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"Using a Terrorist Attack to Estimate the Effect of Police on Crime"

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Using a Terrorist Attack to Estimate the Effect of Police on Crime

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Abstract

Three important challenges in the crime literature are to isolate significant causal effects of police on crime, to distinguish between deterrence and incapacitation, and to provide some estimate of the amount of displacement induced by visible deterrence activities. Following a terrorist attack on the main Jewish center in the city of Buenos Aires, Argentina, in July 1994, all Jewish and Muslim institutions (including schools, synagogues, mosques and clubs) were given 24-hour police surveillance. Thus, this hideous event induced a geographical allocation of police forces that can be presumed exogenous in a crime regression. Furthermore, induced changes in crime can only reflect deterrence effects. We collected data on the location of all car thefts for three neighborhoods of the city before and after the terrorist attack to study the effect of observable police on crime. Our estimates suggest that there is a large, negative, local effect of police presence. We also find evidence of displacement: there is a positive and significant effect of police presence on the number of car thefts in the immediate surrounding area. The effects approximately cancel out, so there is no overall effect of observable police on crime.

JEL: K42
Keywords: Crime, deterrence, observable police, displacement, natural experiment.

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I. Introduction

Classical criminology assumes that criminals are rational beings who weigh the costs and benefits of their actions. Becker (1968) produced the first fully-fledged theory of crime based on rational behavior and started an enormous literature in economics (see Ehrlich (1973), Witte (1980), McCormick and Tollison (1984), Ehrlich and Brower (1987), Andreoni (1991), Freeman (1996), Glaeser, Sacerdote and Scheinkman (1996), Levitt (1997), Fajnzylber *et al* (1999), *inter alia*). One of the central predictions of the theory is that crime will fall when police presence increases. This is the basis of the main policy implications derived from this theory. A basic problem with this prediction is that it has largely failed to find empirical support. Cameron (1988) presents a survey of the literature. He reports that 18 out of 22 papers surveyed find either a positive effect of police on crime or no relationship between these variables using cross-city variations in the United States.

There are, however, two potentially serious problems with these studies. The first is that most of them look at data aggregated at the city level, so it is possible that changes in police presence have a negative effect on crime in one part of the city under study, and a positive effect on another part (crime is displaced). In such a case, the overall effect of police on crime at the city level may appear to be zero, or even positive, when in fact properly measured the local effect of police on crime is negative. The second problem arises from the simultaneous determination of crime and police presence (see Fisher and Nagin, 1978). It is likely that the government of a city that experiences an increase in crime rates will choose to hire more police officers. Cities that have higher crime rates will have more police officers than those where crime is low. This will introduce a downward bias in the police coefficient in a crime regression. A recent paper that successfully corrects for such simultaneity bias is Levitt (1997). 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 cities in the U.S. during the period 1970-92. The instrumented coefficients are consistently more negative than those obtained using OLS and the point estimates are negative for each of

the seven categories of crime examined. The elasticity of violent crime with respect to sworn officers is –1.0 while that of property crime is –0.3.

While that paper represents a significant point of departure from previous work, Levitt (1997) points out a number of outstanding issues. First, since the timing of elections explains a small part of the variation in police hiring, the 2SLS estimates are imprecise making it difficult to draw policy conclusions. Second, he suggests that the timing of elections may affect crime through other variables than the number of police officers on the street. Controlling for the unemployment rate and spending on education and welfare programs, Levitt (1997) avoids some of these concerns, although there may be other channels through which the timing of elections could affect crime. Police effort and crime reporting (and not only police hiring) may respond to the timing of elections. Or it could be that judges alter their behavior in times of elections (see Posner, 1995). Lastly, the focus is on the effect of police staffing on aggregate crime rates, so it is not possible to estimate displacement effects or to separate incapacitation versus deterrence effects.

In this paper we use a different approach to obtain an estimate of the exogenous effect of police on crime. On July 18th, 1994, a terrorist attack exploded a bomb that completely destroyed A.M.I.A. (Asociacion Mutual Israelita Argentina), the main Jewish center in the city of Buenos Aires, Argentina. 86 people were killed and more than 300 were wounded in the attack. One week later, police protection was placed in front of each Jewish and Muslim institution (such as synagogues, mosques, clubs and schools) in the country. Since the distribution of these institutions can be presumed exogenous in a crime regression, this hideous event can be used as a natural experiment to break the simultaneous determination of crime and police presence.[1] The approach is promising as it allows us to estimate the effect of police on crime with no data on police staffing and distribution, information that is confidential and unavailable to the public in Argentina.

We collected information on the number of motor-vehicle thefts per block in three large neighborhoods in Buenos Aires before and after the terrorist attack. The information covers the nine-month period starting April 1st and ending December 31st,

[1] On natural and randomized experiments, see the discussions in LaLonde (1986), Angrist (1990), Angrist and Krueger (1991), Heckman and Smith (1995) and Hamermesh (1999).

1994. We also collected information on the location of each protected institution in these neighborhoods. We then estimate the effect of police presence on car theft. We find that blocks that receive police protection experience fewer car thefts than the rest of the neighborhood. The effect is economically large. Relative to the sample average, it is equivalent to a drop of 59%. The drop in car thefts in the next block is equivalent to 20%.

Our estimates have two further advantages over previous work. First, there has been considerable interest in the literature in trying to distinguish the mechanisms that make increases in the police force reduce crime (see, for example, Levitt, 1998). One possibility is that more police officers on the streets deter crime because it makes such activities more risky (deterrence). Another possibility is that more police officers reduce crime because they catch more criminals leaving fewer of them around to commit crimes (incapacitation).[2] Our approach is based on the deployment of observable policemen during a relatively brief period of time, not on changes over many years in the size of the police force that is dedicated to combat crime. Thus, all the effect of police on crime in our paper comes from deterrence effects. Moreover, our data allows us to analyze the deterrent effect of exogenous increases in police presence by time of the day, day of the week, and value of the booty, as well as when there are previous crime-protection resources in place.

A second advantage concerns displacement effects. The microstructure of our data allows us to study whether the reduced level of crime in these areas gets displaced to contiguous areas not covered by additional police. Most of the literature uses data on aggregate crime rates at the city level. Thus, one cannot be certain if the observed reduction in city crime does not reflect some shifting of crime into neighboring areas. This makes policy experiments difficult. In our case we have an estimate of the effect of visible police presence on crime rates in neighboring areas. Our estimates do not reject

[2] Kessler and Levitt (1999) use sentence enhancement laws in California for a selected group of crimes to distinguish between incapacitation and deterrence. See also McCormick and Tollison (1984). The significance of incapacitation has been emphasized recently in an article on crime in the UK reporting that *"The Home Secretary, Jack Straw, says that almost half of all crime is committed by a hard core of 100,000 offenders"* (in *The Economist*, February 24th, 2001).

the hypothesis that the total effect of observable police on crime is zero, when we considered the protected blocks and the immediate surrounding area.[3]

In a precise sense, our paper investigates the complementary set of questions to the ones asked by Ayres and Levitt (1998), Lott (1998), and Duggan (2000). They study the effect of the introduction of *unobservable* protection devices (Lojack and concealed handguns) with potentially positive externalities. In our case we estimate the effect of *observable* police presence, which may introduce negative externalities on neighboring areas. Our results are related to a set of important policy questions. One of them concerns whether the deployment of visible self-protection devices, such as private security guards or steering wheel locking bars, should be made taxable or regulated in order to address the market failure generated by the negative externalities. The results can also be useful for the design of the optimal deployment of police forces. For example, to discuss the question of what role, if any, should be given to the disposition of police forces in plain clothes (i.e. not in uniform).[4]

The rest of the paper is organized as follows. In the next section we describe our data and methods. In section III we present our main empirical results. Section IV concludes.

II. Data and Empirical Strategy

II.A. Data

On July 18th, 1994, a terrorist attack destroyed the main Jewish center (A.M.I.A.) in the city of Buenos Aires, Argentina.[5] Seven days later, on July 25th, 1994, 24-hour police protection was given to each Jewish and Muslim institution in the country. Muslim

[3] For a survey of the criminology literature on displacement the reader is referred to Cornish and Clarke (1987) and Hesseling (1994).

[4] Ayres and Levitt (1998) report that in the U.S. it is unauthorized to make a Lojack protection system visible from the outside. The use of plain clothes by officers on duty as an anti-crime strategy is discussed in a recent article in *The Economist*, February 24th, 2001.

[5] This was the second terrorist attack in the city of Buenos Aires. The Embassy of Israel had been destroyed in 1992. In the months immediately following this first attack, the most notorious Jewish centers where given more attention by officers on patrols. However, the surveillance was not generalized and declined gradually. Information on these attacks can be found in www.atentado-amia.com.ar, www.daia.org.ar, www.bnaibrith.org, and www.wzo.org.il.

institutions were protected because of fear of potential retaliations after the attack was claimed by the Islamic organization Hezbollah. A total of over 270 potential targets distributed in the whole country were protected. A significant proportion of the protected buildings are Jewish institutions located within the capital city, Buenos Aires.[6] Although this required the distraction of a non-negligible proportion of the total police force for the city, a serious effort was made to maintain the previous levels of police presence in other parts of the city, particularly in areas where there were many Jewish institutions and the distraction of police forces for protection duties would have been noticeable and large. At that time, it was believed that failure to do so could create a backlash of ill feelings against the Jewish community.[7] In the three neighborhoods under study, the personnel commitment for the new protection duties was significant and simply could have not been fulfilled with the police forces within the neighborhoods.[8] The increased police presence was made up by officers reassigned from administrative tasks, officers from the Communications Division, federal police officers who were previously working in other cities of the country, and officers from the Mounted Police.

A natural question with our approach is the extent to which police officers deployed to protect Jewish and Muslim institutions are effective anti-crime agents. It is true that policemen in this role have a more limited scope for pursuing suspected criminals outside their assigned areas. But they can certainly interfere with crimes that take place near their posts, and they can communicate the presence of suspicious-looking individuals to policemen on patrol cars in the neighborhood. Moreover, criminals probably expect them to intervene. From a more practical point of view, there is ample

[6] In particular, there are no Muslim institutions in the areas considered in our study.

[7] The institutional information for this paper was gathered in a series of interviews with key informants. These included the Secretary of Security of the government (third level of authority in the federal government behind the President and the Ministers) during the period under consideration, the head of the Federal Police force during the period under consideration, the Minister of the Interior during part of the period under consideration, a former federal judge, a former federal prosecutor, and the director of a non-governmental organization devoted to protecting civil rights.

[8] For example, in Once, which is one of the neighborhoods with highest density of Jewish institutions in the city, more than one third of a total of approximately 200 police officers stationed in the neighborhood had to be destined to protection duties. Since this requirement could not be satisfied reallocating the existing police officers, an equal number of officers was sent from outside the neighborhood.

anecdotal evidence of arrests carried out by policemen on duty guarding these institutions.[9]

The data used in this paper comes from three neighborhoods in the city of Buenos Aires. In terms of area, they represent around 3.2% of the city, while in terms of population they account for 6.9%. Each neighborhood is protected by one police station.[10] There were three criteria for selecting these three neighborhoods. First, they have the largest Jewish communities in the city. Second, a significant portion of these neighborhoods is not close to a protected institution (80% of the blocks are more than one block away from a protected institution), so that displacement effects can be evaluated at the micro level. Lastly, we thought three was the maximum number of neighborhoods that we could convince the police force to transfer the data to us, something that had to be done by manual means.[11] There are a total of 836 blocks and 45 protected institutions in this part of the city. 37 of these institutions are inside the considered neighborhoods and the rest are near the boundaries.

We obtained all the information available to the police regarding each auto theft in this area for the 9-month period starting April 1st, 1994 and ending December 31st of that year. This includes the address where the stolen car was parked, time of reported theft, car make and year, and whether the robbery was carried out with the use of violence. A total of 794 non-armed car thefts were reported for these neighborhoods during the period of analysis.[12] We exclude car thefts occurred between July 18th and July 31st.[13] Although thefts normally occur in blocks, in many cases they are reported at

[9] See, for example, *La Nacion*, September 11th, 1999. It reports the conviction of an individual who was apprehended in March 1997 by an officer protecting a Jewish school in Belgrano after robbing a car and killing a person in the vicinity. Two 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 carrying out a burglary of a grocery store nearby -the burglar was later arrested-).

[10] For an in depth discussion of the institutional features of crime and the police force in Argentina, the reader is referred to Pelacchi (2000).

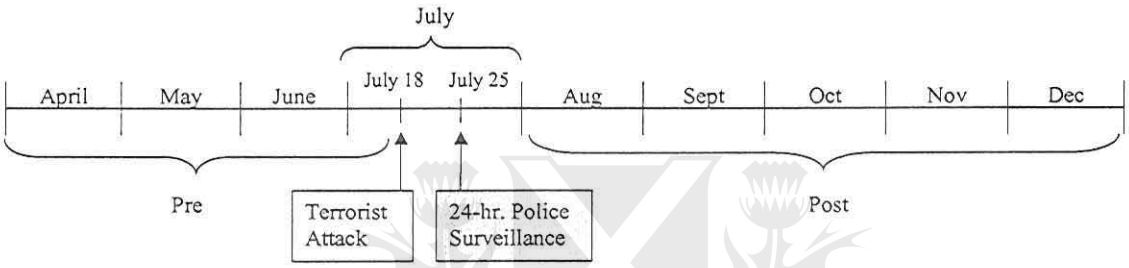
[11] The information was transferred manually from police records. For obvious privacy reasons, it is not normally available to the public, so a special authorization from the Chief of the Federal Police had to be obtained.

[12] We concentrate on non-armed robberies. We also obtained information on a small number (63) of armed robberies.

[13] The first week corresponds to the period before the protection was introduced. A second week guarantees that the surveillance was functioning and known to the public. This eliminates 46 non-armed car thefts. Our results are robust to including the period between July 25th and July 31st.

corners, as this facilitates the victim's verbal description of the crime at the time of filing the police report. We assigned car thefts reported at corners to all the blocks of that intersection.[14] Using the report on the car make and year it was possible to construct an estimate of the car value using standard information from the used car market. The geographic structure of the information is summarized in Tables A1 and A2 in the Appendix. Figure 1 shows a timeline of the events in our study.

Figure 1: Timeline for 1994



Information on self reported crime is usually considered unreliable, as there is a tendency to under-reporting. This problem is minor for car thefts in Buenos Aires for two reasons. Cars have mandatory insurance against accidents. The marginal cost of including insurance against car theft is low and there is a large perceived risk of having your car stolen. This means that the overwhelming majority of cars have insurance against theft. In order for this type of insurance to be activated police intervention is required. Second, criminals often use stolen cars to commit other crimes. By reporting the theft to the police, victims make sure that there can be no confusion about their involvement in these other crimes. A further advantage of auto-theft data is that this category of crime is expected to be more sensitive to street police presence.[15] It appears

[14] This procedure inflates the number of non-armed car-thefts from 748 to 1986. Measurement error in the dependent variable tends to overestimate the standard errors of the regression coefficients.

[15] According to victimization surveys, 89% of car owners in the city of Buenos Aires have car theft insurance and 60% of stolen cars are never recovered (Ministerio de Justicia, 2000). The same study reports that 87% of Buenos Aires car robberies are reported to the police, whereas the percentage is only 29% for all types of crime. It also reports that 94% of Buenos Aires car robberies occur in the street.

that most robberies (where a person is the victim) occur after a brief period of surveillance of the potential victim.[16] Concentrating their attention on the victim, they may miss the presence of police. With auto-theft, on the other hand, the potential victim (parked cars) is stationary and criminals gather some information on the area where they will commit crime.

We completed the data set by collecting information on the geography of these neighborhoods. Our first task was to obtain the exact location of all protected institutions and then to construct four measures of incremental distance from each block to the nearest Jewish institution. The first variable is *Institution 0*, which is a dummy variable that takes the value 1 if there is a protected institution in that block, and zero otherwise. A second variable, *Institution 1*, is a dummy variable that takes the value 1 if a block is one block away from a block containing a protected institution, and zero otherwise. A third measure of proximity to a protected institution is *Institution* 2^{SL}, a dummy taking the value 1 for blocks that are two blocks away from a protected institution, and in a straight line from the blocks that are one-block away from a Jewish institution, and zero otherwise. Finally, *Institution 2* covers all the blocks that are two blocks away from a protected institution, both in a straight line and taking turns. Figure A1 in the Appendix explains the construction of the dummy variables for the protected institutions.[17] We also identified the geographical location of banks, embassies, public buildings, and gas stations in these neighborhoods.

II.B. Empirical Strategy

The empirical exercise exploits two important aspects of our data. First, it uses the fact that the distribution of police officers after the attack is exogenous to the distribution of crime. Second, it uses the fact that we have information on the number of crimes per block before and after the terrorist attack so that we can get rid of a number of unobservables that could affect the amount of crime. To give just one example, if crime is correlated with the location of a protected institution it could simply mean that Jewish institutions tend to be located in middle or high-income areas where there is more

[16] Such criminals are said to operate "on the spot" (in Spanish this is called *al boleo*). All information regarding criminal modes gathered in interviews with key informants.

surveillance. The inclusion of block fixed effects allows us to control for such time-invariant influences by focusing on changes in the amount of auto-theft over time.

We estimate the effect of police on crime controlling for time effects and unobservables, using the following regression:

$$Rob_{it} = M_t + F_i + \alpha\, Police\#_{it} + \varepsilon_{it},$$

where:

Rob_{it} is the number of car thefts in block i in month t,

M_t is a month fixed effect,

F_i is a block fixed effect,

$Police\#_{it} = Institution\#_i * Post_t$ indicates the presence of police protection,

$Institution\#_i$ is a set of dummy variables that indicate the distance of block i to a protected institution (see Figure A1), and

$Post_t$ is a dummy which equals 1 for August, September, October, November, and December, and 0 otherwise.

Finally, it is worth noting that we estimate the effect of police on crime without having direct data on the allocation of police forces (which is confidential). In our study, the presence of police forces is indicated by the distribution of the protected institutions after the terrorist attack.

III. The Effect of Police on Crime

III.A. Basic Estimates

Table 1 shows our basic regression results. In columns (A) and (B) our sample consists of all the blocks in our three neighborhoods. Column (A) focuses on the simplest measure of police presence, *Police 0*, a dummy that takes the value 1 for every month after the attack in every block where there is a Jewish institution. The coefficient on *Police 0* is negative and statistically significant. It is also very large in economic terms. It indicates that blocks that receive direct police protection have 0.173 fewer car thefts than other blocks.

[17] None of the institutions in our sample is located at a corner.

The average number of car thefts per month per block from August through December for all the blocks that are more than two blocks away from a protected institution is 0.296. Relative to this average, car theft falls by 59% in the blocks that have a guarded institution.

A potential problem with this result is that it may be argued that drivers did not want to park near the protected institutions for fear of another terrorist attack. However, both the Embassy of Israel and the AMIA terrorist attacks were focused on the target buildings (the surrounding buildings in the block were not destroyed) so the main impact of fear of future attacks should concentrate on the parking spaces directly in front of the institution. One should also emphasize that finding a legal parking spot in these neighborhoods is often very difficult, reducing the incidence of this problem.[18] A related issue is that, in actual practice, parking is restricted right in front of some protected institutions. To address these issues, we measured that, in average, the parking space in front of the Jewish institutions represents 11% of the total parking space of those blocks (considering both sides of the street). Under a linear relationship, this factor could explain a reduction in the number of car robberies of –0.032 (-0.11 times 0.296 –the average number of car thefts for the control group-). We reject at the 1% significance level that our estimated coefficient equals that value.

Regression (B) includes a larger measure of distance to a protected institution, *Police 1*. This variable captures all blocks that are one block away from the block where the police are situated. Thus, it studies the effect of police on crime on six new blocks. The effect of *Police 0* is negative, significant and only marginally larger in absolute size than the one reported in column (A). The effect of *Police 1*, the marginal effect on neighboring blocks, is negative and significant at the 5% level. It indicates that blocks that are one block away from the guarded institutions experience approximately 0.059 fewer car thefts than the rest of the neighborhood. The estimated effect is approximately one third of the same-block effect. Police presence one block away reduces crime by 20% relative to the control group.

[18] On the severe parking conditions in the city of Buenos Aires, see, for example, *La Nacion*, March 5th, 2001.

It is interesting to note that the size of the effect is still relatively large at a distance that is on the limit of what is visible to the normal human eye. If the car thief is breaking into a car, it is unlikely that he/she will be spotted by a policeman on duty (for an institution that is located exactly in the middle of the block) at a distance ranging from 50 to 150 meters. For example, in the relatively more affluent neighborhoods in Buenos Aires, security guards are placed 100 meters away from each other.[19] If private agents place protection making sure that all parts of the block are covered (i.e. that there are no blind spots) and there are no advantages to having a section of the block protected by two security guards (i.e. with overlapping protection), this suggests that the maximum protection distance is around 50 meters, the distance from the security guard's booth to the house under his/her surveillance that is farthest away.

In columns (C) and (D) we repeat the analysis excluding the blocks where there were no thefts throughout the period of analysis. If for unobservable reasons, there are blocks in which car thefts do not occur, the introduction of police protection in those blocks will have no effect, as the number of car-thefts is already bounded at zero. The inclusion of these blocks in our study would depress the estimated effects. There were 213 such blocks (24% of our sample). As expected, the estimated coefficients are larger (more negative) when we exclude the no-theft blocks. The statistical significance is similar to that shown in columns (A) and (B). The same-block effect is again more than three times larger than the one-block away effect. Excluding the blocks with no thefts throughout the sample period, the average number of monthly car thefts per block from August through December for all the blocks that are more than two blocks away from a protected institution is 0.389. For blocks showing at least one car-theft during the period of analysis, police presence causes a 72% decline in car thefts relative to the control group. The negative effect of police on car thefts taking place in the next block is 20%.[20]

III.B. Displacement Effects and Policy Discussion

[19] Note that such guards are mainly deployed to protect the houses in the block against burglaries, and breaking into a house is more visible than breaking into a car.

[20] Our results are also robust to estimation using count models (such as Poisson and Negative Binomial). Results available upon request.

A standard issue in the crime literature concerns the possibility that criminals that are deterred from committing a crime in a certain area move and commit crimes in another area. By the nature of the data available (aggregated, for example, at the city level), previous research has found it difficult to control for such geographical displacement. Note that it is hard to draw strong policy conclusions without such an estimate, as the benefits of lower crime in one area have to be compared to the costs of higher crime in the displaced areas. Our approach can provide some estimates of displacement effects that are relevant to this type of policy questions.

Table 2 presents estimates of the effect of police presence both for the immediate area of influence and the surrounding areas. Column (A) and (B) simply repeat the previous results using the large sample for purposes of comparison.[21] Regression (C) includes a third measure of proximity to a policeman, *Police* 2^{SL}. This measure takes the value 1 during the post-attack period for all (6) blocks that are two-block away from the block containing a Jewish institution and on a straight line from the blocks that are one-block away (see the measure *Institution* 2^{SL} above). The coefficient on *Police* 2^{SL} is positive and comfortably significant. The size of the coefficient is large in economic terms. Blocks that are outside the immediate radius of the protected institution (2 blocks away) have 0.096 more car thefts than the non-treatment group (the rest of the neighborhood). This difference is slightly over 32% of the control-group level.

Column (D) in Table 2 uses a more generous measure of distance to the police. *Police 2* denotes all (16) blocks that are two-blocks away from the block where there is a Jewish institution, either in a straight line or by taking turns. The coefficient is still positive and significant. Two blocks away from the protected institutions, car-theft increases by 0.055 robberies per month per block relative to the non-treatment blocks. In economic terms, the coefficient represents almost 19% of the control-group average number of car thefts. The size of the coefficient seems smaller than the coefficient on *Police* 2^{SL}, although the difference is not statistically significant.

It is important to make clear our interpretation of the results. There are three possibilities: no deterrence, deterrence with no displacement, and deterrence with

[21] Similar conclusions emerge from studying the sub-sample of blocks for which there was at least one car theft during the sample period.

displacement. If there is no deterrence, then there should be no effect of *Police 0* (or of *Police 1*), so this hypothesis can be rejected. If there were deterrence with no displacement, one would expect that there would be a decline in crime in blocks 0 and 1 relative to the non-treatment group, and then a uniform behavior in all the other blocks. This means that we should not observe a positive and significant coefficient on *Police 2* (or *Police* 2^{SL}), so this hypothesis can also be rejected. The pattern we observe is only possible it there were deterrence with displacement.[22]

Note that the pattern of displacement that we find is inconsistent with endogeneity in the selection of the policemen that take part in the protection duties. Our natural experiment starts from the assumption that the protection of the Jewish institutions is exogenous to the pattern of crime. But it could be argued that policemen are not added, but reshuffled, and that they are taken from areas with little crime to be assigned to the protected institutions. This second potential endogeneity could only explain our results if the low crime areas were located at exactly two blocks from the protected institutions and police were redeployed from those blocks (and not from anywhere else) to blocks 0 and 1, a very unlikely event and one that has been explicitly denied by our key informants.

It is interesting to speculate on the possible reasons for this pattern, whereby police presence displaces crime to nearby areas rather than to far away areas. One possibility is simply that there are search costs involved in travelling to the area where the crime will be committed and finding a potential victim. Once the criminal observes the police in front of the protected institution, he/she moves to the nearby area because it is cheaper than going further away and there are no obvious benefits. Furthermore, it may be a consequence of the time frame of our study in the sense that it may take time for criminals to adapt and find other suitable areas to operate and we are only analyzing the first few months after the attack. It is also possible that criminals "specialize" in a small geographic area. There may be advantages in having specific knowledge on a particular area (like flight routes, hiding places or drivers' parking patterns). These would make it preferable for thieves that usually "work" in a certain geographic area to move to another block but stay in the same vicinity when they know that a block is protected. Maybe

[22] What is particularly convenient is that we find displacement to the immediately surrounding area. Note that even if *Police 2* were zero, we would still be unable to reject the hypothesis of

criminals prefer to be away from a policeman when committing a crime but to be able to predict their movements. The presence of policemen stationed in front of the institutions (from where they are not supposed to move) introduces an element of predictability to police presence. Lastly, it is also possible that gang arrangements divide the city making dangerous for thieves to move to other thieves' turf.[23]

The results can be used to estimate the total effect of police presence on car thefts for the protected block and the surrounding area. Using the estimates presented in Column (C) in Table 2, we cannot reject at conventional levels of significance that the total police effect is zero when we consider the protected block, the six one-block-away blocks, and the six two-blocks-away-in-straight-line blocks. Similar results are obtained using the estimates from Column (D) in Table 2 for a more extended area. These results suggest that placing a visible police officer in a fixed, known location has no deterrent effect on total car theft in the neighborhood. The large, negative effect in the protected area seems to cancel out with the increase in car-theft in the immediate surrounding area.[24]

The results in Columns (C) and (D) of Table 2 compare the change in car-theft zero, one and two blocks away from the institutions relative to the change in car robberies in the more-than-two-block-away area. However, it is possible that the more-than-two-block-away area has also suffered displacement up to some level. If displacement

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uniform displacement to the rest of the neighborhood.

[23] There is anecdotal evidence of the existence of territorial division amongst criminal organizations in Buenos Aires. See *La Nacion*, September 16th, 1997, or January 11th, 1999, for a description of the activities of the so-called "Taxis mafia".

[24] Our key informants have emphasized in interviews that it is well understood in police circles that the probability that a policeman on patrol actually witnesses a crime being committed is quite low. The most likely case is that somebody who has witnessed the crime calls the police to intervene. A recent article in *The Economist*, February 24th, 2001, makes a similar point: *"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."* Note that for a full cost-benefit analysis we should also consider the cost of police presence. Since a policeman in Buenos Aires earns on average a monthly wage of 800 dollars, and policemen work eight-hour shifts and around 21 days per month, the monthly cost of providing police protection is approximately U$3,500 per location.

generated an increase in car robberies in that area, then, by comparing the change in the number of car thefts relative to an inflated baseline, our estimates may be overestimating the local benefits of crime reduction and underestimating the displacement costs. Thus, our estimates of no-overall effect of police on crime could represent an optimistic description if there is some amount of displacement towards the non-immediate surrounding area.

These results have policy implications for other forms of observable crime deterrence activities such as those related to private protection. Ayres and Levitt (1998) cite sources showing the economic importance of private expenditures to reduce crime in the US and how such private spending has outgrown public spending over the recent past. This is also true in many developing countries, including Argentina. This sector is largely unregulated, particularly outside the US. In most countries that we know of, citizens are free to hire visible private protection. Our results show that such activities can shift crime to other areas, suggesting that perhaps some form of taxation or regulation may be appropriate to address such negative externalities. Our results also provide some rationale for why law enforcement agencies condition the acceptance of the Lojack technology on the actual device not being observable from outside of the car (this is reported in Ayres and Levitt, 1998).

A related question concerns the convenience of deploying police forces in a visible way (for example, the proportion of uniformed versus plain-clothes agents). From the point of view of car thefts, visible police protection seems to have a strong local effect with large enough displacement effects so that no overall police effect can be appreciated. But these estimates are obviously insufficient to conclude that deployment of visible police protection is bad from a welfare point of view. Such analysis would require us to obtain some estimate of the benefits to the typical citizen of observing the police protection (and being reminded of their presence), as well as the benefits to risk averse individuals who value knowing if they have police protection or not. Also, visible police protection could make reporting crimes to nearby police easier for victims and by-standers.[25] Furthermore, it would also be important to compare our results with those

[25] It is unclear how important such considerations should be. Remember that everybody would know that police officers are not visible. Thus we could expect that a simple norm to identify

obtained in studies designed to evaluate the effect of fixed police that is unobservable, and in studies designed to estimate the effect of mobile but observable police before drawing more general policy conclusions.

III.C. Further Tests

In this section we present further tests and extensions that help assess the validity of our approach. First we investigate if the blocks that are close to a Jewish institution exhibit a different crime dynamic in the period *before* the terrorist attack. To do this we simply re-estimate our coefficients using a sample that starts on April 1st but that ends on July 17th (just before the terrorist attack takes place). We then imagine a fictitious "terrorist attack" taking place at the middle of this sub-sample period. Thus, we repeat the same exercise as before, but with a reduced sample period and redefining the dummy variable *Post* to take the value 1 for June and July (the first 17 days) and zero for April and May. The results in Table 3 provide some validation to our exercise in the sense that they show that no special crime dynamics affected our treatment group prior to the attack. The results are similar if other alternative cutoff dates for the dummy variable *Post* are used (end of April or end of June).

We also analyze the differential deterrent effect of police presence by time of the day, day of the week, and value of the car in Tables 4 through 6. Table 4 shows that the police effect seems to be stronger during the day than during the night. The difference is significant for the same-block effect. One potential explanation is that the public is more likely to be able to monitor the activities of the policeman during the day, as there are more people on the street. In some sense, during the day the policeman is "under supervision" and must exert effort and be alert. Table 5 suggests that the effect of police presence is higher during weekdays than during weekends. The difference is similar in size to the day-night effect, although it is less well determined.

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police officers in plain clothes would emerge, as has happened in similar social settings. For example, when an adult finds a small child that has gotten lost in a crowded place (such as a beach), he/she starts clapping and places the child on his/her shoulders. Soon most of the people around are clapping. Upon hearing this, parents check where their own children are until the parents of the child identify themselves. This norm is useful because the parents of the lost child are not observable to the adult that has found the lost child, just as the police in plain clothes are not observable to the person wishing to report a crime. It is also very cheap.

Table 6 suggests that the coefficients also display the expected pattern when the sample is divided according to the value of the stolen cars. When there is a policeman present, the thieves will risk it only for more expensive loot. The coefficients on the effect of police presence for expensive car thefts are smaller (less negative) than in the regression restricted to cheap cars, although the difference is not statistically significant.

In some of the blocks in which police protection was placed after the bombing, some source of crime protection was already in place. In Table 7, we compare the effect of the additional police protection in blocks where there is a bank, a public building (including foreign embassies), or a gas station relative to other protected blocks in which these buildings are not present. The first two cases (bank and public building) indicate that there was already some police presence in the block (although only during office hours for the bank and often inside the building). In the last case, the gas station implies significant light and movement during the whole day. We find that the effect of additional police protection is weaker when there were previous sources of crime protection in the same block. The difference, however, is not significant.

IV. Conclusions

Three important challenges in the economics literature on crime started by Becker (1968) are to obtain an estimate of the effect of exogenous increases in police presence, to differentiate between incapacitation and deterrence, and to provide an estimate of how much increased police presence in one area displaces crime to neighboring areas. In this paper we tackle these questions using a natural experiment.

On July 18th, 1994, a terrorist cell exploded a bomb that completely destroyed the main Jewish center in the city of Buenos Aires killing 86 people and wounding more than 300. After the attack, a police officer was placed in front of each Jewish and Muslim institution (such as synagogues, mosques, clubs and schools) in the country. Since the distribution of these institutions can be presumed 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 exact location of car thefts in three neighborhoods in the city of Buenos Aires before and after the attack. Focusing on the

geographic distribution of crime, it is easy to obtain estimates of displacement. Also, by construction, our estimates can only reflect deterrence (not incapacitation) effects.

We find a large, negative and local deterrent effect of police presence on car theft. Blocks that receive police protection experience 0.173 fewer car thefts than blocks that do not receive police protection. The average number of car thefts per block after the attack for our control group is 0.296, so police protection induces a fall in auto theft of approximately 59%. Blocks that are one block away from where such protection is provided experience 0.059 fewer car thefts than the rest of the neighborhood. Thus, in blocks that are close to police protection there are 20% fewer car thefts.

Our estimates also allow us to provide a measure of the amount of auto-theft that is displaced to less protected areas. Blocks that are two blocks away from police protection have around 0.055 *more* car thefts than the rest of the neighborhood. This represents a 19% increase relative to the control-group level. We cannot reject at conventional significance levels that the total effect of observable, fixed police presence on car theft is zero. The negative police effect in the closest area seems to cancel out with the increase in car-theft in the immediate surrounding area.

The police technology considered in this study, the use of uniformed and fixed policemen, resembles exactly the features of the private armed security often hired to protect firms and high-income households. The generalization of our results suggests that these security guards may induce significant negative externalities on the surrounding area. This market failure may generate an overprovision of private security by the free market.

Appendix:

Table A1: Geographical Distribution of Car-Thefts

Neighborhoods	Belgrano	V. Crespo	Once	Total
Blocks	463	260	153	876
Institutions	9	14	22	45
• Inside	7	13	17	37
• In boundaries	2	1	5	8
Non-Armed Car-Thefts	607	197	76	880
• In blocks	181	95	43	319
• In corners	319	87	23	429
• July 18th-July 31st	30	9	7	46
• Misreports†	77	6	3	86

† Misreports correspond to non-existing or incomplete addresses, or car thefts that took place outside the three neighborhoods in our sample (i.e. that were reported in the wrong police station).

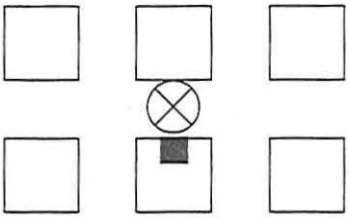
Table A2: Block distance to Jewish Institutions

Block Distance	Frequency	Percent	Cumulative
0	37	4.22	4.22
1	155	17.69	21.92
2	228	26.03	47.95
3	174	19.86	67.81
4	109	12.44	80.25
5	70	7.99	88.24
6	40	4.57	92.81
7	27	3.08	95.89
8	18	2.05	97.95
9	12	1.37	99.32
10	5	0.57	99.89
11	1	0.11	100.00
Total	876	100.00	

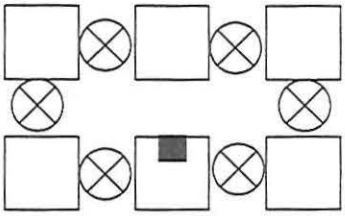
Note: The frequency for block distance # denotes the number of blocks that are # blocks away from the closest Jewish institution.

Figure A1: Four Measures of Distance to a Jewish Institution

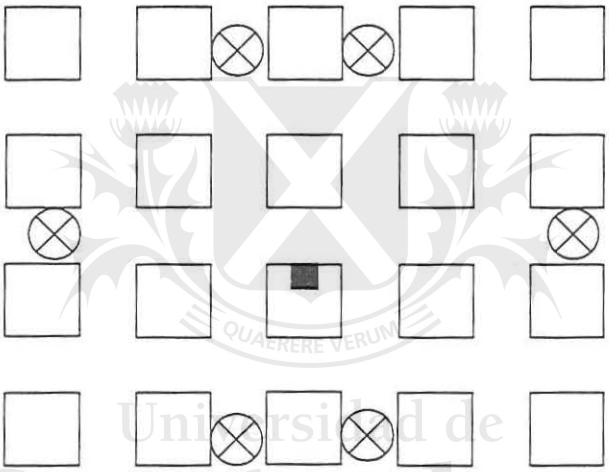
Institution 0



Institution 1



Institution 2^{SL}



Institution 2

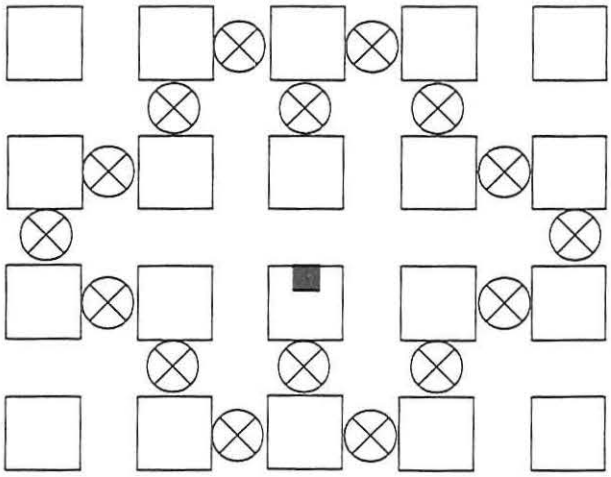


Table 1: The Effect of Police Presence on Car-Theft

	(A) LSDV	(B) LSDV	(C) LSDV	(D) LSDV
Police 0	-0.17330***	-0.18473***	-0.26715***	-0.28133***
	(-3.03)	(-3.21)	(-3.15)	(-3.31)
Police 1		-0.05954**		-0.07641*
		(-1.99)		(-1.93)
Block Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
N of observations	7884	7884	5967	5967
	F=12.56***	F=11.70***	F=12.70***	F=11.81***

Notes: Dependent variable: number of car-thefts per month per block. Least Squares Dummy Variables (LSDV) regressions. Regressions (C) and (D) exclude blocks with no theft throughout our sample period. Car thefts occurred between July 18th and July 31st are excluded. *t*-statistics are in parentheses. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Table 2: Displacement Effects

	(A) LSDV	(B) LSDV	(C) LSDV	(D) LSDV
Police 0	-0.17330***	-0.18473***	-0.17182***	-0.16629***
	(-3.03)	(-3.21)	(-2.98)	(-2.86)
Police 1		-0.05954**	-0.04663	-0.04110
		(-1.99)	(-1.54)	(-1.32)
Police 2^{SL}			0.09617**	
			(2.51)	
Police 2				0.05530**
				(1.99)
F-stat			0.15†	0.75‡
Block Fixed Effect	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes
N of observations	7884	7884	7884	7884
	F=12.56***	F=11.70***	F=11.22***	F=10.96***

Notes: Dependent variable: number of car-thefts per month per block. LSDV regressions. Car thefts occurred between July 18th and July 31st are excluded. *t*-statistics are in parentheses. † Null hypothesis: *Police 0* + 6 x *Police 1* + 6 x *Police* 2^{SL}=0. ‡ Null hypothesis: *Police 0* + 6 x *Police 1* + 16 x *Police 2*=0. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Table 3: Car-Theft Before the Terrorist Attack

	(A) LSDV	(B) LSDV
Police 0	0.03095 (0.41)	0.03912 (0.52)
Police 1		0.04256 (1.09)
Block Fixed Effect	Yes	Yes
Month Fixed Effect	Yes	Yes
N of observations	3504 F=22.73***	3504 F=18.42***

Notes: Dependent variable: number of car-thefts per month per block. LSDV regressions. Sample period: April 1st-July 17th. The variable *Police#* is now constructed using a variable called *Post* which equals 0 between April 1st and May 31st, and 1 between June 1st and July 17th. Similar results obtained with a cutoff for *Post* set at April 30th or at June 30th. *t*-statistics are in parentheses. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Table 4: Day vs. Night

Dependent Variable	(A) Night Thefts	(B) Day Thefts	(C) Night-Day
Police 0	-0.04681	-0.13791***	0.09110*
	(-1.46)	(-3.00)	(1.67)
Police 1	-0.02028	-0.03926	0.01897
	(-1.22)	(-1.64)	(0.67)
Block Fixed Effect	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes
N of observations	7884	7884	7884
	F=5.61***	F=9.54***	F=4.45***

Notes: Dependent variable: number of car-thefts per month per block. LSDV regressions. Car thefts that occurred between July 18th and July 31st are excluded. Night car thefts are car thefts reported between 10pm and 10am. 650 night car thefts and 1336 day car thefts in the sample. In column (C), the dependent variable is the difference between the number of night and day car thefts. *t*-statistics are in parentheses. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Table 5: Weekday vs. Weekend

Dependent Variable	(A) Weekday Thefts	(B) Weekend Thefts	(C) Weekday-Weekend
Police 0	-0.13008***	-0.05464*	-0.07543
	(-2.72)	(-1.70)	(-1.31)
Police 1	-0.03470	-0.02483	-0.00987
	(-1.40)	(-1.49)	(-0.33)
Block Fixed Effect	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes
N of observations	7884	7884	7884
	F=10.79***	F=5.58***	F=6.64***

Notes: Dependent variable: number of car-thefts per month per block. LSDV regressions. Car thefts that occurred between July 18th and July 31st are excluded. 1386 weekday car thefts and 600 weekend car thefts in the sample. In column (C), the dependent variable is the difference between the number of weekday and weekend car thefts. *t*-statistics are in parentheses. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Table 6: By car value

Dependent Variable	(A) Expensive	(B) Cheap	(C) Expensive-Cheap
Police 0	-0.03084**	-0.09822**	0.06738
	(-2.50)	(-2.27)	(1.49)
Police 1	-0.01577**	-0.02491	0.00914
	(-2.46)	(-1.11)	(0.39)
Block Fixed Effect	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes
N of observations	7884	7884	7884
	F=19.21***	F=9.25***	F=4.27***

Notes: Dependent variable: number of car-thefts per month per block. LSDV regressions. Car thefts that occurred between July 18th and July 31st are excluded. Expensive cars are valued above the mean sample value (U$8,871). 674 expensive car thefts and 1,206 cheap car thefts in the sample. Car model and, thus, value is not available for all reported car thefts. In column (C), the dependent variable is the difference between the number of expensive and cheap car thefts. *t*-statistics are in parentheses. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Table 7: Other Sources of Crime Protection

	(A) Bank	(B) Public Building	(C) Gas Station
Police 0 * (1 - Protection)	-0.19090***	-0.19519***	-0.18634***
	(-3.23)	(-3.31)	(-3.20)
Police 1 * (1 - Protection)	-0.06885**	-0.05909*	-0.05901**
	(-2.23)	(-1.94)	(-1.97)
Police 0 * Protection	-0.07662	-0.00162	-0.12662
	(-0.32)	(-0.01)	(-0.37)
Police 1 * Protection	0.04645	-0.06947	-0.10162
	(0.49)	(-0.54)	(-0.42)
F-stat†	0.21	0.61	0.03
F-stat‡	1.37	0.01	0.03
Block Fixed Effect	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes
N of observations	7884	7884	7884
	F=9.88***	F=9.80***	F=9.75***

Notes: Dependent variable: number of car-thefts per month per block. LSDV regressions. Car thefts occurred between July 18th and July 31st are excluded. *Protection* equals 1 when a Bank (Column A), a Public Building (Column B) or a Gas Station (Column C) is located in the block, and 0 otherwise. *t*-statistics are in parentheses. † Null hypotheses: *Police 0* * (1 - *Protection*) = *Police 0* * *Protection*. ‡ Null hypothesis: *Police 1* * (1 - *Protection*) = *Police 1* * *Protection*. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

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