

Do Police Reduce Crime? Estimates Using the Allocation of Police Forces After a Terrorist Attack

By RAFAEL DI TELLA AND ERNESTO SCHARGRODSKY*

An important challenge in the crime literature is to isolate causal effects of police on crime. Following a terrorist attack on the main Jewish center in Buenos Aires, Argentina, in July 1994, all Jewish institutions received police protection. Thus, this hideous event induced a geographical allocation of police forces that can be presumed exogenous in a crime regression. Using data on the location of car thefts before and after the attack, we find a large deterrent effect of observable police on crime. The effect is local, with no appreciable impact outside the narrow area in which the police are deployed. (JEL K42)

Classical criminology assumes that criminals are rational beings who weigh the costs and benefits of their actions. Gary Becker (1968) produced the first fully fledged theory of crime based on rational behavior. His research led to an upsurge of interest in the economics of criminal behavior [see, for example, Isaac Ehrlich (1973), Ann Witte (1980), Ehrlich and George Brower (1987), James Andreoni (1991), Richard Freeman (1996), Steven Levitt (1997), Pablo Fajnzylber et al. (2000), inter alia]. One of the central predictions of Becker's theory is that crime will decrease when police presence increases. A basic problem with this prediction is that it has largely failed to find empirical support. In a survey of the literature, Samuel Cameron (1988) reports that in 18 out of 22

papers surveyed researchers found either a positive effect of police presence on crime or no relationship between these variables. More recent surveys by Thomas Marvell and Carlisle Moody (1996) and John Eck and Edward Maguire (2000) reach similar conclusions.

There is, however, a serious endogeneity problem with these studies that arises from the simultaneous determination of crime and police presence (see Franklin Fisher and Daniel Nagin, 1978). It is likely that the government of a city in which the crime rate increases will hire more police officers. Areas beset by high crime will thus end up with more police officers than areas with low crime rates, introducing a positive bias in the police coefficient in a crime regression. A central challenge in the crime literature has been to break this endogeneity in order to identify causal effects of police on crime.

Two recent papers use a time-series strategy to address this problem. Using data for the United States, Marvell and Moody (1996) find Granger-causation between crime and police running in both directions. In a similar vein, Hope Corman and H. Naci Mocan (2000) exploit high-frequency data for New York City to show that increases in the number of police officers cause a reduction in one out of five crime categories (specifically, burglary). Monthly data are used because hiring and training delays in the response of the police authority to an increase in crime will mitigate simultaneity bias present in low-frequency data. In order to

* Di Tella: Harvard Business School, Boston, MA 02163 (e-mail: rditella@hbs.edu); Schargrotsky: Universidad Torcuato Di Tella, Miñones 2177, (C1428ATG) Buenos Aires, Argentina (e-mail: eschargr@utdt.edu). We thank a co-editor, an extremely constructive referee, Jushan Bai, Sebastian Galiani, Erzo Luttmer, Robert MacCulloch, Sam Peltzman, Andrea Rotnitzky, several key informants, and seminar participants at the 2001 AEA New Orleans meetings, the University of California–Berkeley, Stanford University, Econometric Society, LACEA, UTDT, UdeSA, Getulio Vargas, AAEP, and UNLP for helpful suggestions. The second author thanks SCID at Stanford University for their hospitality. Matias Cattaneo, Luciana Esquerro, and Magali Junowicz provided excellent research assistance. The database and computer programs used in this paper are available at <http://www.people.hbs.edu/rditella> and www.utdt.edu/~eschargr.

validly address the simultaneity concern, these identification strategies depend, crucially, on the assumption that the authorities are unable to forecast crime-fighting needs.¹

Levitt (1997) develops a different approach using instrumental variables to break simultaneity. He documents the presence of an electoral cycle in police hiring and uses the timing of gubernatorial and mayoral elections to instrument for police presence in a panel of 59 large U.S. cities from 1970–1992. Using two-stage least-squares (2SLS) techniques, Levitt finds a negative and significant effect of police on violent crime. The pattern across individual crime categories is surprising, with murder exhibiting the largest (and the only significant) coefficient, and with very imprecise estimates for the categories in which the rational model is presumed to be more relevant (e.g., property crimes). Still, the validity of the instrument might be questioned. The timing of elections may affect crime by way of channels other than the number of police officers on the street. Levitt avoids some of these concerns by controlling for the unemployment rate and public spending, although police effort and crime reporting (as well as police hiring) may also respond to the timing of elections, particularly if the police are the target of political manipulation. Similarly, the behavior of judges and prosecutors may be affected by elections, something that could logically reduce criminal activity during such times.²

A more severe concern raised by Justin McCrary (2002) is that Levitt's 2SLS estimates suffer from a computational error (see also Levitt's reply, 2002). When the mistake is corrected the replication results show no effect of police on crime at standard significance levels. The

state of the evidence leads Levitt (2002) to wonder: "If electoral cycles can provide no more than suggestive evidence of a causal impact of police on crime, are there other identification strategies that can do better?"

In this paper we present a different approach to estimate the causal effect of police on crime. On July 18, 1994 terrorists exploded a bomb that destroyed the Asociacion Mutual Israelita Argentina (A.M.I.A.), the main Jewish center in Argentina. Eighty-five people died and more than 300 were wounded in the attack. One week later the federal government assigned police protection to every Jewish and Muslim building in the country. Because the geographical distribution of these institutions can be presumed to be exogenous in a crime regression, this hideous event constitutes a natural experiment whereby the simultaneous determination of crime and police presence can be broken.³

We collected information on the number of motor vehicle thefts per block in three neighborhoods in Buenos Aires before and after the terrorist attack. The information covers the nine-month period beginning April 1 and ending December 31, 1994. We also collected information on the location of each Jewish institution in these neighborhoods. We then estimated the effect of police presence on car theft. Our difference-in-differences estimates show that blocks that receive police protection experience significantly fewer car thefts than the rest of the neighborhoods. The effect is large. Relative to the control group, car thefts fall by 75 percent in the blocks in which the protected institutions are situated. However, the effect is extremely local. We find no evidence that police presence in a given block reduces car theft one or two blocks away from the protected buildings.

There has been considerable interest in identifying the mechanisms by which police presence reduces crime. Is it that police presence makes criminal activity less attractive (deterrence), or is it that police officers apprehend criminals leaving fewer of them around to com-

¹ Criminologists often emphasize the benefits of anticipating crime patterns. David Bayley (1998), for example, states "The key assumption behind smarter law enforcement is that crime is not evenly scattered through time and space. Police are not faced with meeting all crime threats everywhere all the time. Instead, each form of crime displays a particular pattern which, if understood, provides opportunities for law enforcement" (Bayley, 1998, p. 174). On the allocation of police resources to protect high crime areas, often called "hot spots," see Lawrence Sherman et al. (1989) and Sherman and David Weisburd (1995).

² On the incentives faced by members of the judiciary see, for example, Richard Posner (1993).

³ On natural and randomized experiments, see the discussions in Robert LaLonde (1986), Joshua Angrist (1990), Angrist and Alan Krueger (1991), Daniel Hamermesh (1999), and Bruce Sacerdote (2001).

mit crimes (incapacitation)? Being based on changes in crime levels in particular locations (i.e., the protected blocks) our results are unlikely to reflect changes in the numbers of incarcerated criminals, which should affect all neighborhood blocks, not just those containing Jewish institutions.⁴ Thus, we believe that our estimates are most appropriately interpreted as the causal deterrent effect of police staffing on car theft. However, it is still possible that car thefts were displaced in a way that we are unable to measure, in which case the effect of policing may be smaller than our estimates suggest.

The rest of the paper is organized as follows. In Section I we describe our data. In Section II we discuss the empirical strategy and present our results. Section III concludes.

I. Data Description

On July 18, 1994 a terrorist attack destroyed the main Jewish center (A.M.I.A.) in Buenos Aires, Argentina.⁵ Seven days later, on July 25, the federal government decided to provide 24-hour police protection to more than 270 Jewish and Muslim institutions (including synagogues, mosques, clubs, cemeteries, and schools) in Argentina. Muslim institutions were protected for fear of potential retaliations after the Islamic organization, Hezbollah, claimed responsibility for the attack. Nearly ten years after the attack this protection is still provided.

A significant proportion of the protected buildings are Jewish institutions within Buenos

Aires proper.⁶ Although providing this surveillance required the distraction of a nonnegligible proportion of the police forces protecting the areas in which these buildings are located, the police forces made a serious effort to maintain previous levels of police presence in the rest of these neighborhoods. Government officials worried that compromising police protection throughout the neighborhoods might generate in the residents ill feelings towards the Jewish community.⁷ Because the personnel commitment could not be met with the normal number of police assigned to these neighborhoods, the increased police presence was achieved with officers reassigned from administrative tasks at the Central Police Department, the Communications Division, and the Mounted Police.⁸

The data analyzed in this paper are from three noncontiguous, Buenos Aires neighborhoods that collectively represent about 3.2 percent of the city's area and account for 6.9 percent of its population. One police station is located in each neighborhood.⁹ The neighborhoods were selected on the basis of three criteria: they were the areas with the largest numbers of Jewish institutions in the city;¹⁰ significant portions of the neighborhoods were not close to a protected institution (more than 50 percent of blocks are more than two blocks removed from a protected

⁶ Approximately 85 percent of the Jewish population of the country lives in Buenos Aires and its suburbs.

⁷ Institutional information for this paper was gathered through a series of interviews with key informants, including the Secretary of Security (third level of authority in the federal government, behind the president and ministers), the Chief of the Federal Police, and the Minister of the Interior during the period under consideration as well as a former federal judge, a former federal prosecutor, and the director of a nongovernmental organization devoted to protecting civil rights.

⁸ For example, more than one-third of approximately 200 police officers stationed in Once, one of the neighborhoods with the highest density of Jewish institutions, had to be reassigned to protection duties. The personnel necessary to maintain the previous level of police presence in the rest of the neighborhood was pulled from outside of this police station.

⁹ There are 53 police stations in Buenos Aires. Adrian Pelacchi (2000) provides an in-depth discussion of the institutional features of crime and the police force in Argentina.

¹⁰ There are no Muslim institutions in the neighborhoods considered in our study.

⁴ Daniel Kessler and Levitt (1999) use California's sentence enhancement laws for a selected group of crimes to distinguish between incapacitation and deterrence. See also Levitt (1998). Articles studying responses to increases in detection probabilities include Avner Bar-Ilan and Sacerdote (2001) on red light violations, and Robert McCormick and Robert Tollison (1984), on fouls committed by basketball players.

⁵ This was the second terrorist attack in the city of Buenos Aires. The Israeli embassy had been destroyed on March 17, 1992. In the months immediately following this first attack, the most prominent Jewish centers, including A.M.I.A., had been given more attention by officers on patrol. But surveillance was not generalized and declined gradually. Information on these attacks can be found in www.atentado-amia.com.ar, www.daia.org.ar, and www.bnaibrith.org.

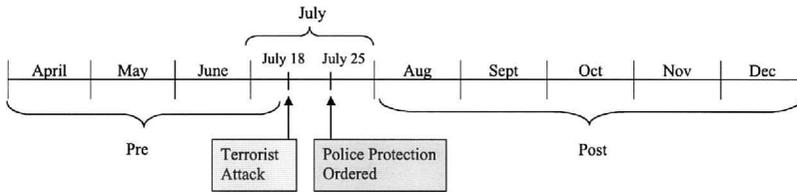


FIGURE 1. TIMELINE OF EVENTS

institution), providing a control group for our study; and three was the maximum number of police stations for which we were able to convince police authorities to provide us data.¹¹ There are a total of 876 blocks in these three neighborhoods. The block constitutes the unit of observation for our study.¹²

We obtained all the information available to the police (with the exception of the victim's name) about each auto theft in these neighborhoods for the nine-month period starting April 1, 1994 and ending December 31, 1994. Figure 1 presents a timeline of the events in our study. April 1 to July 17 constitutes the period before the terrorist attack. The interim period of July 18 to July 31 includes a first week during which surveillance had not yet been introduced and a second week during which police began to implement the protection policy. By the end of the last week of July police protection was fully functioning and known to the public. Finally, August 1 to December 31 covers the period of police protection.

Although victims' tendency to underreport often results in official records underestimating crime levels, this is a minor problem for car thefts in Buenos Aires for two reasons. First, police intervention is required to activate car insurance against theft, a type of insurance carried by most car owners in Buenos Aires (89 percent according to the official victimization

survey, Ministerio de Justicia, 2000). Second, because criminals often use stolen cars in the commission of other crimes, victims who report car thefts to police forestall confusion about their involvement in such crimes. The victimization survey cited above reports that 87 percent of Buenos Aires car thefts are reported to the police, compared to only 29 percent for all types of crime. A further advantage of auto theft data is that this category of crime is expected to be more sensitive to police presence.¹³ Most robberies occur after a brief period of surveillance of the intended victim. Criminals concentrating their attention on mobile victims might miss the presence of police. A parked car, on the other hand, gives criminals time to gather information on areas in which they intend to commit crimes.

Car theft information obtained from the police includes the address at which the stolen vehicle was parked, make and year of the vehicle, day and time of the report, and whether the robbery was violent. During the period of analysis 794 nonarmed car thefts were reported in these neighborhoods.¹⁴ Although they normally occur in the middle of blocks, car thefts in many cases are reported at corners so as to facilitate victims' verbal descriptions of crime locations at the time they file police reports. We assigned one-quarter of each car theft reported at a corner to each of the intersection's four blocks.¹⁵

¹¹ The police stations' daily records, which register auto thefts on the same pages as reports of every other type of crime or incident, are not available to the public. The Chief of the Federal Police had to issue a special authorization instructing police station personnel to transcribe the data for us.

¹² We consider a block as the segment of a street between two corners. With few exceptions, Buenos Aires is a perfect grid city, with streets crossing perpendicularly at corners. Each block is about 100 meters (110 yards) long.

¹³ Ninety-four percent of Buenos Aires car robberies occur in the street (Ministerio de Justicia, 2000).

¹⁴ We exclude a small number (63) of armed robberies reported during this period as well as 86 misreports that correspond to nonexistent or incomplete addresses or to car thefts that occurred outside of our sample neighborhoods (i.e., that were reported to the wrong police station).

¹⁵ This procedure assigns some fractions of thefts to blocks outside the boundaries of the neighborhoods under study, which reduces the total number of car thefts from 794 to 778.75.

TABLE 1—DEMOGRAPHIC CHARACTERISTICS OF CONTROL AND TREATMENT AREAS

Demographic characteristics	Census tracts without Jewish institutions (A)	Census tracts with Jewish institutions (B)	Difference (C) = (A) - (B)
Home ownership rate	0.696 (0.008)	0.663 (0.017)	0.032 (0.019)
Overcrowding rate	0.014 (0.001)	0.017 (0.002)	-0.002 (0.003)
Poverty rate	0.042 (0.003)	0.052 (0.008)	-0.010 (0.009)
Education of household head	11.653 (0.147)	11.052 (0.300)	0.600 (0.335)
Number of household members	2.719 (0.023)	2.685 (0.054)	0.034 (0.059)
Female population	0.556 (0.001)	0.552 (0.003)	0.003 (0.003)
Unemployment rate	0.053 (0.001)	0.059 (0.003)	-0.005 (0.003)
Age	38.005 (0.128)	37.690 (0.223)	0.315 (0.258)
Number of census tracts	53	14	

Notes: Columns (A) and (B) present the mean of each variable for census tracts without and with Jewish institutions in our sample. Column (C) presents the differences of means. Standard deviations are in parentheses. Home ownership rate is the percentage of owner-occupied houses. Overcrowding rate is the percentage of households with more than three people per room. Poverty rate is the percentage of households with at least one unmet basic need (overcrowding; four or more members per working member and household head with low educational attainment; poor quality housing; school-age children not attending school; or no fecal evacuation system). Education of the household head is the average educational attainment of the household head in number of years. Female population is the percentage of women in the total population. Unemployment rate is the rate of unemployment for the population of age 14 or higher. Age is the average age of the population.

Source: 1991 Population Census.

The completed data set included information on the geography of these neighborhoods, in particular, the precise location of each Jewish institution. There are 45 protected institutions in this part of the city. Thirty-seven of them are within these neighborhoods, while the rest are near the boundaries (but less than three blocks away).¹⁶ The geographical distribution of blocks, institutions, and car thefts is summarized in Table A1 in the Appendix.

Using information from the 1991 census, Table 1 compares socioeconomic characteristics potentially related to crime victimization and car ownership across areas without and with Jewish institutions. The lowest level of aggre-

gation for which census information is available in Buenos Aires is census tracts (*fracciones censales*), which cover approximately eight to ten contiguous hectares. Tests of means reveal no statistical differences between census tracts that contain and do not contain Jewish institutions along the following dimensions: home ownership rate, percentage of overcrowded households, percentage of poor households, number of household members, percentage of women, employment rate, and age. The only dimension along which these census tracts differed was years of education of the household head: 11.65 and 11.05, respectively, for tracts without and with Jewish institutions. We interpret these results as evidence that the surveillance policy was randomly assigned across socioeconomic characteristics. Table A2 in the Appendix compares demographics and car theft

¹⁶ None of the protected institutions in our sample is located at a corner.

TABLE 2—MONTHLY EVOLUTION OF CAR THEFT

Month	More than two blocks from nearest Jewish institution (A)	Jewish institution on the block (B)	One block from nearest Jewish institution (C)	Two blocks from nearest Jewish institution (D)	Difference (E) = (B) - (A)	Difference (F) = (C) - (A)	Difference (G) = (D) - (A)
April	0.09955 (0.248)	0.12162 (0.361)	0.12111 (0.287)	0.12278 (0.297)	0.02206 (0.060)	0.02156 (0.025)	0.02323 (0.022)
May	0.10840 (0.235)	0.08783 (0.205)	0.07763 (0.181)	0.09734 (0.259)	-0.02056 (0.035)	-0.03076 (0.018)	-0.01106 (0.020)
June	0.07853 (0.196)	0.12837 (0.286)	0.07763 (0.215)	0.06969 (0.186)	0.04983 (0.047)	-0.00090 (0.019)	-0.00884 (0.015)
July (1-17)	0.03926 (0.145)	0.02027 (0.069)	0.05900 (0.210)	0.03097 (0.141)	-0.01899 (0.013)	0.01973 (0.017)	-0.00829 (0.011)
July (18-31)	0.03926 (0.146)	0.02702 (0.078)	0.07298 (0.217)	0.06858 (0.238)	-0.01224 (0.014)	0.03371 (0.018)	0.02931 (0.017)
August	0.11836 (0.287)	0.04729 (0.175)	0.06677 (0.219)	0.12721 (0.304)	-0.07106 (0.031)	-0.05159 (0.024)	0.00884 (0.024)
September	0.10176 (0.256)	0.01351 (0.057)	0.09006 (0.276)	0.09845 (0.248)	-0.08825 (0.015)	-0.01170 (0.024)	-0.00331 (0.020)
October	0.12112 (0.267)	0.06081 (0.215)	0.09782 (0.260)	0.08849 (0.236)	-0.06031 (0.037)	-0.02330 (0.024)	-0.03263 (0.020)
November	0.09623 (0.240)	0.02702 (0.078)	0.11024 (0.288)	0.10176 (0.217)	-0.06921 (0.017)	0.01400 (0.025)	0.00553 (0.018)
December	0.10176 (0.268)	0.02702 (0.078)	0.11645 (0.278)	0.10619 (0.225)	-0.07474 (0.018)	0.01468 (0.025)	0.00442 (0.019)
Number of blocks	452	37	161	226			

Notes: The first four columns present the mean and standard deviation (in parentheses) of the number of car thefts for each type of block per month. The average number of car thefts for July can be obtained by summing the subperiods. The last three columns present the differences of means of columns (B), (C), and (D) relative to column (A), with standard deviations in parentheses.

rates for the neighborhoods under study relative to the whole city.

A key dimension in our empirical exercise is the distance of each block in our sample to the nearest Jewish institution, whether or not the building is within our neighborhoods. We distinguish among blocks that contain a Jewish institution, blocks that are contiguous in any direction to a block containing a Jewish institution, and blocks that are two blocks away in any direction from a block containing a Jewish institution. We then compare these with blocks that are more than two blocks away from a block containing a Jewish institution.

Table 2 presents means (and standard deviations) of auto thefts for each month for each type of block. The bottom row tallies the number of blocks of each type. For the month of July we consider, separately, the period before and after the terrorist attack. For the post-July

period, the table shows that, relative to the control group (i.e., blocks more than two blocks away from the nearest Jewish institution), blocks occupied by a Jewish institution experienced a lower level of car theft. A similar reduction is not observed for blocks that are one or two blocks away from the nearest Jewish institution. In particular, differences of means indicate that average car theft in blocks with protected institutions is significantly less than average car theft for the control group for every month after July, with the exception of October. Although casual inspection of the data for blocks that contain a Jewish institution also suggests a decline for the first days of July (before the attack), the difference with the control group is not statistically significant for this period. Indeed, for every period prior to the terrorist attack we cannot reject that the car theft mean for the blocks with Jewish

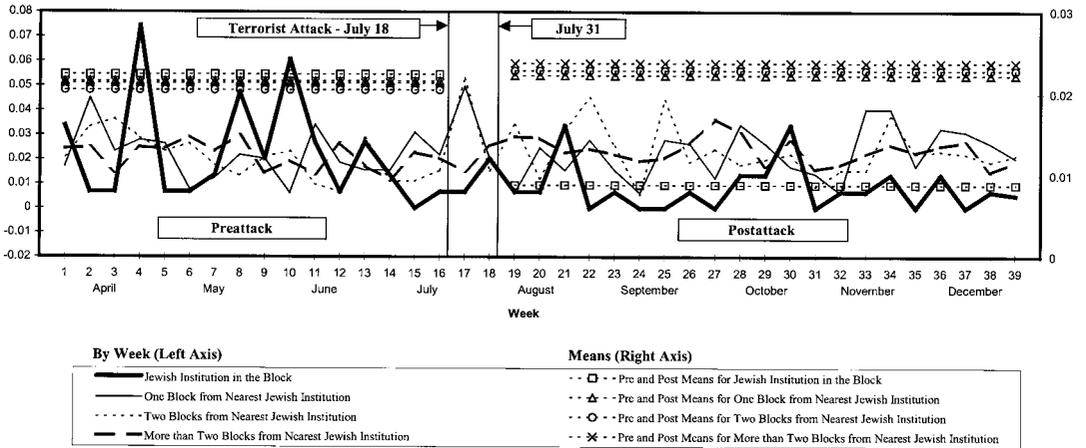


FIGURE 2. WEEKLY EVOLUTION OF CAR THEFTS

Notes: Per-week average of car thefts for blocks that contain a Jewish institution (37 blocks), blocks that are one block away from the nearest Jewish institution (161), blocks that are two blocks away from the nearest Jewish institution (226), and blocks that are more than two blocks away from the nearest Jewish institution (452). The horizontal lines are pre- and postattack averages (excluding car thefts that occurred between July 18 and July 31).

institutions is equal to the mean for the control group.

Figure 2 plots the same information at a more disaggregated level, namely, by week. The series (left axis) are obviously more volatile for the aggregates that average a smaller number of blocks (see the bottom row of Table 2). The horizontal lines (right axis) represent the pre- and postattack averages for the weekly data for each block type. Prior to the attack there are no discernible differences in these averages across the different types of blocks. After the attack, however, average car thefts for blocks that contain Jewish institutions evolve around a lower mean. Instead, car theft levels for the other types of blocks show a slight increase over time.

II. The Effect of Police on Car Theft

A. Empirical Strategy

Our purpose is to identify the causal effect of police presence on car thefts. Using the total number of car thefts per block during each month from April to December as the dependent variable gives us a panel with nine observations

for each block.¹⁷ We exclude car thefts that occurred between July 18 and July 31.¹⁸ Having data on blocks with and without protected institutions allows us to define a treatment and a control group. We include month fixed effects that control for any aggregate shocks in the evolution of crime, and block fixed effects that control for time-invariant influences. Controlling for time and individual effects, we obtain the difference-in-differences estimators of the effect of police on crime using the following model:

$$\begin{aligned}
 Car\ Theft_{it} = & \alpha_0 Same\ Block\ Police_{it} \\
 & + \alpha_1 One\ Block\ Police_{it} \\
 & + \alpha_2 Two\ Blocks\ Police_{it} \\
 & + M_i + F_i + \varepsilon_{it},
 \end{aligned}$$

¹⁷ Of course, our monthly level of aggregation is arbitrary. Similar results obtain when we aggregate the data, for example, at the weekly level. All results reported but not presented are available upon request.

¹⁸ Including the period between July 25 and July 31 does not affect our results.

where:

Car Theft_{it} is the number of car thefts in block *i* for month *t*;

Same-Block Police_{it} is a dummy variable that equals 1 for the months after the terrorist attack (August, September, October, November, and December) if there is a protected institution in the block, 0 otherwise;

One-Block Police_{it} is a dummy variable that equals 1 after the terrorist attack (August, September, October, November, and December) if the block is one block away from the nearest protected institution, 0 otherwise;

Two-Blocks Police_{it} is a dummy variable that equals 1 after the terrorist attack (August, September, October, November, and December) if the block is two blocks away from the nearest protected institution, 0 otherwise;

M_t is a month fixed effect;

F_i is a block fixed effect;

ε_{it} is the error term.¹⁹

The key aspect of our empirical exercise is that the geographical allocation of police forces induced by the surveillance policy is exogenous to the distribution of crime. Officers are placed in those blocks to protect a potential terrorist target, not in response to levels of common crime. Thus, the terrorist attack provides a natural experiment that breaks the simultaneous determination of crime and police presence.

Note that we estimate the effect of police presence on crime absent direct data on the distribution of police forces. In our study the postattack distribution of the protected institutions stands in for the presence of police forces. Indeed, our paper could be construed to be an instrumental variables application. If a portion of the police force is endogenously allocated to fight common crime, whereas another portion is exogenously deployed to protect Jewish buildings, we could use the surveillance policy as an instrument for police presence. A two-stage exercise, however, would require detailed data on

the distribution of police forces per block at any given time, information that is confidential. Although we can calculate the reduced-form regression of the outcome on the instrument, it is not feasible to estimate the instrumented two-stage regression.

A natural question with respect to our empirical approach is the extent to which police officers deployed to protect Jewish and Muslim institutions are effective anticrime agents. Although policemen in this role have limited scope for pursuing suspected criminals outside their assigned areas, they can nevertheless interfere with crimes committed near their posts and communicate the presence of suspicious-looking individuals to policemen patrolling the neighborhood. Moreover, criminals probably expect the police to intervene. From a more practical point of view, there is anecdotal evidence of arrests made by policemen on duty guarding these institutions.²⁰

B. Basic Estimates

Table 3 reports our basic regression results. Column (A) uses only the shortest measure of proximity to police presence, *Same-Block Police*, a dummy that takes the value 1 for every month after the attack for every block in which there is a Jewish institution. This regression considers all the other blocks in the sample as our control group. The coefficient on *Same-Block Police* is negative and significant.

Regression (B) includes a broader measure of distance to a protected institution, *One-Block Police*. This variable captures the effect of police presence on the six blocks that are contiguous in any direction to each block occupied by a guarded institution. The effect of *Same-Block Police* is negative, significant, and marginally larger in absolute size than the one reported in column (A). The effect of *One-Block Police* (the effect on immediately neighboring blocks)

¹⁹ Standard errors are Huber-White (i.e., heteroskedasticity-consistent) unless specified.

²⁰ See, for example, *La Nacion*, September 11, 1999, which reports the conviction of a car thief apprehended in March 1997 by an officer protecting a Jewish school in Belgrano. Similar events were reported in Villa Luro and Once (where a police officer protecting a Jewish institution was stabbed in a struggle with a thief who was robbing a nearby grocery store; the burglar was later arrested).

TABLE 3—THE EFFECT OF POLICE PRESENCE ON CAR THEFT

	Difference-in-difference			Cross section	Time series
	(A)	(B)	(C)	(D)	(E)
<i>Same-Block Police</i>	-0.07752*** (0.022)	-0.08007*** (0.022)	-0.08080*** (0.022)	-0.07271*** (0.011)	-0.05843*** (0.022)
<i>One-Block Police</i>		-0.01325 (0.013)	-0.01398 (0.014)	-0.01158 (0.010)	-0.00004 (0.013)
<i>Two-Blocks Police</i>			-0.00218 (0.012)	-0.00342 (0.009)	0.01701 (0.010)
Block fixed effect	Yes	Yes	Yes	No	Yes
Month fixed effect	Yes	Yes	Yes	Yes	No
Number of observations	7,884	7,884	7,884	4,380	3,816
R ²	0.1983	0.1984	0.1984	0.0036	0.1891

Notes: Dependent variable: number of car thefts per month per block. Least-squares dummy variables (LSDV) regressions. Car thefts that occurred between July 18 and July 31 are excluded. Column (D) excludes observations for the preattack period (April through July). Column (E) excludes observations for the blocks that are more than two blocks away from the nearest protected institution. Huber-White standard errors are in parentheses.

*** Significant at the 1-percent level.

is not significant. It indicates that blocks one block removed from a protected institution do not experience significantly fewer car thefts than the rest of the neighborhood.

Regression (C) includes a third measure of proximity to a guarded institution, *Two-Blocks Police*. This measure takes the value 1 during the postattack period for all the blocks that are two blocks away from the nearest Jewish institution. The significance of *Same-Block Police* remains below the 1-percent level, whereas the coefficients on *One-Block Police* and *Two-Blocks Police* are not significant. For blocks within the immediate radius of the protected institutions (one or two blocks removed) car thefts are not fewer than for the control group (i.e., the rest of the neighborhood).²¹ We later introduced additional treatment dummies (*Three-Blocks Police*, *Four-Blocks Police*, and so forth), but their coefficients were not significantly different from zero.

Our results suggest that the introduction of fixed and observable police presence generated a significant decline in car thefts in the protected blocks but no effect one or two blocks away relative to the rest of the neighborhoods. For the

rest of the paper we focus on this specification, which takes as our control group blocks more than two blocks away from the guarded buildings. Our results do not change when we treat blocks one and two blocks removed from the protected institutions as part of the control group (or if we restrict the control group to an even more distant area).

The effect of police presence in the same block is quite large in economic terms. The difference-in-difference coefficient of column (C) indicates a reduction of 0.081 car thefts per month in the blocks that received direct police protection. The average number of car thefts per month per block from August through December for all blocks more than two blocks away from a protected institution is 0.108. Relative to this average, car theft declines by 75 percent in blocks in which a protected institution is situated.

The estimated drop in crime in protected blocks can be approximated into an elasticity of crime with respect to police presence. The estimated percentage change in car theft is -75 percent. To calculate the percentage change in police presence, we note, for example, that in the Once neighborhood approximately 200 officers were deployed prior to the attack. Assuming that Buenos Aires policemen work eight-hour shifts and an average of 21 days per month, there are approximately 47 officers on

²¹ Similar results are obtained if we include a set of dummy variables that measure the distance in blocks to the Jewish institutions rather than the block fixed effects. None of these dummies is significant.

patrol at any given time. Inasmuch as Once comprises 153 blocks, the average presence of officers per block is 0.31. Roughly approximated, the change in police presence is $(1 - 0.31)/0.31 = 2.23$, for an approximate elasticity of car theft with respect to police of -0.33 . It might also be appropriate to reduce the number of policemen present at any given time by the proportion of patrol officers relative to officers performing other duties (e.g., administrative or investigative tasks). Bayley suggests 60 percent as a reasonable number for this proportion in the United States (see Chapter 2 in Bayley, 1998). Using these estimates the elasticity of car theft with respect to police is -0.17 .

This number is smaller than the police elasticity of motor vehicle theft reported for the United States in previous work (see, for example, Levitt, 1997; McCrary, 2002). Comparisons with previous studies, however, must allow for the fact that they use changes in crime at the city level and cannot distinguish between deterrence effects of extra police and incapacitation effects. The empirical strategy employed in our paper, premised on policemen standing guard, suggests that estimated effects are exclusively deterrence effects. The effects of incarceration and subsequent reduction of the criminal population (i.e., incapacitation) should be observed for all blocks, not just for those occupied by protected Jewish institutions.

Before proceeding it is useful to consider two alternative estimators. The similarity of demographic characteristics and preintervention car theft rates across areas with and without Jewish institutions suggests that it is worth considering a simple cross-section estimator. This is presented in column (D) of Table 3 where we only consider the observations for the postattack period and exclude the block fixed effects. The coefficient on *Same-Block Police* is -0.073 . We cannot reject the hypothesis that the *Same-Block Police* coefficients are equal for the cross-section and the difference-in-difference specification.²² A second alternative, suggested

by the approximate similarity in car theft rates before and after the attack for the control group, is to focus exclusively on the time-series variation. Column (E) compares the pre- and postattack car theft rates excluding the observations in the control group and the month fixed effects. The coefficient is -0.058 , somewhat smaller than the result in column (C). The difference is due to the small upward trend in the control group, although we still cannot reject equality with the difference-in-difference coefficient.²³ We interpret the similarity between the cross-section, the time-series, and the difference-in-difference estimators as informative of the robustness of our research design.

C. Robustness

In this subsection we present further tests to help assess the validity of our results. For example, a simple potential objection is that, because the protection policy also imposed parking restrictions right in front of some of the protected institutions, the policy might have led mechanically to a depressed number of victims. To address this issue, we measured the forbidden parking space in front of each institution. This area represents, on average, 11 percent of the total parking space for protected blocks.²⁴ Under a linear relationship this factor could explain a 0.012 reduction in the number of car thefts (11 percent of 0.108, the average number of car thefts for the control group). We reject at the 1-percent significance level that our estimated coefficient equals this value.

column (D) are estimated collapsing the data for each block (so that there are only 876 observations corresponding to the postattack per-block averages).

²³ In the absence of month fixed effects, the dependent variable is normalized into number of car thefts per month of equal (30-day) length. We cannot reject equality of the coefficient on *Same-Block Police* for the difference-in-difference and the time-series specification when the standard errors in column (E) are estimated collapsing the data for each block (so that there are only 848 observations corresponding to the pre- and postattack per-block averages for the 424 noncontrol blocks).

²⁴ This figure represents an upper limit of the parking restrictions generated by the terrorist attack inasmuch as parking in front of some of the institutions was already restricted before the attack.

²² It can be argued that pooling observations, without individual effects, in column (D) leads to underestimated standard errors. We cannot reject equality of the coefficient on *Same-Block Police* for the difference-in-difference and the cross-section specification when the standard errors in

A related issue is that drivers may have preferred to avoid parking near the protected institutions for fear of another terrorist attack. To address this concern, we estimated separate police coefficients for each month after the terrorist attack. If fear of another terrorist attack was preventing neighbors from parking in these blocks, we should expect the effect to diminish over time.²⁵ Yet, our coefficients on *Same-Block Police* remain constant over all the months in the postattack period. Moreover, both the Israeli embassy and the A.M.I.A. terrorist attacks were focused on the target buildings (surrounding buildings in the block were not destroyed), so the impact of fear of future attacks should be concentrated on parking spaces directly in front of the Jewish institutions.²⁶ Interestingly, for August, the first month after the attack, a negative and significant coefficient is obtained on *One-Block Police*, leading us to speculate that criminals needed time to realize that the police guards were actually restricted to their posts. Instead, for the following months the policemen show no effect in deterring crime one block away.

We also investigate whether blocks close to a Jewish institution exhibit a different crime dynamic than the rest of the neighborhood in the period *before* the terrorist attack. If crime was diminishing in blocks occupied by Jewish institutions before the attack, perhaps we are capturing a spurious correlation. To analyze this issue, in Table 4 we consider a sample that starts on April 1 and ends on July 17. We then reestimate our basic regression redefining our *Same-Block Police*, *One-Block Police*, and *Two-Blocks Police* dummy variables to take the

²⁵ Similarly, it may be argued that after the attack common criminals experienced a bloom of civility that led them to avoid committing crimes in front of buildings of the Jewish community. Again, we would expect such a bloom to fade over time. Reduced crime in New York City immediately after the terrorist attack of September 11, 2001 was attributed to a rise in civility (see, for example, "U.S. Crime Rate Up, Ending Decade of Decline," *Christian Science Monitor*, June 25, 2002).

²⁶ It should also be emphasized that finding a legal parking space in these neighborhoods is often difficult, thus reducing the incidence of this problem. On the severe parking conditions in Buenos Aires, see, for example, *La Nación*, March 5, 2001.

TABLE 4—CAR THEFTS BEFORE THE TERRORIST ATTACK

	Police dummies activated on April 30 (A)	Police dummies activated on May 31 (B)	Police dummies activated on June 30 (C)
<i>Same-Block Police</i>	-0.01864 (0.053)	0.01467 (0.040)	-0.03611 (0.038)
<i>One-Block Police</i>	-0.02553 (0.025)	0.01402 (0.019)	0.02310 (0.022)
<i>Two-Blocks Police</i>	-0.03263 (0.022)	-0.01465 (0.017)	-0.00940 (0.016)
Block fixed effect	Yes	Yes	Yes
Month fixed effect	Yes	Yes	Yes
Number of observations	3,504	3,504	3,504
R ²	0.3206	0.3202	0.3204

Notes: Dependent variable: number of car thefts per month per block. Least-squares dummy variables (LSDV) regressions. Sample period: April 1–July 17. The variable *Same-Block Police* in column (A) equals 1 between April 30 and July 17 (for blocks that contain a Jewish institution) and 0 otherwise. The same is true for *One-Block Police* and *Two-Blocks Police* (for blocks one block away from the nearest Jewish institution and blocks two blocks away from the nearest Jewish institution, respectively). Column (B) redefines these variables using May 31, and column (C) uses June 30. Huber-White standard errors are in parentheses.

value 1 at the end of each month prior to the attack [end of April in column (A), end of May in column (B), and end of June in column (C)]. In this way, we reproduce our exercise as if the terrorist attack had occurred during the pretreatment period. The nonsignificant results in Table 4 validate our exercise in the sense that they reveal no special crime dynamics affecting our treatment group before the terrorist attack.

The question of whether the timing of the change in the evolution of car thefts around Jewish institutions coincides with the date police protection was deployed can be approached as a test for the timing of a structural break in the model presented in Section II, subsection A. We estimate a series of models with treatment dummies defined for every possible breakdate. We then calculate the sum of squared errors for each model. The least-squares breakdate estimate is the date for which the sum of squared errors associated with the model is minimized (Jushan Bai, 1994; Bai, 1997; Bai et al., 1998; Bruce Hansen, 2001). Considering the full

TABLE 5—ROBUSTNESS

	LSDV (A)	LSDV (B)	LSDV (C)	LSDV (D)	LSDV (E)	LSDV (F)	Poisson (G)
<i>Same-Block Police</i>	-0.08080*** (0.023)	-0.08080*** (0.025)	-0.08080*** (0.022)	-0.08216*** (0.025)	-0.08344*** (0.024)	-0.12617*** (0.037)	0.29635** (0.145)
<i>One-Block Police</i>	-0.01398 (0.015)	-0.01398 (0.016)	-0.01398 (0.016)	-0.01475 (0.019)	-0.01658 (0.015)	-0.01789 (0.019)	0.86699 (0.178)
<i>Two-Blocks Police</i>	-0.00218 (0.012)	-0.00218 (0.013)	-0.00218 (0.017)	-0.00002 (0.016)	-0.00243 (0.012)	-0.00394 (0.015)	0.98322 (0.179)
Block fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effect	Yes	Yes	Yes	Yes	No	Yes	Yes
Neighborhood × month fixed effect	No	No	No	No	Yes	No	No
Number of observations	1,752	7,884	7,884	32,412	7,884	5,967	5,967
	$R^2 = 0.6519$	$R^2 = 0.1984$	$R^2 = 0.1984$	$R^2 = 0.0521$	$R^2 = 0.2010$	$R^2 = 0.1616$	$W = 41***$

Notes: In regression (A) the dependent variable is the pre- and postattack average of car thefts per block. In regression (D) the dependent variable is the number of car thefts per week per block transformed into monthly levels. In the rest of the table the dependent variable is the number of car thefts per month per block. Regressions (F) and (G) exclude blocks with no thefts throughout our sample period. All regressions exclude car thefts that occurred between July 18 and July 31. Huber-White standard errors are in parentheses in columns (A), (E), and (F). Standard errors clustered on 876 blocks are in parentheses in column (B). Standard errors clustered on 27 neighborhood-month combinations are in parentheses in column (C). Standard errors clustered on 111 neighborhood-week combinations are in parentheses in column (D). Poisson incidence rate ratios (standard errors in parentheses) are reported in column (G).

*** Significant at the 1-percent level.

** Significant at the 5-percent level.

sample period (all observations from April 1 to December 31, including the July 18 to July 31 interim period), we first perform this exercise redefining the treatment dummies to take the value 1 at the end of each month. The sum of squared errors is minimized by the regression that considers the end of July to be the breakdate. To gain precision, we also perform the exercise at a weekly level of aggregation. In this case, the breakdate estimator corresponds to the end of the fourth week of July.²⁷ Thus, at both frequencies of aggregation the least-squares breakdate estimates coincide with the actual date police protection was deployed to the Jewish institutions.

In the presence of positive serial correlation, a potential concern is that OLS regressions underestimate standard errors. This problem might be exacerbated in difference-in-differences estimates when the treatment is serially correlated, as is the case for the proxy for police presence that we use in our exercise. We employ two solutions (discussed in Marianne Bertrand et al., 2004) to solve this problem for large sample

sizes such as ours. First, we collapse the data for each block into two observations (pre- and post-periods). In column (A) of Table 5, we regress the monthly averages of car thefts per block for the pre- and postattack periods on the treatment variable. The results remain unaltered. The second solution is to allow for an arbitrary covariance structure within blocks over time. This is presented in column (B) of Table 5, which shows that our results are unaffected when standard errors are computed through clustering on blocks.²⁸

Another concern is the possible presence of spatial correlation across blocks of the same neighborhood that could be commonly affected by local shocks. To address this issue, in column (C) of Table 5 the standard errors are calculated through clustering on neighborhood-

²⁸ The variance formula for the calculation of the clustered standard errors is given by $\hat{\mathbf{Z}} = q_c(\mathbf{X}'\mathbf{X})^{-1} \times (\sum_{h=1}^M \mathbf{u}_h' \mathbf{u}_h)(\mathbf{X}'\mathbf{X})^{-1}$; where: $\mathbf{u}_h = \sum_{j \in G_h} \mathbf{u}_j$; G_1, G_2, \dots, G_M are the clusters; M is the number of clusters; $\mathbf{u}_j = (y_j - \mathbf{x}_j \mathbf{b}) \mathbf{x}_j$; and $q_c = \frac{N-1}{N-k} \frac{M}{M-1}$; while $y_j, \mathbf{x}_j, \mathbf{b}, \mathbf{X}, N$ and k

²⁷ In the weekly exercise, the second lowest sum of squared errors is obtained by the regression that considers the breakdate at the end of the first week of July.

follow standard econometric notation (for further details, see STATA, 2001, p. 87). This variance estimator coincides with the Huber-White variance estimator when each cluster contains one observation.

TABLE 6—EXPENSIVE VS. CHEAP, WEEKDAY VS. WEEKEND, AND NIGHT VS. DAY

	Expensive car thefts (A)	Cheap car thefts (B)	Weekday thefts (C)	Weekend thefts (D)	Night thefts (E)	Day thefts (F)
<i>Same-Block Police</i>	-0.02798*** (0.009)	-0.04213** (0.019)	-0.05879*** (0.017)	-0.02201 (0.015)	-0.02922** (0.013)	-0.05157*** (0.018)
<i>One-Block Police</i>	-0.00848 (0.007)	-0.00607 (0.011)	-0.00807 (0.012)	-0.00591 (0.006)	-0.01368 (0.008)	-0.00030 (0.011)
<i>Two-Blocks Police</i>	-0.00763 (0.007)	0.00392 (0.009)	0.00212 (0.010)	-0.00431 (0.006)	-0.00033 (0.006)	-0.00185 (0.010)
Block fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7,884	7,884	7,884	7,884	7,884	7,884
R ²	0.1383	0.1650	0.1629	0.1792	0.1558	0.1737

Notes: Dependent variable: number of car thefts of each type per month per block. Least-squares dummy variables (LSDV) regressions. Car thefts that occurred between July 18 and July 31 are excluded. Expensive cars are those valued above the mean sample value (\$8,403). The sample covers 244.25 expensive car thefts and 446.25 cheap car thefts. The post-July means of expensive and cheap car thefts for the control group are 0.039 and 0.065, respectively. Car model and, thus, value is not available for all reported car thefts. Weekday car thefts are those reported from Monday through Friday. The sample covers 518.25 weekday car thefts and 214.5 weekend car thefts. The post-July means of weekday and weekend car thefts for the control group are 0.078 and 0.030, respectively. Night car thefts are those reported between 10 p.m. and 10 a.m. The sample covers 239.75 night car thefts and 493 day car thefts. The post-July means of night and day car thefts for the control group are 0.035 and 0.073, respectively. Huber-White standard errors are in parentheses.

*** Significant at the 1-percent level.

** Significant at the 5-percent level.

month combinations. This does not affect the significance of our results. The number of clusters, however, could be considered insufficient when we cluster on the 27 neighborhood-month combinations. In column (D) we show that the results do not change when we consider weekly observations (transformed into monthly levels) and cluster on the 111 week-neighborhood combinations. As another strategy to control for the potential presence of local shocks, in column (E) we include neighborhood-month fixed effects rather than our month fixed effects. Our results again remain unaltered.

In column (F) we repeat the analysis, excluding blocks in which no thefts occurred throughout the period of analysis. Introducing police protection in these blocks should have no effect, the number of car thefts already being bounded at zero. As expected, the coefficient is larger (more negative) when we exclude the 213 no-theft blocks (24 percent of our sample), corresponding to a drop in car theft of 89 percent relative to the control group. Finally, a Poisson specification presented in column (G) shows our results to be robust to estimation using

count data models.²⁹ The Poisson incidence rates imply that protected blocks have 70 percent less car theft than the control group.

D. Further Results

Using standard information from the used car market we exploit our data on car make and year to construct an estimate of the value of the stolen cars. We then analyze, in Table 6, the differential deterrent effect of police presence (splitting the sample between cheap and expensive cars). We perform similar analyses for weekday and weekend and night and day thefts. The *Same-Block Police* coefficients show no statistically significant differences in the effect of police presence by car value, day of the week, or time of day (after normalizing the coefficients by the average number of thefts of each type).³⁰

²⁹ Similar results are obtained using a negative binomial model.

³⁰ For example, the *Same-Block Police* coefficient of -0.028 in column (A) corresponds to a reduction in expensive car thefts of 71 percent (the post-July mean of expensive

TABLE 7—OTHER SOURCES OF CRIME PROTECTION

	Bank (A)	Public building (B)	Gas station (C)	All (D)
<i>Same-Block Police</i> × (1 - <i>Protection</i>)	-0.08391*** (0.024)	-0.08498*** (0.024)	-0.08196*** (0.023)	-0.09008*** (0.025)
<i>Same-Block Police</i> × <i>Protection</i>	-0.02641** (0.012)	-0.00766 (0.040)	-0.03891 (0.070)	-0.02141 (0.022)
<i>One-Block Police</i> × (1 - <i>Protection</i>)	-0.01940 (0.014)	-0.01350 (0.014)	-0.01375 (0.014)	-0.01869 (0.015)
<i>One-Block Police</i> × <i>Protection</i>	0.04762 (0.047)	-0.02462 (0.021)	-0.03266 (0.046)	0.01921 (0.032)
<i>Two-Blocks Police</i> × (1 - <i>Protection</i>)	-0.00206 (0.012)	-0.00433 (0.012)	-0.00231 (0.012)	-0.00481 (0.012)
<i>Two-Blocks Police</i> × <i>Protection</i>	-0.00375 (0.043)	0.05640 (0.051)	0.00275 (0.058)	0.01644 (0.030)
<i>F</i> -stat [†]	5.21**	2.80*	0.35	4.31**
Block fixed effect	Yes	Yes	Yes	Yes
Month fixed effect	Yes	Yes	Yes	Yes
Number of observations	7,884	7,884	7,884	7,884
<i>R</i> ²	0.1987	0.1986	0.1985	0.1987

Notes: Dependent variable: number of car thefts per month per block. Least-squares dummy variables (LSDV) regressions. Car thefts that occurred between July 18 and July 31 are excluded. *Protection* equals 1 when a Bank (Column A), a Public Building (Column B), a Gas Station (Column C), or any of these (Column D) is located in the block, 0 otherwise. Huber-White standard errors are in parentheses.

* Significant at the 10-percent level.

** Significant at the 5-percent level.

*** Significant at the 1-percent level.

[†] Null hypotheses: *Same-Block Police* × (1 - *Protection*) = *Same-Block Police* × *Protection*.

For some of the blocks to which police were assigned postattack some source of crime protection was already in place. In Table 7 we compare the effect of the additional police protection for blocks occupied by a bank, public building (including foreign embassies), gas station, or any of these buildings relative to other guarded blocks not previously provided this security. The first two cases (bank and public building) indicate some police protection already in the block. The latter case (gas station) implies significant light and movement during the entire day. As expected, the coefficient on additional police presence is always smaller when previous sources of crime protection were already present in the block. The effect of police

presence for previously protected blocks is significantly different from zero only in blocks that have a bank [see column (A)], where police are present only during office hours and often inside the building.

E. Discussion

The focus of our paper is to estimate the deterrent effect of police on car theft and to explore the internal validity of our estimates. It is also worth discussing briefly the public finance question (i.e., whether treatment benefits outweigh costs) and the external validity of our results (i.e., whether our estimates can be used to predict the effect of police on car theft in other settings). Although a full evaluation of these issues is not possible, some of the information that we have available is worth considering.

A starting point for the cost-benefit analysis is the direct cost of police surveillance relative to car values. During this period, a Buenos

car thefts in the control group is 0.039). The coefficient of -0.042 for cheap car thefts [column (B)] indicates a reduction of 65 percent (the post-July mean of cheap car thefts in the control group is 0.065). The difference is not statistically significant. Note that car model and, thus, value is not available for all reported car thefts.

Aires policeman was earning on average a monthly wage of \$800. Given that policemen work eight-hour shifts and average 21 work days per month, the monthly cost of providing police protection for one block is approximately \$3,428. Our estimates suggest that police presence in a block would induce a reduction of 0.081 of a car theft per month. The average value of the stolen cars in our sample is \$8,403.³¹ Thus, in terms of the reduction in auto theft exclusively, the protection policy was not cost effective. Of course, visible police protection provides other social benefits besides a lower rate of car theft. It deters other types of crime not considered in our study (e.g., burglaries or terrorist attacks) and provides citizens with feelings of greater security. Moreover, incapacitating criminals is likely to substantially benefit society, whereas our estimates capture only deterrence effects. On the other hand, crime reduction in the protected blocks could simply reflect the displacement of criminal activity to other areas of the city as we discuss below.

Another way to evaluate the impact of the policy, relevant for the public finance question, is to determine whether total car thefts in Buenos Aires were affected as much as or less than the reduction in the blocks that contain protected institutions. Unfortunately, reliable monthly data for car thefts citywide in Buenos Aires are unavailable. The annual published crime data show a general upward trend in car thefts and other property crime during the 1990's in the city of Buenos Aires (that is less apparent in the rest of the country). The year 1994 was not an outlier in this process. One should also note that increments in crime levels were concentrated in low-income urban areas (see Di Tella et al., 2002), whereas the neighborhoods considered in our study were middle-to-high income areas. Finally, the protected blocks probably represent too small a share of

the city, meaning that, given the noise in the official statistics and the trends affecting the level and the distribution of crime during the period, it is unclear whether the impact of our natural experiment can be perceived at the aggregate level.

With respect to the external validity of our findings, a number of issues are worth mentioning. First, the standing-policeman technology we analyze is similar to that used for private security in the most affluent neighborhoods of Buenos Aires. In these areas booths for private security guards are placed at each corner approximately 100 meters from one another. If privately provided surveillance covers all parts of the block (i.e., there are no blind spots) and there are no advantages to having a section of the block protected by two security guards (i.e., overlapping protection), then the maximum protection distance is about 50 meters, the distance from the security guard's booth to the house furthest from his surveillance. Interestingly, our results are consistent with the coverage solution reached by the private security market.

Second, our results involving a technology based on police in a fixed location might be relevant in the analysis of other forms of police presence such as officers on patrol. Switching to mobile police is unlikely to induce a different response in car theft given that the act of breaking into a car is of extremely short duration. Because criminals check that there are no policemen watching when they start the process, the likelihood of discovery conditional on no monitoring at the start of the break-in is virtually zero.³² Furthermore, the kind of police

³¹ If some stolen cars are recovered in working condition, then only a fraction of their value should be counted. Official publications put the proportion of cars stolen in Buenos Aires that are never recovered at 60 percent (Ministerio de Justicia, 2000). One should also count, however, the amount of time consumed by the recovery process and the cost of any needed repairs.

³² Our key informants have emphasized that the probability of a policeman on patrol actually witnessing a crime being committed is quite low. The most likely case is that someone who has witnessed a crime calls the police. *The Economist* (February 24, 2001) makes a similar argument: "But putting more police on the beat will probably not have much impact on crime figures. A single patrolling officer typically covers an area containing 18,000 inhabitants, 7,500 houses, 140 miles of pavements, 85 acres of parks, 77 miles of roads, 23 pubs and 10 schools. The chance of that officer actually catching an offender red-handed is extremely small. A Home Office study estimates that a patrolling policeman in London might expect to pass within 100 yards of a burglary in progress once every eight years, and even on that occasion is very unlikely to realize that a crime is taking place, let alone catch the burglar."

presence we analyze is visible. An alternative, to deploy police agents in plain clothes, would make breaking into a car more risky for criminals, but it could make reporting crimes to nearby police more difficult for victims and bystanders. Interestingly, policy makers' tendency to address public pressure for more protection from crime by deploying more visible police on the streets could imply counterproductive police reallocations.³³ Given the highly local impact of street deployments, it would be interesting to obtain estimates of the effect of extra policemen on investigative tasks.

Finally, an important issue in establishing the external validity of our findings is the potential presence of crime displacement to other areas.³⁴ The simplest form of this hypothesis involves displacement to the immediately adjacent blocks. If all the criminals that used to operate in the protected blocks moved to the contiguous blocks, one would expect an increase in crime in adjacent blocks of $-(-0.08/4.35) = 0.018$, where 4.35 is the average sample number of blocks one block away from each protected block ($4.35 = 161/37$). We reject the complete adjacent displacement hypothesis at conventional levels of significance. However, under some specifications we found evidence consistent with the presence of displacement to blocks

that are located two blocks away from the protected institutions (for example, using a binary Logit model that treats equally any positive number of thefts). More generally, since we do not know how criminals select the blocks in which they steal cars, we know little about where they move their activities when they are deterred from stealing in a specific location. It is indeed possible that the car theft deterred in the protected blocks was displaced to other areas of the city (inside or outside the neighborhoods in our sample), although our study does not have the statistical power to learn about the magnitude of these displacement effects.

III. Conclusions

A crucial challenge in the literature on crime is to obtain an estimate of the effect of increased police presence. In this paper, we have tackled this question exploiting a natural experiment. On July 18, 1994 a terrorist cell exploded a bomb that destroyed the main Jewish center in the city of Buenos Aires, killing 85 people and wounding more than 300. Following the attack a police officer was stationed in front of each Jewish and Muslim institution in the country. Because the distribution of these institutions can be presumed to be exogenous in a crime regression, it is possible to use this hideous event to break the simultaneous determination of crime and police presence.

We collected data on the precise locations of car thefts in three neighborhoods in Buenos Aires before and after the attack. We find a large, negative, and highly local effect of police presence on car theft. Blocks that receive police protection experience 0.081 fewer car thefts per month than blocks that do not. The postattack average number of car thefts per block for our control group is 0.108, so police protection induces a decline in auto theft of approximately 75 percent. Blocks one or two blocks away from where protection is provided, however, do not experience fewer car thefts relative to the rest of the neighborhoods.

The robustness of our empirical strategy is illustrated by the fact that we reach similar conclusions using a cross-section, a time-series, or a difference-in-difference approach. The results are also robust to alternative specifications

³³ A similar point is raised by a former policeman in an open letter to *The Washington Post*: "The other alternative was to 'get officers from behind desks' to walk foot patrols, allegedly at no additional cost. But while this second method seemed more palatable, it had hidden costs that undermined the police department's ability to solve and prevent crimes. Most of the so-called desk officers were performing important investigative and support functions that happen to occur out of public sight. In each staffing cycle in which desk officers were put on the street, the police department's ability to solve crimes such as murder, robbery, rape and assault was diminished." ("Citizen Police for the City," *The Washington Post*, September 10, 2000.) The literature on crime distinguishes between two different forms of law enforcement: monitoring and investigation (see, for example, Dilip Mookherjee and Ivan Png, 1992).

³⁴ Derek Cornish and Ronald Clarke (1987) and Rene Hesselting (1994) survey the criminology literature on displacement. Ian Ayres and Levitt (1998), John Lott (1998), and Mark Duggan (2001) study the effect of introducing *unobservable* protection devices (Lojack and concealed handguns) with potentially positive externalities. Similarly, in our study, *observable* police presence might induce negative externalities in neighboring areas.

and do not seem to be generated by spurious correlations associated with different crime dynamics for the treatment and control groups. The empirical strategy employed in our paper, premised on policemen standing guard, suggests that the estimated effects correspond exclusively to deterrence effects. A limitation of

our approach, however, is that it does not allow us to provide a precise estimate of the extent of crime displacement to other areas.

Our results, in brief, suggest that a posted and visible police guard exerts a large, negative, local effect on auto theft and little or no effect outside a narrow area.

APPENDIX

TABLE A1—DISTRIBUTION OF BLOCKS, INSTITUTIONS, AND CAR THEFTS BY NEIGHBORHOOD

	Belgrano	Villa Crespo	Once	Total
Blocks	463	260	153	876
Institutions	9	14	22	45
Inside	7	13	17	37
In boundaries	2	1	5	8
Car thefts	530	191	73	794
April 1–July 17	197	73	24	294
July 18–July 31	30	9	7	46
August 1–December 31	303	109	42	454

TABLE A2—NEIGHBORHOOD AND CITY CHARACTERISTICS

	Belgrano	Villa Crespo	Once	City of Buenos Aires
Home ownership rate	0.731	0.670	0.593	0.694
Overcrowding rate	0.009	0.022	0.026	0.018
Poverty rate	0.026	0.058	0.078	0.070
Education of household head	12.338	10.278	10.516	10.482
Number of household members	2.768	2.763	2.506	2.805
Female population	0.561	0.543	0.550	0.546
Unemployment rate	0.051	0.061	0.055	0.059
Age	38.166	36.874	38.373	38.022
Car theft rate	478.703	298.315	167.952	219.231

Notes: Home ownership rate is the percentage of owner-occupied houses. Overcrowding rate is the percentage of households with more than three people per room. Poverty rate is the percentage of households with at least one unmet basic need (overcrowding; four or more members per working member and household head with low educational attainment; poor quality housing; school-age children not attending school; or no fecal evacuation system). Education of the household head is the average number of years of education of the household head. Female population is the percentage of women in the total population. Unemployment rate is the rate of unemployment for the population of age 14 or higher. Age is the average age of the population. The data source for the demographic characteristics is the 1991 Population Census. Car theft rate is the annual rate per 100,000 inhabitants for 1994. For the three neighborhoods, the April through December data available for this study are extrapolated into annual rates. The data source for car theft rates are the Justice Ministry for the whole city, and the Federal Police for the three neighborhoods.

REFERENCES

- Andreoni, James.** "Reasonable Doubt and the Optimal Magnitude of Fines: Should the Penalty Fit the Crime?" *RAND Journal of Economics*, Autumn 1991, 22(3), pp. 385–95.
- Angrist, Joshua.** "Lifetime Earnings and the Vietnam Era Draft Lottery: Evidence from Social Security Administrative Records." *American Economic Review*, June 1990, 80(3), pp. 313–36.
- Angrist, Joshua and Krueger, Alan.** "Does Compulsory School Attendance Affect Schooling and Earnings?" *Quarterly Journal of Economics*, November 1991, 106(4), pp. 979–1014.
- Ayres, Ian and Levitt, Steven.** "Measuring Positive Externalities from Unobservable Victim Precaution: An Empirical Analysis of Lojack." *Quarterly Journal of Economics*, February 1998, 113(1), pp. 43–77.
- Bai, Jushan.** "Least Squares Estimation of a Shift in Linear Processes." *Journal of Time Series Analysis*, September 1994, 15(5), pp. 453–72.
- _____. "Estimation of a Change Point in Multiple Regression Models." *Review of Economics and Statistics*, November 1997, 79(4), pp. 551–63.
- Bai, Jushan; Lumsdaine, Robin and Stock, James.** "Testing For and Dating Common Breaks in Multivariate Time Series." *Review of Economic Studies*, July 1998, 65(3), pp. 395–432.
- Bar-Ilan, Avner and Sacerdote, Bruce.** "The Response to Fines and Probability of Detection in a Series of Experiments." National Bureau of Economic Research (Cambridge, MA), Working Paper No. 8638, December 2001.
- Bayley, David.** "Introduction to Chapter 2: Patrol," in D. Bayley, ed., *What works in policing*. New York: Oxford University Press, 1998, pp. 26–30.
- Becker, Gary.** "Crime and Punishment: An Economic Approach." *Journal of Political Economy*, March–April 1968, 76(2), pp. 169–217.
- Bertrand, Marianne; Duflo, Esther and Mulainathan, Sendhil.** "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics*, February 2004, 119(1), pp. 249–76.
- Cameron, Samuel.** "The Economics of Crime Deterrence: A Survey of Theory and Evidence." *Kyklos*, May 1988, 41(2), pp. 301–23.
- Corman, Hope and Mocan, H. Naci.** "A Time-Series Analysis of Crime, Deterrence, and Drug Abuse in New York City." *American Economic Review*, June 2000, 90(3), pp. 584–604.
- Cornish, Derek and Clarke, Ronald.** "Understanding Crime Displacement: An Application of Rational Choice Theory." *Criminology*, November 1987, 25(4), pp. 933–47.
- Di Tella, Rafael; Galiani, Sebastian and Scharrotsky, Ernesto.** "Crime Inequality when Victims Adapt." Mimeo, Universidad Torcuato Di Tella, 2002.
- Duggan, Mark.** "More Guns, More Crime." *Journal of Political Economy*, October 2001, 109(5), pp. 1086–1114.
- Eck, John and Maguire, Edward.** "Have Changes in Policing Reduced Violent Crime? An Assessment of the Evidence," in A. Blumstein and J. Wallman, eds., *The crime drop in America*. New York: Cambridge University Press, 2000, pp. 207–65.
- Ehrlich, Isaac.** "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation." *Journal of Political Economy*, May–June 1973, 81(3), pp. 521–65.
- Ehrlich, Isaac and Brower, George.** "On the Issue of Causality in the Economic Model of Crime and Law Enforcement: Some Theoretical Considerations and Experimental Evidence." *American Economic Review*, May 1987 (*Papers and Proceedings*), 77(2), pp. 99–106.
- Fajnzylber, Pablo; Lederman, Daniel and Loayza, Norman.** "Crime and Victimization: An Economic Perspective." *Economia*, Fall 2000, 1(1), pp. 219–302.
- Fisher, Franklin and Nagin, Daniel.** "On the Feasibility of Identifying the Crime Function in a Simultaneous Equations Model of Crime," in A. Blumstein, D. Nagin, and J. Cohen, eds., *Deterrence and incapacitation: Estimating the effects of criminal sanctions on crime rates*. Washington, DC: National Academy of Sciences, 1978, pp. 361–99.
- Freeman, Richard.** "Why Do So Many Young American Men Commit Crimes and What Might We Do About It?" *Journal of Eco-*

- omic Perspectives*, Winter 1996, 10(1), pp. 25–42.
- Hamermesh, Daniel.** “The Art of Labormetrics.” National Bureau of Economic Research (Cambridge, MA), Working Paper No. 6927, February 1999.
- Hansen, Bruce.** “The New Econometrics of Structural Change: Dating Breaks in U.S. Labor Productivity.” *Journal of Economic Perspectives*, Fall 2001, 15(4), pp. 117–28.
- Hesseling, Rene.** “Displacement: A Review of the Empirical Literature,” in R. Clarke, ed., *Crime prevention studies*, III. Monsey, NY: Criminal Justice Press, 1994, pp. 197–230.
- Kessler, Daniel and Levitt, Steven.** “Using Sentence Enhancements to Distinguish between Deterrence and Incapacitation.” *Journal of Law and Economics*, April 1999, 42(1), pp. 343–63.
- LaLonde, Robert.** “Evaluating the Econometric Evaluations of Training Programs with Experimental Data.” *American Economic Review*, September 1986, 76(4), pp. 604–20.
- Levitt, Steven.** “Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime.” *American Economic Review*, June 1997, 87(3), pp. 270–90.
- . “Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation or Measurement Error?” *Economic Inquiry*, July 1998, 36(3), pp. 353–72.
- . “Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: Reply.” *American Economic Review*, September 2002, 92(4), pp. 1244–50.
- Lott, John.** *More guns, less crime*. Chicago: University of Chicago Press, 1998.
- Marvell, Thomas and Moody, Carlisle.** “Specification Problems, Police Levels, and Crime Rates.” *Criminology*, November 1996, 34(4), pp. 609–46.
- McCormick, Robert and Tollison, Robert.** “Crime on the Court.” *Journal of Political Economy*, April 1984, 92(2), pp. 223–35.
- McCrary, Justin.** “Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: Comment.” *American Economic Review*, September 2002, 92(4), pp. 1236–43.
- Ministerio de Justicia.** “Estudio de Victimización en Centros Urbanos de la Argentina.” Mimeo, Ministerio de Justicia de la República Argentina, 2000.
- Mookherjee, Dilip and Png, Ivan.** “Monitoring vis à vis Investigation in the Enforcement of Law.” *American Economic Review*, June 1992, 82(3), pp. 556–65.
- Pelacchi, Adrian.** *Tratado sobre la Seguridad Pública*, 318. Buenos Aires: Editorial Policial, 2000.
- Posner, Richard.** “What Do Judges and Justices Maximize?” *Supreme Court Economic Review*, 1993, 3(1), pp. 1–41.
- Sacerdote, Bruce.** “Peer Effects With Random Assignment: Results for Dartmouth Roommates.” *Quarterly Journal of Economics*, May 2001, 116(2), pp. 681–704.
- Sherman, Lawrence; Gartin, Patrick and Buerger, Michael.** “Hot Spots of Predatory Crime: Routine Activities and the Criminology of Place.” *Criminology*, 1989, 27(1), pp. 27–55.
- Sherman, Lawrence and Weisburd, David.** “General Deterrent Effects of Police Patrol in Crime ‘Hot Spots’: A Randomized, Controlled Trial.” *Justice Quarterly*, December 1995, 12(4), pp. 625–48.
- STATA.** *Stata reference manual. Number 7, vol. 3 Q-St.* College Station, TX: Stata Press, 2001.
- Witte, Ann.** “Estimating the Economic Model of Crime with Individual Data.” *Quarterly Journal of Economics*, February 1980, 94(1), pp. 155–67.