. 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 (in the Table denoting T_b*$POST_t$ from equations (5) and (6) as T*Post-Attack). The adjacent columns ((2)-(4) and (6)-(8)) then split this period in two with one interaction term for the six-week Operation Theseus

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

period (denoting T_b*$POST_t^1$ from equations (8) and (9) as T*Post-Attack1) and another for the remaining part of the year (denoting T_b*$POST_t^2$ as T*Post-Attack2). As already noted, the second term is useful for testing whether there were any persistent effects of the police deployment or any longer-term trends in the treatment group after police deployment fell back to its pre-attack levels.

The findings from the unconditional DiD estimates reported earlier are confirmed in the basic models in Table 2. The estimated coefficient on T*Post-Attack1 in the reduced form police equation shows a 34.1% increase in police deployment during Operation Theseus, and there is no evidence that this persists for the rest of the year (i.e. the T*Post-Attack2 coefficient is statistically indistinguishable from zero). For the crime rate reduced form there is an 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.[10] 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 (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

[10] 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 obtain 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).

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 and Di Tella and Schargrodsky (2004) reported an elasticity of motor vehicle thefts with respect to police of -0.33.

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 use a measure of total crimes. However, heterogeneity of the overall effect by the type of crime is potentially important. 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 through increased visibility, 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 1 and 2 should be concentrated in crime types more susceptible to this type of technology.

We have therefore estimated the reduced form treatment effect across the 6 major crime categories defined by the Metropolitan Police – thefts, violent crimes, sexual offences, robbery, burglary and criminal damage – and these are reported in Table 3. There are differences across these groups, with strongly significant effects 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 crime 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 4 we aggregate 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 2, 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. The estimated elasticity of susceptible crimes with respect to police deployment in the column (8) model is -0.39 and again very precisely determined.

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 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 Figures 3a and 3b. Figure 3a shows the results for police hours repeating the clear pattern seen in Figure 2a 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 3b. 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 despite the noise and volatility of the weekly data.[11]

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 - and subsequent bounce back - 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 5 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 5 formally tests for this difference in the journeys across treatment and comparison groups. It shows reduced form estimates 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 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

[11] As a further check on the issue of volatility we made use of monthly, borough-level crime data available from 2001 onwards (as the daily crime data we use to construct our weekly panel is only available since the beginning of 2004). These data allow us to examine whether there is a regular pattern of negative effects in the middle part of the year. In this exercise, we define year-on-year differences in crime for the July-August period over the a range of intervals: 2001-2002, 2002-2003, 2003-2004, and 2004-2005. The results are shown in Web Appendix Table A3. 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 likened to arbitrary fluctuations of previous years.

through the treatment group were effectively closed down for approximately four weeks after 7th July. To examine 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 5 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 Figures 3a and 3b. 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. 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 5 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.[12] In contrast, shifts in travel patterns by inbound commuters did not match the timing and location of the police response.[13]

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 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

[12] 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.

[13] Further support for the hypothesis that changing travel patterns did not match the timing of change in police presence follows from an analysis of Labour Force Survey (LFS) data. The LFS data gives information on where people live and where they work and so we were able to look at whether the number of inbound commuters to Inner London changed. There is no evidence that the work travel decisions of people commuting in from Outer London and the South-East were affected by the attacks in that changes in the proportion of inbound commuters before and after the attacks are statistically insignificant, lending support to the idea that modes of transport activity were affected more than the volume of travel (see Web Appendix Table A4).

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. This does seem to have been 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, we have re-estimated the baseline models excluding all observations relating to weekends.[14] This results in very similar coefficient estimates and only slightly larger standard error as shown in Table 6. 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 already seen there is a distinctive pattern to the timing of the fall in crime and its subsequent bounce back. 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 also 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, responses on attitudes to the terror attacks given by Inner and Outer London residents are closely comparable.[15] 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.

Possible Crime Displacement

The final empirical issue we consider is that of crime displacement, both spatial and temporal. These two displacement effects have opposing effects on the police-crime relationship we have estimated above. Firstly, spatial displacement into the control areas is likely to impart a downward bias on our estimate. That is, spatial displacement will move criminal activity into the non-treated boroughs, increasing crime there and

[14] 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 prior to aggregating to a weekly frequency.
[15] See Web Appendix Table A5.

lowering the difference-in- difference estimate. Secondly, temporal displacement could impart an upward bias on our estimate. Criminals operating in the treatment group could delay their actions, thus contributing to a larger fall in crime during the policy-on period. However, under a temporal displacement effect there will be a compensating increase in crime in the wake of the policy.

Draca, Machin and Witt (2009) looks at the spatial displacement effects of Operation Theseus in more detail. Here, we note the results of a robustness check where we restrict the comparison group to a set of adjacent and/or Central London boroughs. 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 difference-in-difference estimate.

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.[16] In specifications comparable to the standard baseline estimates discussed earlier (-0.132, with associated standard error 0.031), for susceptible crimes the estimated effects (standard errors) were -0.129 (0.040) for Adjacent and -0.108 (0.051) for Central Ten. Thus 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. In line with the earlier baseline results there was no impact on non-susceptible crimes. As such, it does not seem that spatial displacement is operating strongly at the borough level.[17]

Finally, the issue of temporal displacement can be best addressed by referring back to the week-by-week estimates of treatment effects in Figure 3). As we have already stressed, 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 where criminal activity rebounds after the police deployment is withdrawn from the treatment group.

[16] Adjacent boroughs were: Brent, Hackney, Hammersmith and Fulham, Lambeth, Newham, Southwark and Wandsworth. Central Ten boroughs were: Westminster, Camden, Islington, Kensington and Chelsea, Tower Hamlets (Treatment Group) and Brent, Hackney, Hammersmith and Fulham, Lambeth and Southwark.

[17] As a further check for displacement effects, we also followed the approach of Grogger (2002) in contrasting crime trends between adjacent and non-adjacent comparison boroughs. However, again we could not uncover decisive evidence of between-borough displacement effects.

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 new, highly robust IV-based estimates of the police-crime elasticity. 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.

This research design 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. However, as with other papers like ours that adopt a 'quasi-experimental' approach, one might have some concerns about the study's external validity. 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 to -0.39, which is similar to the existing results in the literature (e.g. those of Levitt, 1997, and Corman and Mocan, 2000, and Di Tella and Schargrodsky, 2004). 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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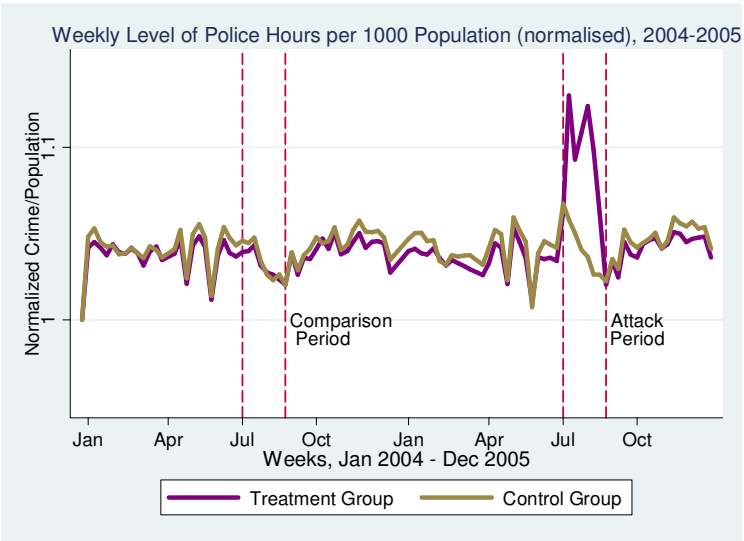
FIGURE 1: MAP OF LONDON BOROUGHS

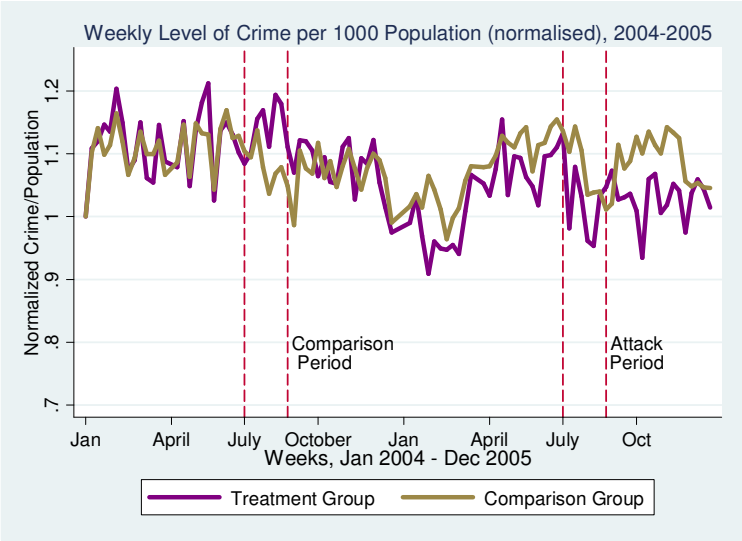
Notes: 32 boroughs of London. Treatment group for Operation Theseus police intervention includes: Camden, Kensington and Chelsea, Islington, Tower Hamlets and Westminster. See Table A1 of the Web Appendix for descriptive statistics on crime levels for the treatment and comparison groups.

FIGURE 2: POLICE HOURS AND TOTAL CRIME (LEVELS) 2004-2005, TREATMENT VERSUS COMPARISON GROUP

(a) Police Hours (per 1000 population)



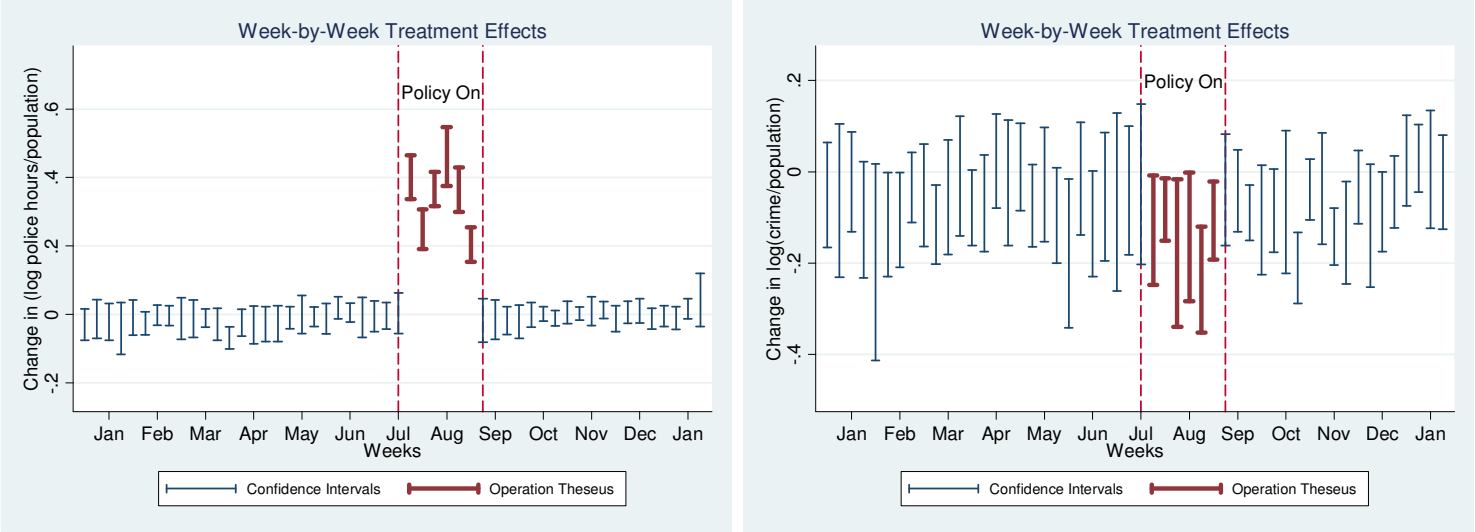
(b) Total Crimes (per 1000 population)



Notes: This Figure plots levels of police and crime for the treatment and comparison groups. Horizontal axis covers the period from January2004 – January 2006. The values of police and crime have been normalised relative to the values in the first week of January 2004. Treatment and Comparison groups defined as per Figure 1.

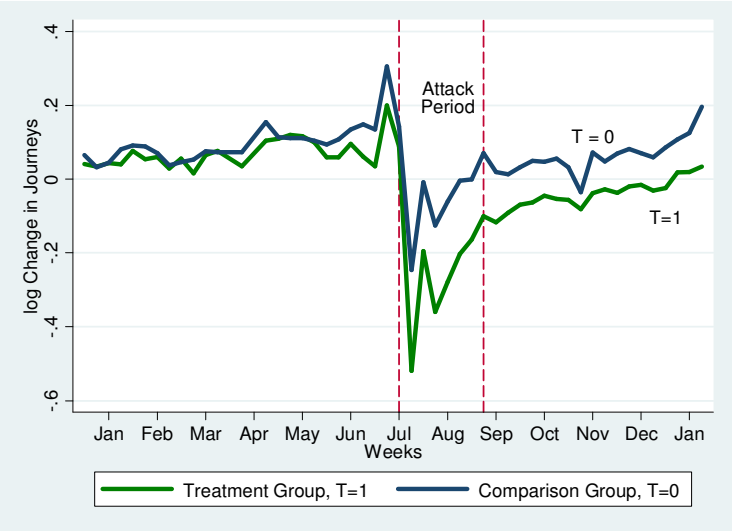
FIGURE 3:
WEEK-BY-WEEK PLACEBO POLICY EFFECTS-POLICE HOURS AND SUSCEPTIBLE CRIMES

(a) Year-on-Year Change in Police Hours (per 1000 population) (b) Year-on-Year Change in Susceptible Crime Rate



Notes: This Figure plots the coefficients and confidence intervals for week-by-week treatment*week interactions from January 2005-January 2006. These are estimated following the reduced form specifications in the main body of the paper. Standard errors clustered by borough. Note that since this is year-on-year, seasonally differenced data it reflects an underlying sample extending from January 2004-January 2006.

FIGURE 4:
YEAR-ON-YEAR CHANGES IN NUMBER OF TUBE JOURNEYS, JANUARY 2004-JANUARY 2006.



Notes: Horizontal axis covers the period from January 2005-January 2006. Note that since this is year-on-year, seasonally differenced data it reflects an underlying sample extending from January 2004-January 2006. The vertical axis measures the year-on-year log change in tube journeys. Tube journeys per station are measured as the sum of station entry and exit (i.e. inward and outward journeys) as recorded at station gates. Journeys per station are then aggregated to the borough and treatment/comparison group level for this graph. Data provided by Transport for London (TfL).

TABLE 1:
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	-0.02
Differences-in-Differences (Levels)			70.65*** (5.28)			-0.42*** (0.11)
Differences-in-Differences (Logs)			0.34*** (0.03)			-0.11*** (0.03)

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. Standard errors are in parentheses.

TABLE 2:
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. Standard errors clustered by borough in parentheses. Boroughs weighted by population. Post-period for baseline models (1) and (5) defined as all weeks after 7/7/2005 until 31/12/2005 attack inclusive. Weeks defined in a Thursday-Wednesday interval throughout to ensure a clean pre and post split in the attack weeks. T*Post-Attack is then defined as interaction of treatment group with a dummy variable for the post-period. T*Post-Attack1 is defined as interaction of treatment group with a deployment "policy" dummy for weeks 1-6 following the July 7th 2005 attack. T*Post-Attack2 is defined as treatment group interaction for all weeks subsequent to the main Operation Theseus deployment. Treatment group defined as boroughs of Westminster, Camden, Islington, Tower Hamlets and Kensington-Chelsea. Police deployment defined as total weekly hours worked by all police staff at borough-level. Controls based on Quarterly Labour Force Survey (QLFS) data and include: borough unemployment rate, employment rate, males under 25 as proportion of population, and whites as proportion of population (following QLFS ethnic definitions).

TABLE 3:
TREATMENT EFFECTS BY MAJOR CATEGORY OF CRIMES

	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
	ROBBERY, BURGLARY AND CRIMINAL DAMAGE					
Crime 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 2. Treatment group also defined as per Table 2. See Table A6 in the Web Appendix for definitions of the Major Crime categories in terms of the constituent Minor Crimes. Crime categories used follow the definitions provided by the Metropolitan Police Service (MPS).

TABLE 4:
SUSCEPTIBLE CRIME VERSUS NON-SUSCEPTIBLE CRIMES, 2004-2005.

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

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. Standard errors clustered by borough 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 2. Controls also defined as per Table 2.

TABLE 5:
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)
	Baseline	Add Controls	Baseline	Add Controls
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
	(1)	(2)	(3)	(4)
	Weekdays	Weekends	Weekdays	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, 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 6:
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 week fixed effects. Standard errors clustered by borough in parentheses. Boroughs weighted by population. These models estimate similar models to Table 4 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 4.

Web Appendix:
'Panic on the Streets of London: Police, Crime and the July 2005 Terror Attacks' by Mirko Draca, Stephen Machin and Robert Witt

TABLE A1:
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 A2:
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 A3:
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)

Year on Year Changes	(1) July/August 2001 – July/August 2002	(2) July/August 2002 – July/August 2003	(3) July/August 2003 – July/August 2004	(4) July/August 2004 – July/August 2005
Treatment boroughs (T=1)	0.030	-0.059	-0.056	-0.097
Control boroughs (T=0)	0.071	-0.021	-0.026	0.007
(T=1) – (T=0) 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)

Year on Year Changes	(1) July/August 2001 – July/August 2002	(2) July/August 2002 – July/August 2003	(3) July/August 2003 – July/August 2004	(4) July/August 2004 – July/August 2005
Treatment boroughs (T=1)	-0.025	-0.120	-0.120	-0.054
Control boroughs (T=0)	0.001	-0.065	-0.065	-0.005
(T=1) – (T=0) 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 4 of the main paper.

TABLE A4:
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 A5:
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 A6:
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

Notes: All crimes occurring in the 32 boroughs of London between 1st January 2004 and 31st December 2005. Proportions calculated as share of the total crimes for the relevant Major Crime category. Both the Minor and Major Crime categories reported are those defined and used by police.