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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

Two of the main challenges in the literature on crime are to isolate significant causal effects of police on the amount of crime, and to distinguish between deterrence and incapacitation. Following a terrorist attack on the main Jewish center in July 1994, all Jewish and Muslim institutions in the city of Buenos Aires (including schools, synagogues, mosques and clubs) were given 24-hour police surveillance. Thus, in these areas the distribution of police presence after the terrorist attack can be presumed exogenous in a crime-regression. Furthermore, crime reductions can only reflect deterrence effects. We collected data on the location of all car thefts for three neighborhoods of the city of Buenos Aires before and after the terrorist attack to study the deterrent effect of police on crime. Our estimates suggest that there is a large, negative effect of visible police presence on car-theft. We also find evidence of displacement: there is a positive and significant effect of visible 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 police on crime.

JEL: K42
Keywords: Crime, deterrence, incapacitation, 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 (Beccaria (1764), Bentham (1789)). 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), Andreoni (1991), 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 US.

There are, however, two potentially serious problems with these studies. The first is that most of them look at aggregate data, so it is possible that changes in police presence have a negative effect on crime on one area under study, and a positive effect on another area (crime is displaced). In such a case, the overall effect may appear zero or even positive, when in fact properly measured the local effect of police on crime is negative. The second problem is one of endogeneity arising 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 this 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 US over the period 1970-92. The instrumented coefficients are consistently more negative than the first differenced ones 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 the previous literature, 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. By including spending on education and public welfare programs and the unemployment rate, Levitt (1997) avoids some of these concerns, although the point estimates on the coefficients on these variables are often insignificant and there may be other channels through which the timing of elections could affect crime. He mentions that the police are an ideal target for political manipulation so that police effort and crime reporting (and not only police hiring) may respond to the timing of elections. Or it could be that in times of elections, politicians undertake public works that raise wages for low skilled jobs and this is somehow related to crime. Another channel other than police hiring through which elections may affect crime would occur if judges alter their behavior in times of elections (see Posner (1995)). Levitt also finds a surprising pattern across crime categories, with murder yielding the largest apparent effect of police and the category of crime where the rational model is presumed to be more relevant, auto-theft, exhibiting very imprecise estimates. Lastly, the focus is on the effect of police staffing on aggregate crime rates, so no separation of incapacitation versus deterrence effects is possible.

In this paper we use a different approach to estimate the exogenous effect of police on crime. On July 18, 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. The approach is promising as it allows us to estimate the effect of the police on crime with little or no data on police staffing and distribution, information that is secret and unavailable to the public in

Argentina. Furthermore, the application proposed does not suffer from a number of shortcomings when experimentation is used. See, for example, the discussions in Angrist (1990) and Heckman and Smith (1995).

We collect information on the number of motor-vehicle thefts per block in three large neighborhoods in Buenos Aires. The information covers the nine-month period starting April 1st and ending December 31st, 1994. We also collected information on the location of each protected institution in these neighborhoods. We then compare the distribution of theft before and after the terrorist attack. We find that the number of thefts falls by 70% in the block where there is direct police presence, and by 20% in the next block.

Our estimates have two further advantages over previous work. First, there has been considerable interest in the literature in trying to distinguish how exactly 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 incapacitation. More police officers reduce crime because they catch more criminals leaving fewer of them around to commit crimes.[1] Our approach is based on the geographical distribution of the police force during a relatively brief period of time, not on changes in the size of the police force over many years. Thus, all the effect of police on crime in our paper comes from deterrence effects. Moreover, our data allows us to analyze the differential deterrent effect of police presence by time of the day, day of the week, and value of the booty.

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 areas not covered by 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, i.e. an increase in non-city crime. This makes policy experiments difficult. In our case we have an estimate of the effect visible police presence on crime rates in neighboring areas. To the extent that our measures of local displacement can be used to predict the effect for larger jurisdictions,

we can be reasonably comfortable that we are estimating the total effect of police on crime. In a more general sense, our paper investigates the opposite set of questions asked in Lott (1998) and Ayres and Levitt (1998). They study the effect of the introduction of laws that allow for the use of *unobservable* devices (concealed handgun laws and Lojack respectively) with potential positive externalities. In our case we estimate the effect of *observable* police presence that may show negative externalities on neighboring areas.

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 18, 1994 a terrorist attack destroyed the main Jewish center (A.M.I.A.) in the city of Buenos Aires, Argentina.[2] Seven days later, on July 25, 1994, 24-hour police protection was given to each Jewish and Muslim institution in the country. Muslim 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.[3] 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. At that time, it was believed that failure to do this could create a backlash of ill feelings against the Jewish community at a time of rising crime. The reason why this movement of officers did not imply a reduction of protection in other parts of the city was that most of the increased police presence was made up by officers

[1] Kessler and Levitt (1999) uses sentence enhancement laws in California for a selected group of crimes to distinguish between incapacitation and deterrence. See also McCormick and Tollison (1984).

[2] 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.

reassigned from administrative tasks, officers from the Communications Division, federal police officers who were previously working in places other than the city of Buenos Aires, and officers from the Mounted Police.[4,5]

A natural question with our approach concerns 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 while they take place near their posts, and they can communicate the presence of such 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 anecdotal evidence of arrests carried out by policemen on duty guarding these institutions.[6] A further reason why the presence of an institution in an area is expected to increase the amount of active police presence is the fact that patrol officers include these blocks in their rounds so as to make periodic checks on their colleagues who are assigned to fixed targets. In other words, rounds were redesigned so as to pass in front of guarded institutions.

The data used in this paper comes from three neighborhoods in the city of Buenos Aires. In terms of area, they represent x% of the city, while in terms of population they account for y%. There were three criteria for selecting these three neighborhoods. First, they have the largest Jewish communities in the city. Second, the largest of them has a significant portion (almost x% of that neighborhood and y% of the total area under

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

[4] Information gathered through interviews with key informants. These include the Secretary of Security of the government (third level of authority, 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 (second level of authority behind the President) during part of the period under consideration, and a former federal Judge.

[5] This does not mean that the personnel commitment of each police station was insignificant. In Once, for example, which is one of the neighborhoods with highest density of Jewish institutions in the city, almost two thirds of a total of approximately 200 police officers had to be destined to protection duties. An equal number of officers were sent from outside the neighborhood.

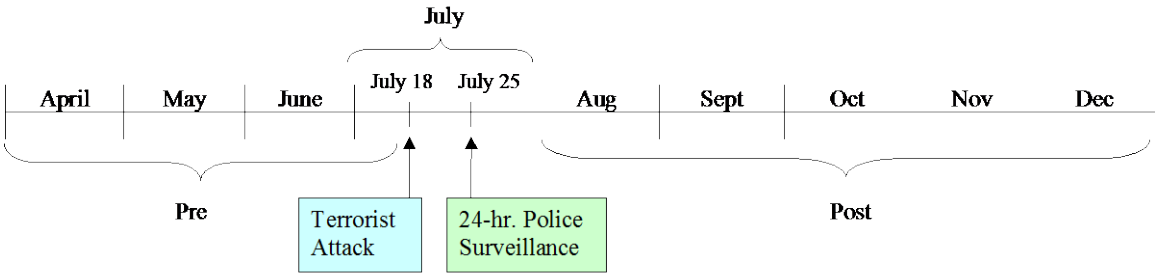
[6] See, for example, *La Nacion*, September 11, 1999. It reports the conviction of an individual who was apprehended in March 1997 by an officer guarding a Jewish school in Belgrano after robbing a car and killing a person in the vicinity. Another police officer guarding a Jewish institution in Once was stabbed in a struggle with a thief who was carrying out a burglary of a grocery store near the institution. The burglar was later arrested. Another arrest is reported in Villa Luro.

analysis) with no guarded institutions 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 manually transfer the data for us. There are a total of 836 blocks and 45 guarded 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 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.[7] A total of 794 non-armed car thefts were reported for these neighborhoods during the period of analysis.[8] We exclude car robberies occurred between July 18 and July 31.[9] Although robberies normally occur in blocks, in many cases they are reported in 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 in corners to all the blocks of that intersection.[10] 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. Figure 1 shows a timeline of the events in our study.

[7] The information was transferred manually from police records. It is not normally available to the public, so an authorization from the Chief of the Federal Police was obtained.
[8] We also obtained information regarding 63 armed robberies.
[9] 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 and 5 armed car robberies. Our results are robust to including the July 25-July 31 period.
[10] Our results are robust to assigning these car-thefts arbitrarily to one of these four streets. This inflates the number of non-armed car-thefts from 748 to 1986.

Figure 1: Timeline for 1994



The geographic structure of the information is summarized in Tables A1 and A2 in Appendix A.

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 police presence. It appears that most robberies (where a person is the victim) occur after a brief period of surveillance of the potential victim.[11] 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.[12]

We completed the data set collecting information on the geography of these neighborhoods. Our first task was to obtain the exact location of all guarded institutions and then to construct four measures of distance from each block to the nearest institution. The first variable is *Institution 1*, which is a dummy variable that takes the value 1 if

[11] Such criminals are said to operate "on the spot" (in Spanish this is called *al boleo*).

there is a guarded institution in that block, and zero otherwise. A second variable, *Institution 2*, is a dummy variable that takes the value 1 if a block is zero or one block away from a block containing a guarded institution, and zero otherwise. A third measure of proximity to a protected institution is *Institution 3*, a dummy taking the value 1 for blocks that are two blocks away in a straight line from a block containing a Jewish institution. *Institution 4* is similar, only that it covers blocks that are two blocks away in a straight line or taking turns from a block containing a protected institution. Figure A1 in Appendix A explains the construction of the dummy variables for the protected institutions.[13]

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 un-observables that could affect the amount of crime. To give just one example, if crime is correlated with the location of protected institution it could simply mean that Jewish and Muslim institutions tend to be located in rich areas where there is more surveillance. By focusing on the change in crime we can make sure that such influences are controlled for.

To estimate the effect of police on crime controlling for time effects and unobservables, we run the following regression:

$$Rob_{it} = M_t + F_i + \alpha\, PoliceX_{it} + \varepsilon_{it},$$

where:

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

M_t is a monthly fixed effect,

F_i is a block fixed effect,

$PoliceX_{it} = InstitutionX_i * Post_t$, which indicates the presence of police protection,

where:

[12] Information gathered through interviews with key informants.
[13] None of the institutions in our sample is located in a corner.

$InstitutionX_i$ indicates the distance of block *i* to a guarded institution,

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

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 guarded institutions.

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 1*, 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 1* is negative and comfortably significant. It is also very large in economic terms. The average number of monthly car robberies per block (excluding robberies between July 18 and July 31) is 0.252 (considering all the blocks). Thus, in the same block, robberies fall by 69% (relative to the mean level).

Regression (B) includes a larger measure of distance to a protected institution, *Police 2-1*. 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 1* is negative, significant and only marginally larger in absolute size than the one reported in column (A). The effect of *Police 2-1*, the marginal effect on neighboring blocks, is negative and significant at the 5% level. The estimated effect is approximately one third of the same-block effect. Police presence one block away reduces crime by 24%.

In columns (C) and (D) we repeat the analysis excluding the blocks where there are no thefts throughout the period of analysis. If for unobservable reasons, there were blocks in which robberies do not occur, the inclusion of these blocks in our study would introduce noise into the measurement of our proxy for police presence and reduce 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 those shown in columns (A) and (B). The same-block effect is again more than three times larger than the one-block effect. Excluding the blocks with no thefts throughout the sample period, the average number of monthly car robberies per block is now 0.333. Thus, relative to the mean level for blocks showing at least one car-theft during the period of analysis, police presence causes an 84% decline in car thefts. The negative effect of police on car thefts taking place in the next block is 23%.

III.B. Displacement Effects and Policy Discussion

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 displacement effects. 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 study can provide some estimates that have to do with these 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.[14] Regression (C) includes a third measure of proximity to a policeman, *Police 3-2*. This measure takes the value one during the post-attack period for all (6) blocks that are two-block away from the block containing a Jewish institution on a straight line from the blocks that are one-block away (see the measure *Institution 3* above). The coefficient on *Police 3-2* is positive and comfortably significant. The size of the coefficient is large in economic terms. Police presence induces a 38% increase in the number of car thefts that take place per month per block in those blocks that are outside the immediate radius of the protected institution.

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

Column (D) in Table 2 uses a more generous measure of distance to the police (*Institution 4*). *Police 4-2* denotes all (16) blocks that are two-block away from the block where there is a Jewish institution, both in a straight line or taking turns. The coefficient is still positive and significant at the 10% level. The size of the coefficient seems smaller than the coefficient on *Police 3-2* in column (C). Although the difference between these two coefficients is not statistically significant, it is interesting to speculate on the possible reasons. Maybe criminals prefer to be away from a policeman when committing a crime but to be able to see their movements. As these policemen are stationed in front of the institutions and should not move, their presence introduces an element of predictability (at the end of each shift replacements arrive and the policemen leave for the central office).

The results can be used to estimate the total effect of police presence on car thefts for the guarded block and the surrounding area. Using the estimates presented in Column (C) in Table 2, we do not reject at any relevant significance level that the total police effect is zero when we consider the guarded block, the six one-block-away blocks, and the six two-block-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 total deterrent effect on car theft in the neighborhood. The large, negative effect in the closest area seems to cancel out with the increase in car-theft in the immediate surrounding area.[15]

These results have policy implications for other forms of 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 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 citizens can hire visible private protection. Our results show how such activities shift crime to other areas. Our results also provide some rationale for why law enforcement agencies condition the acceptance of the Lojack

[15] A cost-benefit analysis should also consider the cost of police presence. Since a policeman in Buenos Aires earns a monthly wage of 800 dollars, and policemen work eight-hour shifts and 21 days per month, the monthly cost of providing police protection is U$3,429 (= 4.29 x 800).

technology on the actual device not being observable from outside of the car (reported in Ayres and Levitt (1998)).

III.C. Further Tests

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 3 through 5. Table 3 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 observing 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" whereas during the night he/she can look the other way. Table 4 suggests that the effect of police presence is higher during weekdays than during weekends. The difference, however, is not significant.

Table 5 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 an expensive loot). The coefficient on the effect of police presence in the number of expensive car thefts is smaller (less negative) than in the regression restricted to cheap cars. But again, the difference is not statistically significant.

IV. Conclusions

One of the main predictions in the economics literature on crime started by Becker (1968) is that the presence of police deters crime. Unfortunately, the previous literature has failed to find evidence consistent with this hypothesis, mainly because of a simultaneity bias. The main exception is Levitt (1997), who finds significant negative effects of police on crime using electoral cycles in police hiring as an instrument for police staffing. A number of issues remain open, however. These include the fact that electoral cycles are only weakly related to police hiring, leading to imprecise second stage estimates and making policy conclusions very tentative, and the possibility that there may be other channels (apart from police hiring) through which electoral cycles could affect crime, including police effort, crime reporting, judicial activity or political cycles in public

works. Furthermore, the estimates do not distinguish incarceration versus deterrence effects nor provide an estimate of displacement effects.

In this paper we obtain estimates of the effect of visible police presence on crime using a natural experiment. On July 18, 1994 a terrorist cell exploded a bomb that completely destroyed the main Jewish center in the city of Buenos Aires, Argentina. 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.

Our estimates suggest that there is a large, negative and local deterrent effect of police presence on car theft. The average number of car thefts falls by approximately 70% when there is direct police protection. One block away from where such protection is provided, car thefts fall by 24%. Our estimates also allow us to provide a measure of the amount of displacement. Two blocks away the average number of car theft increases by 38%. We cannot reject at any relevant significance level that the total effect of visible, fixed police presence 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. Our estimates also suggest that the effect seems to be stronger during the day than during the night, and not to depend on the value of the stolen cars, or whether we look at the effect of police presence during weekdays versus weekends.

Appendix A

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	192	99	48	339
• In corners	338	92	25	455
• Misreports[A]	77	6	3	86
Armed Car-Robberies	51	12	5	68
• In blocks	19	0	0	19
• In corners	27	12	5	44
• Misreports[A]	5	0	0	5

Note: (A) denotes 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).

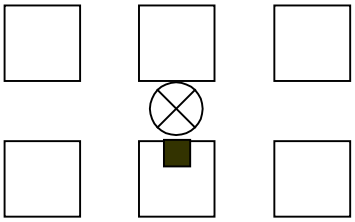
Table A2: Block distance to Jewish Institutions

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	

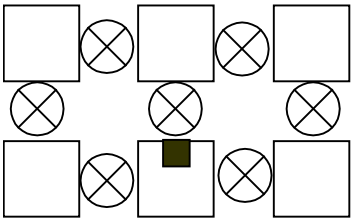
Note: The frequency for block distance X denotes the number of blocks that are X blocks away from at least one Jewish institution.

Figure A1: Four Measures of Distance to a Jewish Institution

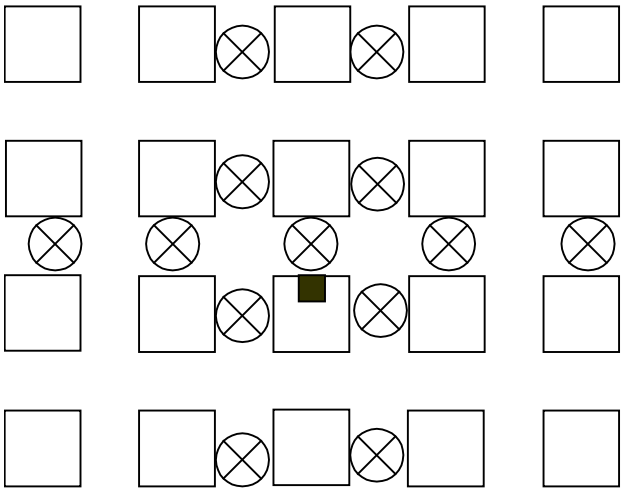
Institution 1



Institution 2



Institution 3



Institution 4

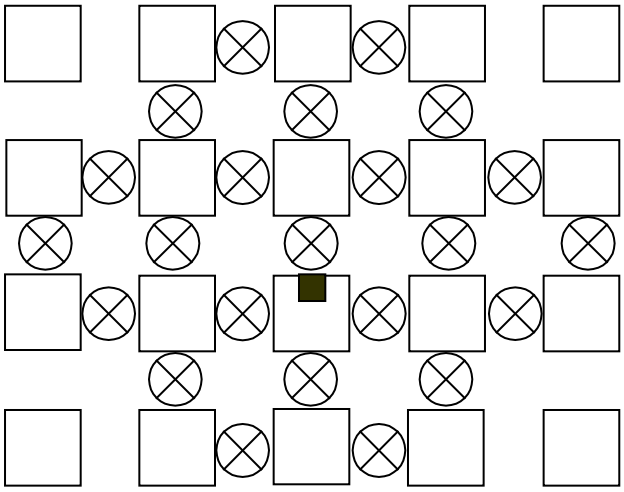


Table 1: The Effect of Police Presence on Car-Theft

	(A) LSDV	(B) LSDV	(C) LSDV	(D) LSDV
Police 1	-0.17330***	-0.18473***	-0.26715***	-0.28133***
	(-3.029)	(-3.213)	(-3.155)	(-3.311)
Police 2-1		-0.05954**		-0.07641*
		(-1.994)		(-1.926)
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 18 and July 31 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 1	-0.17330***	-0.18473***	-0.17182***	-0.16807***
	(-3.029)	(-3.213)	(-2.978)	(-2.889)
Police 2-1		-0.05954**	-0.04663	-0.04478
		(-1.994)	(-1.540)	(-1.450)
Police 3-2			0.09617**	
			(2.508)	
Police 4-2				0.05085*
				(1.860)
F-stat			0.15†	0.50‡
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 18 and July 31 are excluded. *t*-statistics are in parentheses. † Null hypothesis: *Police 1* + 6 x *Police 2-1* + 6 x *Police 3-2*=0. ‡ Null hypothesis: *Police 1* + 6 x *Police 2-1* + 16 x *Police 4-2*=0. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Table 3: Day vs. Night

Dependent Variable	(A) Night Robberies	(B) Day Robberies	(C) Night-Day
Police 1	-0.04681	-0.13791***	0.09110*
	(-1.459)	(-2.996)	(1.666)
Police 2-1	-0.02028	-0.03926	0.01897
	(-1.217)	(-1.642)	(0.668)
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 18 and July 31 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 numbers 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 4: Weekday vs. Weekend

Dependent Variable	(A) Weekday Robberies	(B) Weekend Robberies	(C) Weekday-Weekend
Police 1	-0.13008***	-0.05464*	-0.07543
	(-2.724)	(-1.697)	(-1.308)
Police 2-1	-0.03470	-0.02483	-0.00987
	(-1.400)	(-1.485)	(-0.330)
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: LSDV regressions. Car thefts that occurred between July 18 and July 31 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 5: By car value

Dependent Variable	(A) Expensive	(B) Cheap	(C) Expensive-Cheap
Police 1	-0.03084**	-0.09822**	0.06738
	(-2.500)	(-2.266)	(1.491)
Police 2-1	-0.01577**	-0.02491	0.00914
	(-2.463)	(-1.107)	(0.390)
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: LSDV regressions. Car thefts that occurred between July 18 and July 31 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 the reports. 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.

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