Volume Title: The Economics of Crime: Lessons for and from Latin America

Volume Author/Editor: Rafael Di Tella, Sebastian Edwards, and Ernesto Schargrodsky, editors

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-15374-6 (cloth); 0-226-79185-8 (paper)
ISBN13: 978-0-226-15374-2 (cloth); 978-0-226-79185-2 (paper)

Volume URL: http://www.nber.org/books/dite09-1

Conference Date: November 29-30, 2007

Publication Date: July 2010

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

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Chapter URL: http://www.nber.org/chapters/c11849

Chapter pages in book: (359 - 374)

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

Mirko Draca, Stephen Machin, and Robert Witt

10.1 Introduction

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

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

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We would like to thank Trevor Adams, Jay Gohil, Paul Leppard, and Carol McDonald at the Metropolitan Police and Gerry Weston at Transport for London for assistance with the data used in this study. Part of this chapter uses results from Draca, Machin, and Witt (2008) and extends the empirical analysis in a number of directions. Any errors are our own.

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

differentiated by time and location, thereby creating the possibility of temporal or spatial displacement in criminal activity.

The issue of displacement has been considered much more in criminology than economics. Braga (2001) provides a review of five studies in experimental criminology that focused on potential spatial displacement effects across a diverse set of crime reduction programs. These studies encompassed drug, gun, and general crime interventions and used research designs similar to those employed by empirical economists. There was minimal evidence of spatial displacement across a number of potential outcome variables (including actual crimes committed and service call-outs to police). Overall, these findings are in line with the conclusions of previous surveys of the criminology literature on displacement such as Hessling (1994) and Sherman and Weisburd (1995).

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

Braga (2001) provides a review of the "hot spot" policing strategies covered in the criminology literature that are relevant to the issue of crime displacement. They discuss risk-focused policing strategies, that is, attempts to target particular high-crime areas with additional police resources. These resources included actions such as tailored "problem-oriented" policing responses, patrol programs, and actions based on crackdowns or raids. The nine main studies they focus on cover the cities of Minneapolis, Jersey City (United States), St Louis, Kansas City, Houston, and Beenleigh (in Brisbane, Australia). Among these, five studies consider possible displacement effects, typically by looking at crime in closely adjacent areas (including the block-level). However, none of these studies were able to uncover systematic displacement effects; for example, while one St. Louis drug market study did find displacement effects in one location, it found no effects in two other areas.

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

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

The rest of the chapter is structured as follows. In section 10.2, we discuss the issue of crime modeling with respect to the direct and indirect effects of increased police presence and the big increase in police deployment in London induced by Operation Theseus. Section 10.3 presents our empirical models of the direct and indirect effects on crime following the increased police presence. Section 10.4 concludes.

10.2 Crime, Police, and Displacement

In this section, we provide a more detailed discussion of crime displacement and give a short overview of the policy intervention at the center of our analysis. Our paper on the crime and police relation before and after the July

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

2005 terror attacks (Draca, Machin, and Witt 2008) discusses the estimation and interpretation of the direct effects of this intervention in much more detail and formally analyzes the intervention as a quasi-experiment.

10.2.1 Crime and Police

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

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

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

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

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

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

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

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

This estimating equation is useful for characterizing both the direct and indirect crime effects of an increased in police presence, P_{jt}.

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

2. Indirect effects: The indirect effects are more complex because they rely on displacement of some kind in response to an increased police presence. We consider two possibilities. The first is spatial displacement. As will be made clear in the following, we identify the impact of police on crime

by considering what happened to crime in areas where a sizable increase in police presence occurred as compared to areas where this did not happen. If criminals choose to relocate their criminal activities from the first to the second set of areas, then spatial displacement will occur. The second possibility is temporal displacement. In this case, criminals will still engage in crime in the same areas but will shift their activities to a different time period when the increased police presence does not occur. Thus, temporal displacement will occur if this dynamic notion of criminal behavior applies.

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

10.2.2 Operation Theseus and the July 2005 Terror Attacks

In practical terms, the δ parameter is estimated by a difference-in-difference strategy centered on a group of London boroughs treated by a heavy police deployment. This deployment occurred in the six weeks following the terrorist attack of July 7, 2005. This attack involved the detonation of three bombs on London Underground train carriages near the tube stations of Russell Square (in the borough of Camden), Liverpool Street (in Tower Hamlets), and Edgware Road (in Kensington and Chelsea). A fourth bomb was detonated on a bus in Tavistock Square, Bloomsbury (in Camden). A second wave of attacks occurred two weeks later on July 21, 2005, 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 July 29, 2005.

In response to these attacks, the London Metropolitan Police intensified their police patrols and greatly increased their public presence at transport nodes (particularly Tube stations) and other sites of public importance.

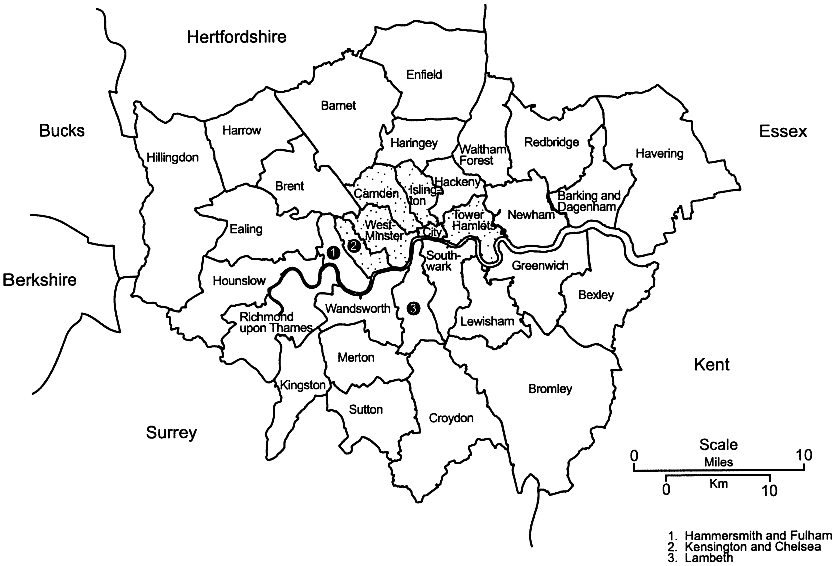

Fig. 10.1 A map of London boroughs

This extra deployment was achieved in various ways, including extending police overtime for approximately six weeks. Furthermore, the deployment was concentrated in the five boroughs of Westminster, Camden, Kensington and Chelsea, Tower Hamlets, and Islington (see figure 10.1 for a map). This deployment involved a 34 percent increase in police hours worked in these boroughs relative to the same period in the previous year.

Figure 10.2 plots police hours worked for this group of treated boroughs against all other London boroughs (the comparison group used in Draca, Machin, and Witt 2008). Also, in table 10.1, we report the changes in pre- and postpolicy levels of police and crime for different groups of boroughs. The striking thing to note from table 10.1 is the composition of the relative change in police hours for the treatment group. That is, police hours increased for the treatment group by 37.6 percent in year-on-year terms but stayed roughly constant across all of the remaining comparison boroughs. Furthermore, even when we break up the comparison group into smaller sets of boroughs (which is what we do to consider the possibility of spatial crime displacement), it is clear that the comparison boroughs did not suffer an absolute fall in police resources during Operation Theseus. This was made possible first by the increase in overtime hours worked across the Metropolitan Police and second by a reallocation of resources across boroughs. Specifically, extra hours worked in the comparison group boroughs were committed to a "central aid" policy, where officers assisted in the security operation underway in the treated boroughs.

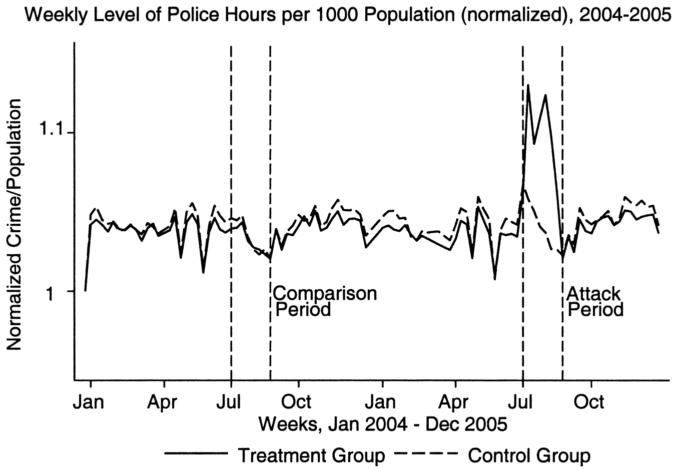

Fig. 10.2 Police deployment, year-on-year changes 2004–2005, treatment versus comparison group

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

10.2.3 Data

Our main data comprises daily crime reports from the London Metropolitan Police Service (LMPS) covering the years 2004 to 2005. The daily reports are given at the ward level (641 wards across London) and aggregated to borough level at the weekly frequency. There are thirty-two London boroughs as shown on the map in figure 10.1.[3]

These boroughs correspond to the organizational units used by the LMPS, known as Borough Operational Command Units (BOCUs), apart from the case of Heathrow Airport, which represents a separate BOCU. Our final weekly panel, therefore, covers thirty-two London boroughs over two years, giving 3,328 observations. We use borough-level population estimates supplied by the Office of National Statistics (ONS) online database to normalize our counts of crime.

The police deployment data are reported only at the borough level and were provided under agreement with the LMPS. The underlying data source

3. Note that the City of London has its own police force, and so this small area is excluded from our analysis.

Table 10.1 Operation Theseus—year-on-year changes in police and crime

	Police deployment (hours worked per 1,000 population)			Crime rate (susceptible crimes per 1,000 population)		
	Pre-Period (1)	Post-Attack1 (2)	Difference (logs) (3)	Pre-Period (4)	Post-Attack1 (5)	Difference (logs) (6)
Treatment group						
Treatment group (5)[a]	169.46	242.29	0.376	3.30	2.89	–0.134
			(0.029)			(0.030)
Comparison groups						
All comparison (27)	82.77	84.95	0.030	1.41	1.40	–0.004
			(0.013)			(0.015)
Inner (8)	113.71	116.26	0.023	1.85	1.84	–0.008
			(0.015)			(0.029)
Adjacent (7)	111.28	112.83	0.014	1.80	1.78	–0.003
			(0.015)			(0.029)
Central (10)	121.86	123.75	0.015	1.99	1.93	–0.023
			(0.026)			(0.045)
Outer (19)	69.79	71.81	0.033	1.22	1.22	–0.002
			(0.018)			(0.019)
Difference-in-Difference			0.346			–0.129
(Treatment–All comparison)			(0.028)			(0.031)

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

[a]Number of boroughs.

used to construct the data is known as CARM (Computer Aided Resource Management), the police service's human resource management system. The CARM database records hours worked by individual officers on a daily basis, and we aggregate this to the weekly, borough level. Furthermore, CARM also contains useful information on the allocation of hours worked by incident or police operation. Although hours worked are available according to officer rank, our main hours measure is based on total hours worked by all officers in the borough. We do, however, adjust for the reallocation effect, whereby officers were deployed into the treatment group during Operation Theseus in order to support security operations. As a supplement to this data, we also use daily data on tube journeys for all stations across the London boroughs. Again, this data (provided by Transport for London [TfL]) was aggregated up to the weekly, borough level. Finally, we also use data from the United Kingdom. Labour Force Survey (LFS) to provide information on area demographics and local labor market trends.

10.3 Empirical Models and Results

10.3.1 Estimating Direct and Indirect Effects

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

$$(4)\quad p_{bt} - p_{b(t-52)} = \alpha_1 + \beta_1 \text{POST}_t + \delta_1(\text{POST}_t \bullet \text{T}_b) + \lambda_1[x_{bt} - x_{b(t-52)}] + [u_{1bt} - u_{1b(t-52)}]$$

$$(5)\quad c_{bt} - c_{b(t-52)} = \alpha_2 + \beta_2 \text{POST}_t + \delta_2(\text{POST}_t \bullet \text{T}_b) + \lambda_2[x_{bt} - x_{b(t-52)}] + [u_{2bt} - u_{2b(t-52)}],$$

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

The analogous structural equation for these reduced forms is

$$(6)\quad c_{bt} - c_{b(t-52)} = \alpha_3 + \beta_3 \text{POST}_t + \delta_3[p_{bt} - p_{b(t-52)}] + \lambda_3[x_{bt} - x_{b(t-52)}] + [u_{3bt} - u_{3b(t-52)}].$$

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

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

$$(7) \qquad \Delta c_{b52} = \alpha_4 + \beta_4 \mathrm{POST}_t + \delta_4(\mathrm{POST}_t \bullet T_b) + \theta_{\mathrm{SD}}(\mathrm{POST}_t \bullet \mathrm{SD}_b) + \lambda_4[x_{bt} - x_{b(t-52)}] + [u_{4bt} - u_{4b(t-52)}],$$

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

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

$$(8) \qquad \Delta c_{b52} = \alpha_5 + \beta_5 \mathrm{POST}_t + \delta_5(\mathrm{POST}_t \bullet T_b) + \theta_{\mathrm{TD}}(\mathrm{TD}_t \bullet T_b) + \lambda_5[x_{bt} - x_{b(t-52)}] + [u_{5bt} - u_{5b(t-52)}],$$

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

10.3.2 Descriptive Statistics

Table 10.1 shows some descriptive statistics on crime and police before and after the terror attacks. It shows a sharp rise in police deployment in the treatment groups in the six weeks following the first round of terror attacks (a 38 percent increase in hours worked per 1,000 population, rising from 169.46 to 242.29). In the full comparison group of twenty-seven boroughs, there was barely any change (going from 82.77 to 84.95). At the same time, crime fell significantly in the five treatment boroughs (by around 13 percent), while there was no change in the comparison boroughs. Thus, crime fell significantly in the treatment group relative to the control group (by 12.9 percent in the difference-in-difference given in the final row of the table).

For exploring possible spatial displacement effects, the table also shows what happened to crime and police for three groups of possible pseudo-treatment boroughs—a group for all of Inner London (as per the definition given by the Office of National Statistics, labelled "Inner"); a group of all those boroughs bordering the treatment group ("Adjacent"); and the five boroughs closest to the treatment group ("Central"). These are the different definitions of SD_b from equation (7) that we consider in our empirical analysis. The unconditional preperiod and postperiod statistics for these different groups are given in table 10.1. As we have already noted, in terms of police hours, there was very little change for any of our proposed pseudo-treatment boroughs. Similarly, these unconditional statistics do not show any evidence of the increase in crime that would be expected if the police intervention was displacing criminal activity from the treatment group into nearby boroughs.

10.3.3 Estimates of the Direct Impacts of Police on Crime

We report the main reduced form OLS and structural IV results corresponding to the direct effect of police on crime in table 10.2. We specify two T • Post-Attack terms that correspond first to the six-week policy-on period after July 7, 2005, and then second to the remaining postpolicy period in

Table 10.2 **Direct crime effects of Operation Theseus**

	Reduced forms			
	Police (1)	Crime (2)	OLS (3)	IV (4)
T • Post-Attack1	0.342***	–0.131***		
	(0.028)	(0.030)		
T • Post-Attack2	0.001	–0.035		
	(0.010)	(0.030)		
ln(Police Deployment)			–0.031	0.382***
			(0.050)	(0.089)
Controls	Yes	Yes	Yes	Yes
No. of boroughs	32	32	32	32
No. of observations	1,664	1,664	1,664	1,664

Source: See Draca, Machin, and Witt (2008) for a more detailed set of results.

Notes: All specifications include week-fixed effects. Clustered standard errors in parentheses. Boroughs weighted by population. Weeks defined in a Thursday–Wednesday interval throughout to ensure a clean pre- and postsplit in the attack weeks. T • Post-Attack is defined as interaction of treatment group with a dummy variable for the postperiod. T • Post-Attack1 is defined as interaction of treatment group with a deployment "policy" dummy for weeks one–six following the July 7, 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 and 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 twenty-five as proportion of population, and whites as proportion of population (following QLFS ethnic definitions). OLS = ordinary least squares; IV = instrumental variable.

***Significant at the 1 percent level.

the second half of 2005. The second term is, therefore, useful for detecting any persistent effects of the police deployment. The first two columns show the reduced form for police deployment and crime. The key term here is T • Post-Attack1, which represents the treatment • policy-on interaction for Operation Theseus. The coefficients here show that police deployment increased by 34.1 percent and crime fell by 13.1 percent in the treatment group during the policy-on period. Furthermore, there is no evidence of a persistent effect of the deployment (i.e., the T • Post-Attack2 coefficient is statistically indistinguishable from zero) for either crime or police.

The final two columns of table 10.2 show the results for the OLS and IV specifications that correspond to these reduced forms. Note that the OLS specification in column (3) is specified in differences, yielding a negative insignificant coefficient. This reflects the fact that, outside of our Operation Theseus policy window, there is minimal time series variation in police on a year-on-year basis. As reported in Draca, Machin, and Witt (2008), the levels version of this model shows a significant, positive coefficient (with an estimate 0.73 [0.053]). The final column in table 10.2 gives the IV estimate. This elasticity indicates that a 10 percent increase in police reduces crime by approximately 3.8 percent.

10.3.4 Estimates of the Indirect Impact of Crime—Spatial Displacement

The results using different control groups to explore possible spatial displacement are reported in table 10.3. The possible displacement effects for the six-week period during Operation Theseus are reported in first row as Area • Post-Attack1, where Area is a dummy variable for each our pseudo-treatment SD_b definitions. In the second row, we interact the Area dummy with a time dummy for all the weeks after Operation Theseus from late August until the end of 2005. This is done to test for potential long-term persistence effects. Similarly, the direct effects of the intervention are given in the rows labeled T • Post-Attack1 and T • Post-Attack2. It is clear from these conditional estimates that there are no significant, positive displacement effects—in fact, the coefficients are estimated to be slightly negative.

10.3.5 Estimates of the Indirect Impact of Crime—Temporal Displacement

The fact that the timing of the police increases and crime falls go hand in hand is in line with the idea that temporal displacement did not occur. In table 10.2, there is a sharp rise in police deployment in the six weeks after the first round of attacks (as shown by the significant positive coefficient on T • Post-Attack1), which then falls back to preattack levels for the rest of 2005 (as shown by the insignificant coefficient on T • Post-Attack1). The same is true of crime where the estimated coefficient on T • Post-Attack1 is significant and negative, yet the estimated coefficient on T • Post-Attack2 is insignificantly different from zero.

Table 10.3 **Spatial displacement effects of Operation Theseus**

	Inner (1)	Adjacent (2)	Central (3)
Area • Post-Attack1	–0.007	–0.001	–0.027
	(0.033)	(0.032)	(0.043)
Area • Post-Attack2	0.011	–0.007	–0.029
	(0.024)	(0.021)	(0.021)
T • Post-Attack1	–0.133	–0.132	–0.138
	(0.032)	(0.032)	(0.031)
T • Post-Attack2	–0.031	–0.037	–0.041
	(0.031)	(0.031)	(0.030)
Controls	Yes	Yes	Yes
No. of boroughs	32	32	32
No. of observations	1,664	1,664	1,664

Notes: Standard errors clustered by borough in parentheses. All regressions include week-fixed effects. Boroughs weighted by population. Weeks defined in a Thursday–Wednesday interval throughout to ensure a clean pre- and postsplit in the attack weeks. *T* • Post-Attack is defined as interaction of treatment group with a dummy variable for the postperiod. *T* • Post-Attack1 is defined as interaction of treatment group with a deployment "policy" dummy for weeks one–six following the July 7, 2005 attack. *T* • Post-Attack2 is defined as treatment group interaction for all weeks subsequent to the main Operation Theseus deployment. Area • Post-Attack1 and Area • Post-Attack2 are dummies for the pseudo-treatment boroughs interacted with the Post-Attack time dummies. Definitions of these areas are given in the notes to table 10.1. Controls based on Quarterly Labour Force Survey (QLFS) data and include borough unemployment rate, employment rate, males under twenty-five as proportion of population, and whites as proportion of population (following QLFS ethnic definitions).

The common timing of police increases and crime falls thus seems inconsistent with temporal displacement by criminals. This is considered in more detail in Table 10.4. In this table, we estimate the treatment group and week interaction for every week for the eight weeks following the end of Operation Theseus. This is effectively a placebo test for potential "policy" effects outside of the six-week period of the intervention. Following the intertemporal displacement hypothesis, a postpolicy rebound or "smoothing" response by criminals would be evident in the weeks after Operation Theseus. However, there is no evidence that crime increased significantly in year-on-year terms for the weeks after the policy was switched off. In fact, the coefficients are consistently negative, probably reflecting the slight downward trend for crime in the treatment group. This is in contrast to the direct effects during the policy-on period, which are significant for each week of the intervention.

10.3.6 Comparison of Estimated Effects With Those in the Literature

Therefore, it seems that our estimated direct effects are not contaminated by spatial or temporal displacement. Moreover, the magnitudes of our casual estimates are similar to the small number of causal estimates found

Table 10.4 Temporal displacement effects in the post Operation Theseus period

Weeks after Operation Theseus	Coefficient	Standard error
+1	–0.040	0.061
+2	–0.041	0.045
+3	–0.090	0.030
+4	–0.106	0.060
+5	–0.085	0.045
+6	–0.067	0.078
+7	–0.211	0.039
+8	–0.039	0.033

Notes: Controls and boroughs included follow those for table 10.2. This table reports the results for $T \cdot$ Week placebo policies for the eight weeks following the end of Operation Theseus. The full set of $T \cdot$ week coefficients for all weeks are shown graphically in Draca, Machin, and Witt (2008).

in the literature. They are also estimated much more precisely in statistical terms because of the very sharp discontinuity in police deployment that occurred.

Table 10.5 reports estimates from the other causal studies we know of. For example, Levitt's (1997) study found elasticities in the –0.43 to –0.50 range, while Corman and Mocan (2000) estimated an average elasticity of –0.45 across different types of offences. The papers based upon terror attacks (Di Tella and Schargrodsky [2004] and Klick and Tabarrok 2005) report elasticities in this range. Our results are certainly qualitatively similar, with our preferred result being –0.38. This coincidence of estimates in very different contexts is strongly supportive of the external validity of these studies.

10.4 Conclusions

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

As this last comment implies, our results do not rule out the possibility that displacement effects may have had a role at a more disaggregated level.

Table 10.5 **Instrumental variable (IV) estimates of the police-crime relationship**

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

Our tests for spatial displacement are effectively tests for between-borough displacement. We are unable to test for within-borough displacement arising from the allocation of police inside the treatment group boroughs. For example, less heavily treated parts of the treatment boroughs may experienced increases in crime relative to more heavily treated areas. However, as we point out in Draca, Machin, and Witt (2008), this would lead to a downward bias on our estimates of the direct effects of the intervention.

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Comment Catherine Rodriguez

The objective of the chapter “Crime Displacement and Police Interventions: Evidence from London’s ‘Operation Theseus’” is to present evidence on the casual impact of police presence on crime rates and investigate its indirect effects through potential crime displacement. Using weekly data of crime and police force in London for the period between January 1, 2004, to December 31, 2005, the authors find an elasticity of crime with respect to police of approximately –0.3. In contrast with this clear direct effect, the authors do not find any evidence of significant spatial or intertemporal displacement in crime during or after the intervention took place.

Without a doubt, the question that the authors are interested in is of extreme importance. Previous work, such as Levitt (1997), Di Tella and Schargrodsky (2004), and Klick and Taborrak (2005), have established the direct effect of police force in crime reduction. However, as the authors men-

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