

# EFFECTS OF GUN SEIZURES ON GUN VIOLENCE: "HOT SPOTS" PATROL IN KANSAS CITY\*

LAWRENCE W. SHERMAN  
University of Maryland-College Park

DENNIS P. ROGAN  
Crime Control Institute

We tested the hypothesis that greater enforcement of existing laws against carrying concealed weapons could reduce firearms violence with a quasi-experimental, target beat/comparison beat design. Over a six-month period in a ten-by-eight-block area with a homicide rate 20 times higher than the national average, intensive patrol near gun crime hot spots produced a 65 percent increase in firearms seized by police. Gun crimes declined in the target area by 49 percent, with no significant displacement to any patrol beat surrounding the target area. Neither gun crimes nor guns seized changed significantly in the comparison beat several miles away.

Can police take more guns off the streets? Would it make any difference for gun crime if they did? The answers to these questions are critically important, almost regardless of the future of gun control laws in the United States. Even if all handgun possession or carrying were banned tomorrow, the gun stock to be controlled would be an estimated three handguns for every 10 persons in the United States (Kleck 1991: Table 2.1). If gun carrying is an essential proximate cause of gun crimes outside the home, then compliance with laws against gun carrying is a key issue for reducing gun crime.

## GUN CARRYING AND GUN CRIME

While the literature on guns and violence has focused primarily on gun *density* (Cook 1991; Reiss and Roth 1993: Chapter 6) gun

---

\* This report was supported in part by grant 91-DD-CX-KO56 from the National Institute of Justice to the University of Maryland. Findings and conclusions of the research reported here are those of the authors and do not necessarily reflect the official position or policies of the U.S. Department of Justice. The authors present this report in collaboration with the Kansas City (Mo.) Police Department, with special thanks to Police Chief Steven Bishop, Major Dennis Shreve, Deputy Chief James Nunn, Captain John Hamilton, Sergeants Tim Edwards and David Burns, and Ms. Judy Robinette.

*carrying* is a logical extension and specification of the dynamics of gun density. The two variables may be highly correlated over time, allowing strong relationships to emerge between gun density and homicide (e.g., McDowall 1991). But carrying frequency per gun may be the behavioral mechanism by which gun density is translated into gun crime. Thus it is conceivable that some cities with high handgun density may have lower gun crime rates because the carrying frequency per handgun is lower than in a city with few guns but high carrying frequency per gun. Reports of multiple murders over time committed with the same gun in Washington, DC, where all handguns are banned (Lewis 1995), suggest that carrying frequency per handgun may vary enormously, depending on the social networks and community conditions in which the gun is circulated.

The importance of gun carrying is also suggested by the results of the evaluations of the 1974 Bartley-Fox law in Massachusetts, which created mandatory one-year prison sentences upon conviction for illegal gun carrying. Although the implementation of this law was eventually watered down, the substantial publicity effort and early vigorous enforcement reduced gun homicides as well as gun use in robberies and assaults (Pierce and Bowers 1979; Reiss and Roth 1993: 275).

The importance of gun carrying is consistent with the strong geographic concentration of gun crimes. Even in high-crime neighborhoods, most addresses never suffer any gun crimes, while a few addresses experience them repeatedly. Over a two-year period in Indianapolis, for example, gun crimes were reported at only 3 percent of all addresses, with 6,409 gun calls at only 5,024 addresses; after seven gun calls at an address, the probability of further gun calls rose to 100 percent. Concentrations are even clearer at the block level: one block registered 68 calls for service about guns; another, 55; a third, 53; and a fourth, 33 (Sherman 1995). At these gun crime hot spots, the percentage of persons on the streets carrying guns may be far higher than elsewhere in the city.

Gun-carrying hot spots also may attract repeat gun criminals, a clearly documented problem. One study of juvenile arrests in Washington, DC showed that 373 (17 percent) of 2,176 persons arrested at least once on gun charges were arrested again for the same reason over a six-year period. Fifty had at least three gun charges as juveniles; 10 of these were later charged with murder and five were shot to death themselves (Lewis 1995).

*Policing Gun Carrying*

These findings are consistent with Wilson's (1994) hypothesis that police can reduce gun violence by more enforcement of laws against gun carrying in high-risk places, by high-risk people, at high-risk times. Kleck offers a similar suggestion:

[P]olice departments might experiment with increasing street searches and arrests for unlawful carrying, and/or improving the targeting of searches. Even in the absence of increased use of prison sentences for violators, increased carrying arrests might deter the casual, routine carrying of firearms, and thereby indirectly reduce opportunistic robberies. (1991: 441).

Most big-city police agencies, however, have done relatively little to test that hypothesis. Rather than focusing on gun seizures as a strategy for preventing gun crime, police have tended to obtain guns reactively in the course of other enforcement duties (Moore 1980). Cities vary widely in both the effort they expend on gun seizures and in the results (Brill 1977). A survey of 1993 gun seizures in 30 large police agencies (Sherman and Bridgeforth 1994) found the highest rate of guns seized per 100 police employees (331 in Phoenix) was seven times as great as the lowest (47 in New York City). The highest rate per 100,000 population (1075 in Chicago) was five times the lowest rate (202 in Long beach). The greatest variation was in the rate of guns seized per 100 homicides: the rate for the highest-ranking city, Albuquerque (8,333), was 13 times as great as for the lowest ranking city, Los Angeles (639).

Perhaps the most surprising result of the survey was that almost half of the cities surveyed (of all 59 over 250,000 population) responded that they did not know the number of guns seized. Few police chiefs use that number as a productivity statistic, and few annual police reports mention it. Few officers apparently go out of their way to try to find guns on the street. In an 11-month period in 1993-1994, half of the 895 Indianapolis police officers seized no guns at all, while 10 percent of the officers seized over 50 percent of the total 2,318 guns seized (Sherman 1995).

The lack of importance given to seizures of illegal guns may reflect pessimism about the difficulty of replacing guns. Many police agree that an offender from whom a gun is taken can replace that gun within hours. Gun offenders' ability to replace seized weapons, however, should not be taken for granted. As youthful gun violence has exploded—with Washington, DC juvenile arrests three times more likely than adult arrests to include gun charges (Lewis 1995)—young people's ability to replace guns is increasingly important to total gun crime. Incarcerated felons have boldly told

survey researchers that they believed they could obtain a replacement gun "within a few hours," but the reality may be closer to this case reported from Washington, DC:

One youth told [the interviewer] he had spent more than two weeks hiding in his house after his gun was confiscated when he was frisked entering a local disco. He said he didn't leave the house again until he had obtained another gun (Lewis 1995:A1).

Taking guns away from high-risk persons may not incapacitate them permanently from carrying guns, but it may do so in the short term. It may incapacitate them long enough to allow the cooling of their anger at someone they had planned to shoot. It also may reduce the supply of rent-a-guns (Lewis 1995) widely circulated for criminal purposes. Alternatively, taking guns may specifically deter the confiscatees and may generally deter others from carrying guns in high-gun-enforcement areas, so that they may avoid the inconvenience, expense, and perceived danger (due to lack of self-defense capacity) of temporarily losing access to a gun.

Even if Wilson's (1994) hypothesis is correct, one still might expect some displacement of gun crime to other places beside the targeted gun hot spots. Yet if routine activities theory is correct (Felson 1994), the likelihood that gun crimes will occur outside the hot spots should be substantially lower than within those spots. Fewer interactions with likely victims or antagonists should lead to fewer uses of the weapon, even if the carrying rate remains the same outside the hot spots. Thus the hypothesis that greater enforcement of gun carrying in gun crime hot spots can reduce gun crime overall is at least theoretically plausible. Not for the first time in modern police history, the Kansas City (Mo.) Police Department was the first to put the hypothesis to a controlled quasi-experimental test.

### **THE KANSAS CITY GUN EXPERIMENT**

The experiment developed in 1991 from the first federal grant awarded under the U.S. Bureau of Justice Assistance (BJA) "Weed and Seed" program. The Kansas City (Missouri) Police Department (KCPD) was given wide latitude in planning its Weed and Seed strategy. Shortly after the BJA made the award to the KCPD, the National Institute of Justice (NIJ) awarded the University of Maryland a grant to evaluate the Kansas City effort. This timing allowed the police and researchers to collaborate in planning a focused program with a strong research design.

Because the area already selected by federal officials for the Weed and Seed grant had the second highest number of drive-by

shootings of any patrol beat city wide in 1991, the police and academic team designing the experiment chose reduction of gun crime as the principal objective of the program. The program budget for police overtime and extra patrol cars then was dedicated to removing guns from the street as cost-effectively as possible.

Because the program was restricted to one target patrol beat (#144 in the Central Patrol District), the planning team selected a before-after comparison area design. One can argue that the comparison area design is preferable to a citywide trend comparison because citywide trends are more stable than area-level trends; they average out any sharp differences across areas and therefore create a bias toward showing an effect of treating a single area. Area-level trends, in contrast, are more likely to reflect local changes in gun crime patterns, and are less likely than citywide trends to remain stable simply because of their size. The comparison area chosen was Patrol Beat 242, in the Metro Patrol District. It was selected primarily because it was almost identical to Beat 144 in the drive-by shootings<sup>1</sup> in 1991: 25 drive-bys were reported in Beat 242, compared with 24 in Beat 144. Although no special efforts were made to limit police activities in Beat 242, no funds were available for extra patrol time in that area.

The KCPD attempted three different strategies for increasing gun seizures in Beat 144: door-to-door solicitation of anonymous tips, training police to interpret gun-carrying cues in body language, and field interrogations in gun crime hot spots (Shaw 1994; Sherman, Shaw, and Rogan 1995). Both the federal funds for extra police patrol and the measurable effects were associated entirely with the third strategy, as described in this evaluation.

### *Hypothesis*

According to the theory, if the added patrols increased gun seizures, gun crime would be reduced in turn. We suggested two possible mechanisms: deterrence and incapacitation. The deterrence theory held that if it became known that police were likely to take away a gun, illegal gun carriers would become less likely to carry guns in the area. The incapacitation theory held that if guns were confiscated from enough potential gun criminals in the area, they would be unable to commit gun crimes (incapacitated)—at least for the time required to obtain a new gun. A third theory—

---

<sup>1</sup> As defined by the KCPD Perpetrator Information Center, which classifies drive-by shootings from an ongoing review of incident reports and produces monthly statistics by beat. These statistics are employed in all analyses of those events discussed here.

that increased patrol visibility in the area would generally deter all crime—was also suggested.

Neither of the two primary theoretical mechanisms could be examined directly within the limits of the study, although the third could be tested by measuring non-gun crimes as well as gun crimes. The evaluation focused on the basic hypothesis that *gun seizures and gun crime would be inversely related*. From the outset, the team recognized that support for the hypothesis would not prove that more gun seizures cause reduction of gun crime. The design could not eliminate all competing explanations that could be suggested for the results. Yet if an inverse correlation between gun seizures and gun crime was found, it could suggest the value of further research and development. It could also support a policy of extending the patrols, regardless of the exact mechanism that made them effective.

#### *Design and Experimental Period*

For 29 weeks in 1992-1993 (July 7 to January 27), the Kansas City Police Department focused extra patrol attention on gun crime “hot spots” (Sherman, Gartin, and Buerger 1989) in the target beat. The hot spot locations were identified by University of Maryland computer analysis of all gun crimes in the area.<sup>2</sup> The extra patrol was provided in rotation by officers from Central Patrol Division in a pair of two-officer cars, working on overtime funded by the U.S. Bureau of Justice Assistance Weed and Seed program. The officers on overtime worked from 7 p.m. to 1 a.m. seven days a week. They were asked to focus on gun detection through proactive patrol, and were not required to answer calls for service.

<sup>2</sup> “Gun crime” is defined as any offense report in which the use of a gun by the offender is mentioned. The data presented in this report include the following offense types reported as gun crimes on one or more occasions in either the target or the comparison area during the year before and after the initiation of the hot spot patrols:

| Offense Type                 | Number   |          |
|------------------------------|----------|----------|
|                              | Beat 144 | Beat 242 |
| Criminal Homicide            | 10       | 30       |
| Rape                         | 6        | 5        |
| Armed Robbery and Attempts   | 124      | 222      |
| Aggravated Assault           | 293      | 409      |
| Aggravated Assault on Police | 3        | 1        |
| Burglary                     | 0        | 1        |
| Simple Assault, Gun Pointed  | 1        | 0        |
| Destruction of Property      | 18       | 38       |
| Kidnapping                   | 0        | 1        |
| Casualty Injury, Firearm     | 2        | 4        |
| Suicide and Attempts         | 1        | 1        |
| Totals                       | 468      | 712      |

Although the evaluation concentrated primarily on this first phase of the Weed and Seed grant, additional findings report what happened when the funding of Phase 1 patrols ended (first half of 1993) and when the funding of Phase 2 allowed resumption of the patrols (second half of 1993).<sup>3</sup>

### *Target and Control Areas*

Beat 144, the target beat, is an eight-by-10-block area with a 1991 homicide rate of 177 per 100,000 persons, or about 20 times the national average that year (Shaw 1994: 120). In addition to eight murders in 1991, this beat registered 14 rapes, 72 armed robberies, 222 aggravated assaults (142 with firearms), and a total of 349 violent felonies—close to one a day. Table 1 shows that the population of the beat is almost entirely nonwhite, with very low property values for the predominantly single-family detached homes. The rate of homeownership is very high, over two-thirds of all households.

**Table 1. Characteristics of Target and Comparison Beats**

| Characteristic                  | Target Beat (144) | Comparison Beat (242) |
|---------------------------------|-------------------|-----------------------|
| Population                      | 4,528             | 8,142                 |
| % Female                        | 53                | 56                    |
| % under 25                      | 38                | 41                    |
| Median Age                      | 32                | 31                    |
| % Nonwhite                      | 92                | 85                    |
| % Age 25+ High School Grad.     | 53                | 73                    |
| Residential Square Blocks       | 80                | 150                   |
| Population Density per Mile     | 7,075             | 4,308                 |
| % Single-Family Housing         | 84                | 93                    |
| % Land Parcels Vacant           | 34                | 14                    |
| % Houses Owner-Occupied         | 63                | 71                    |
| Median Years Owned              | 12                | 10                    |
| Median Parcel Value             | \$14,181          | \$23,953              |
| 1991 Firearms-Related Crimes    | 183               | 252                   |
| (per 1,000)                     | 40                | 31                    |
| 1991 Shots Fired Incidents      | 86                | 120                   |
| (per 1,000)                     | 19                | 15                    |
| 1991 Drive-By Shootings         | 24                | 25                    |
| (per 1,000)                     | 5                 | 3                     |
| 1991 Homicides                  | 8                 | 11                    |
| (per 1,000)                     | 1.77              | 1.35                  |
| 7/7/91 to 7/5/93 Gun Crimes     | 468               | 712                   |
| % Gun Crimes Aggravated Assault | 63                | 57                    |

<sup>3</sup> Data on guns seized by beat, however, are available only for the first phase.

Table 1 also shows that the comparison beat, 242, is similar to the target beat in many ways, although hardly a perfect match—a virtual impossibility in this kind of design. The major difference is that Beat 242 has almost twice as much population and three times as much land area, including a park. Housing prices are somewhat higher in the comparison beat. Both beats have substantial volumes of violent crime, which provide a more reliable basis for assessing trends over time than in beats with less crime.

### *Treatment Content and Dosage*

Four officers at a time (volunteering from the entire patrol district) worked six hours of overtime each night (7 p.m. to 1 a.m.) for 176 nights, and only two officers worked on 24 other nights, for a total of 200 nights, 4,512 officer-hours, and 2,256 patrol car-hours of hot spot patrol. Officers reported spending 3.27 car-hours of the 12 car-hours per night actually patrolling the target area (27 percent), for a total of 1,218 officer-hours of potential gun detection and visible patrol presence in the area. The officers thus spent 70 percent of their time in processing arrests and performing other patrol related duties, as well as in patrol work outside the target area.

Despite their limited time in the area, the officers did a great deal. They were not assigned calls for service except in extraordinary circumstances, but they initiated a high volume of contact with the street population. Both inside and outside Beat 144, the directed patrols issued 1,090 traffic citations, conducted 948 car checks and 532 pedestrian checks, and made 170 state or federal arrests and 446 city arrests, for an average of one police intervention for every 40 minutes per patrol car. There is some evidence that activity levels declined somewhat during the period from October to January, as is usually true of street activity in colder weather (Shaw 1994: 243). The average number of car checks made per day, for example, began at its high of 6.5 in July, and declined to its low of 3.2 in November. Time in the target area, miles driven, and traffic citations issued, however, did not change substantially over the first six months.

The actual techniques used by the officers to find the guns varied widely; they included searches incident to arrest on other charges and *Terry v. Ohio* (1968) safety frisks associated with car stops for traffic violations (also see *Pennsylvania v. Mimms* 1977; *Michigan v. Long* 1983). Of the 29 guns seized by officers on hot spot patrols, 10 (34%) were detected during a safety frisk, six (21%) were found in plain view and 13 (45%) were discovered during a



search incident to arrest. Every arrest for carrying concealed weapons had to be approved as to adequate probable cause by signature of a supervisory detective.

The exact nature of the work leading to gun seizures can be illustrated, although not comprehensively, by the following examples:

*Safety frisk during traffic stop:* When the officer asks the driver for his license, the driver leans over to the glove compartment and reveals a bulge under the jacket on the left arm. The officer grabs the bulge, feels a hard bulk in the shape of a gun, and reaches into the jacket to pull the gun out.

*Plain view:* As the officer approaches the car he has stopped for speeding, he shines a flashlight onto the floor in front of the back seat and sees a shotgun. Ordering the driver and passenger out of the car, he finds that the shotgun is loaded.

*Search incident to arrest on other charges:* A driver is stopped for running a red light. The officer asks for the driver's license. A computer check reveals that the driver is wanted for a failure to appear on domestic assault charges. The officer arrests the driver, searches him, and finds a gun hidden inside his shirt.

Many important questions can be asked about these methods and about the "sixth sense" that officers use initially to identify cars and pedestrians to stop. The literature on this question (e.g., Skolnick 1966) tends to stress situational cues out of order, such as a man wearing a heavy coat on a warm day. Yet the wide variety of situational cues that could be "out of order" is virtually impossible to specify. In recent years, for example, General Motors vehicles have been thought more likely to be stolen than others. Thus a GM car with windshield wipers running when there is no rain (suggesting the driver is unfamiliar with the car) can be stopped by an officer on "suspicion." The questions raised most often by critics about these procedures concern the rate of false positives and the potential discrimination entailed in responding to certain patterns of cues. Although such questions are beyond the scope of this study, they merit continuing attention. Yet it is interesting that none of the U.S. Supreme Court decisions even attempt to articulate the substantive bases for police officers' suspicions; they place on the officers the burden of articulating a reasonable basis and implicitly accept as reasonable the facts in the cases cited.

### *Measures*

We measured both police activity and crime. Because the extra patrol hours were federally funded, separate bookkeeping was required to document the time. In addition, the on-site University of

Maryland evaluator accompanied the officers on 300 hours of hot spot patrol and coded every shift activity narrative for patrol time and enforcement inside and outside the area (Shaw 1994). We analyzed property room data on guns seized, as well as computerized crime reports, data on calls for service, and arrest records. We made no attempt to conduct victimization surveys, although other papers (Shaw 1994, 1995) report a before-and-after survey of the target and comparison beats conducted in order to measure residents' perceptions of the program.

We counted gun crimes according to computerized offense records showing that a gun had been used in the crime. Actual discharges of the weapon could be determined only by reading the narrative section of the incident reports for every offense in the target and the comparison beat (Shaw 1994).

### *Analyses*

Building on the precedent of previous studies of gun crime policy impact (Loftin et al. 1991), we examined the data using four different models. The primary analyses assumed that the gun crime counts were independently sampled from the beats examined before and after the intervention. This model treated the before-during difference in the mean weekly rates of gun crimes as an estimate of the magnitude of the effect of the hot spot patrols, and assessed the statistical significance of the differences with standard two-tailed *t*-tests. A second model assumed that the weekly gun crime data points were not independent but were correlated serially, and thus required a Box-Jenkins ARIMA (autoregressive integrated moving average) test of the effect of an abrupt intervention in a time series, computed with BMDP software (BMDP 1990).<sup>4</sup> A third model examined rare events, such as homicide and drive-by shootings, aggregated in six-month totals on the assumption that those counts were independent, using one-way analysis of variance tests. A

---

<sup>4</sup> We identified the model as a "white noise" ARIMA (0,0,0) model by examining the plots of the autocorrelation and partial autocorrelation functions. Because the series spans two years, we examined it carefully for seasonality components. None were found.

We estimated the white noise model using the conditional least squares method. The equation estimated was  $At = 0 + Yt$ , substituting the mean of the series  $At = 4.510 + Yt$  or the dependent variable. A white noise model contains no ARIMA parameters other than the mean of the series. Examination of the plot of residual autocorrelation added support to the white noise interpretation. The Box-Ljung test (LBQ) for 25 lags is .31 and the critical value is 43.38; this test failed to reject the null hypothesis that the cumulative autocorrelation function is 0 in the population and that the model is acceptable.

We then added an intervention component to the model to account for the introduction of the hot spot patrol in July 1992 as the dividing point of the series. This intervention component is a dummy variable coded 0 before the patrols and 1 thereafter. Examination of the combined model reveals a white noise model with the cumulative amount of autocorrelation in the residuals equal to 0.

fourth model also assumed independence of observations, and compared the target with the control beat in a before-during chi-square test.

Displacement tests for gun crime in each contiguous area were conducted with the difference of means, ARIMA, and ANOVA models.

### *Time Periods*

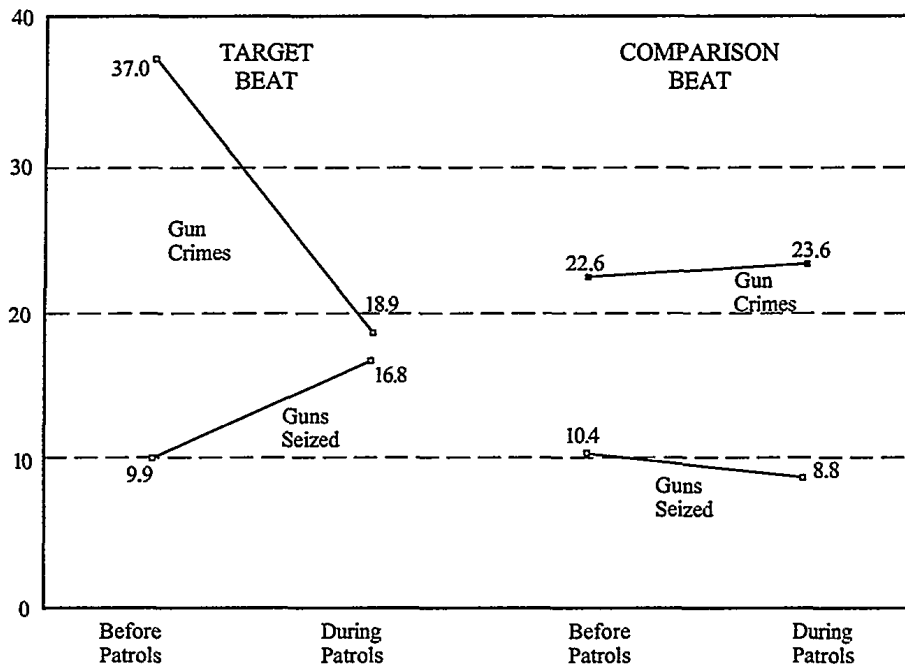
The t-tests compared weekly gun crimes for all 29 weeks of the Phase 1 patrol program (July 7, 1992 through January 25, 1993, when the initial funding for the special patrols expired) with the 29 weeks preceding Phase 1, using difference of means tests. The ARIMA models extended the weekly counts to a full 52 weeks before and after the beginning of Phase 1 patrols in the target area, in order to obtain more reliable estimates of effect size. The ANOVA models added another year before Phase 1 (all of 1991), as well as 1993, the year after the Phase 1 patrols. During that year, hot spot patrols stopped for six months and then were resumed for six months in the second half of 1993, when Phase 2 funding was awarded. These changes offered six periods of six months each; in two of these periods the program was in effect, and in four it was not. We used the chi-square tests to compare 1991-1992 differences in gun crimes for all four quarters, as well as in both half-years, in both target and comparison beats (Shaw 1994).

## RESULTS

### *Gun Seizures*

The federally funded hot spot patrol officers found 29 guns in addition to the 47 guns seized in the target beat by other police units during Phase 1 (second half of 1992), increasing total guns found in the beat by 65 percent over the previous six months and almost tripling the number of guns found in car checks in a 260 percent increase over regular policing activities during the experimental period. Overall there was an increase from 46 guns seized in Beat 144 in the first six months of 1992 to 76 seized in the last six months. The ratio of guns seized to directed patrol time in the experimental period was one gun per 156 hours, but the ratio to time actually spent in the area (and not processing arrests) was one gun per 84 hours and one gun per 28 traffic stops. Figure 1 compares the six-month rates of guns seized per 1,000 resident population in the first and the second half of 1992 in the target and the comparison beat to the respective changes in 29-week rates of gun crimes.

**Figure 1. Firearm Offenses/Guns Seized per 1,000 Persons, First and Second Half of 1992**



Most of the guns were removed permanently from the streets. Not all of the weapons seized were carried illegally; about one-fifth (14) of the total 76 guns taken from target area during Phase 1 (and four of the 29 guns seized by the extra hot spot patrols) were confiscated by police for “safekeeping,” a practice followed in many police agencies when officers have reason to believe that gun violence may occur otherwise. Although guns taken for this reason are usually returned to their rightful owners upon application at the property room, the process can take several days to several weeks. Illegally carried guns, on the other hand, are destroyed by the Kansas City (Mo.) police and are not returned to circulation.

*Trends in Gun Crime*

There were 169 gun crimes in the target beat in the 29 weeks preceding the hot spot patrols, but only 86 gun crimes in the 29 weeks during the Phase 1 patrols—a 49 percent decline, with 83 fewer gun crimes (Table 2). This change was statistically significant in both a test of differences of means (t-test) for that period and in ARIMA (autoregressive integrated moving averages) model covering the longer 52-week before and after period (Table 3).<sup>5</sup>

<sup>5</sup> We had two reasons for extending the ARIMA model to cover 52 weeks before and 52 weeks after the Phase 1 start-up date of July 7, 1992. One is that

**Table 2. Gun Crimes 29 Weeks Before and 29 Weeks During Phase 1 Hot Spots Patrol, by Beat**

| Beat                 | Before<br>12/17/91-7/6/92 | During<br>7/7/92-1/25/93 | % Change     |
|----------------------|---------------------------|--------------------------|--------------|
| Target (144)         | 169                       | 86                       | -49%*        |
| Weekly mean          | 5.83                      | 2.97                     | $t = -3.296$ |
| Standard deviation   | 3.89                      | 2.59                     | $P = .002$   |
| Comparison (242)     | 184                       | 192                      | +4%          |
| Weekly mean          | 6.34                      | 6.62                     | $t = .279$   |
| Standard deviation   | 4.03                      | 3.59                     | $P = .781$   |
| Adjoining Target     |                           |                          |              |
| 141                  | 76                        | 57                       | -25%         |
| Weekly mean          | 2.62                      | 1.97                     | $t = -.972$  |
| Standard deviation   | 3.09                      | 1.85                     | $P = .335$   |
| 142                  | 106                       | 84                       | -21%         |
| Weekly mean          | 3.66                      | 2.90                     | $t = -1.160$ |
| Standard deviation   | 2.56                      | 2.43                     | $P = .251$   |
| 143                  | 39                        | 44                       | +13%         |
| Weekly mean          | 1.34                      | 1.52                     | $t = .415$   |
| Standard deviation   | 1.97                      | 1.25                     | $P = .679$   |
| 213                  | 143                       | 158                      | +10%         |
| Weekly mean          | 4.93                      | 5.45                     | $t = .475$   |
| Standard deviation   | 4.46                      | 3.86                     | $P = .637$   |
| 214                  | 104                       | 138                      | +33%         |
| Weekly mean          | 3.59                      | 4.76                     | $t = 1.487$  |
| Standard deviation   | 2.17                      | 3.64                     | $P = .143$   |
| 331                  | 143                       | 175                      | +22%         |
| Weekly mean          | 4.93                      | 6.03                     | $t = 1.252$  |
| Standard deviation   | 3.08                      | 3.59                     | $P = .216$   |
| 332                  | 153                       | 160                      | +5%          |
| Weekly mean          | 5.28                      | 5.52                     | $t = .166$   |
| Standard deviation   | 4.19                      | 6.58                     | $P = .869$   |
| All Contiguous Beats | 764                       | 816                      | +7%          |
| Weekly mean          | 26.34                     | 28.14                    | $t = .62$    |
| Standard deviation   | 10.11                     | 11.78                    | $P = .537$   |
| All Kansas City      | 4,359                     | 4,287                    | -2%          |
| Weekly mean          | 150.31                    | 147.83                   | $t = -.386$  |
| Standard deviation   | 23.82                     | 25.08                    | $P = .701$   |

\*  $t$  value  $P < .05$ .

Comparison Beat 242 showed a slight reduction in guns seized, from 85 in the first half of 1992 to 72 in the second half. It also

ARIMA models generally require more data points than the 29 actual program weeks to produce more reliable estimates and to eliminate factors such as seasonality. This is true even though there is no specific minimum requirement. The other reason is that according to police crackdown theory (Sherman 1990), we predicted that the effects of the hot spot patrols would linger as a form of residual deterrence even after they ceased.

**Table 3. ARIMA Parameter Estimates, Gun Crimes 52 Weeks Before and 52 Weeks After Phase 1 Hot Spots Patrol Began, by Beat**

| Patrol Beat    | Box-Jenkins Estimate | Standard Error | T-Ratio |
|----------------|----------------------|----------------|---------|
| 144            | 5.788                | .433           | 13.36   |
| Impact         | -2.558               | .613           | -4.17*  |
| 242            | 7.154                | .567           | 12.62   |
| Impact         | -.751                | .080           | -.94    |
| 141            | 2.692                | .339           | 7.95    |
| Impact         | -.981                | .479           | -2.05*  |
| 142            | 3.250                | .311           | 10.46   |
| Impact         | -.539                | .440           | -1.22   |
| 143            | 2.135                | .250           | 8.55    |
| Impact         | -.692                | .353           | -1.96*  |
| 213            | 4.442                | .547           | 8.12    |
| Impact         | .500                 | .773           | .65     |
| 214            | 4.308                | .418           | 10.31   |
| Impact         | -.192                | .591           | -.33    |
| 331            | 5.096                | .451           | 11.29   |
| Impact         | .346                 | .639           | .54     |
| 332            | 5.847                | .730           | 8.01    |
| Impact         | -1.021               | 1.032          | -.99    |
| All Contiguous | 27.777               | 1.375          | 20.19   |
| Impact         | -2.577               | 1.945          | -1.32   |

\*  $P < .05$ .

showed a slight increase in gun crimes, from 184 in the 29 weeks before the program to 192 in the 29 weeks during the program (Table 2). Neither change was statistically significant, in either 29-week t-tests or the 52-week ARIMA model. Shaw's (1994) analysis of incidents in which shots were fired shows very similar results, confirming the computerized KCPD designation of gun crimes with Shaw's qualitative review of incident report narratives.

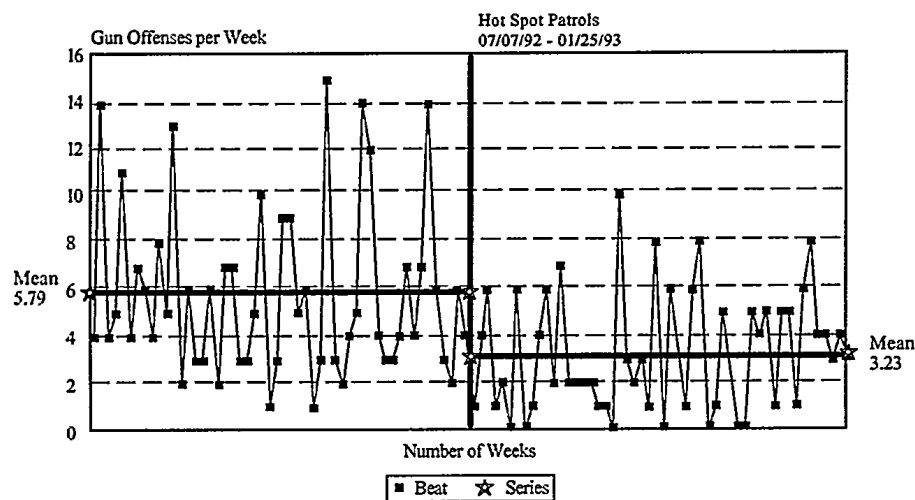
### *Displacement*

Although gun crime dropped in Beat 144, none of the seven contiguous beats registered any significant increases in gun crime, as shown in Table 2 for the 29-week before-and-after tests and in Table 3 for the 52-week ARIMA models. Rather than displacing gun crime, some evidence even suggests that the program's benefits were diffused to two of the adjoining beats. The 52-week before-and-after special tests (ARIMA models) showed significant reductions in gun crimes in Beats 141 and 143.

The only suggestion of displacement is that the sum of changes in gun crime across all seven contiguous beats shows a nonsignificant increase of 52 gun crimes, 7 percent over the “before” period. Although gun crime may have been partially displaced, the displacement was dispersed so well across the contiguous beats that it was not noticeable in any one beat. Even if this were the case, Barr and Pease (1990) have argued that such dispersion is actually a social benefit because high risks of crime are not concentrated on a small number of potential victims. Dispersion helps to make everyone aware of the costs of crime, and may even increase support for public policies that might help prevent crime.

The most likely interpretation, however, is that the small increase in gun crime in the total contiguous areas was due to chance. The two-tailed t-test reported in Table 2 shows a 54 percent likelihood that the 29-week increase was the result of chance. The 52-week before-and-after ARIMA model reported in Table 3 shows a decrease in gun crime in the aggregated contiguous beats, further falsifying the displacement hypothesis, while the target area shows the overall decrease illustrated in Figure 2.

**Figure 2. Plot of Gun Offenses per Week, 52 Weeks Before and After Hot Spot Patrols, Beat 144 (Experimental Beat)**



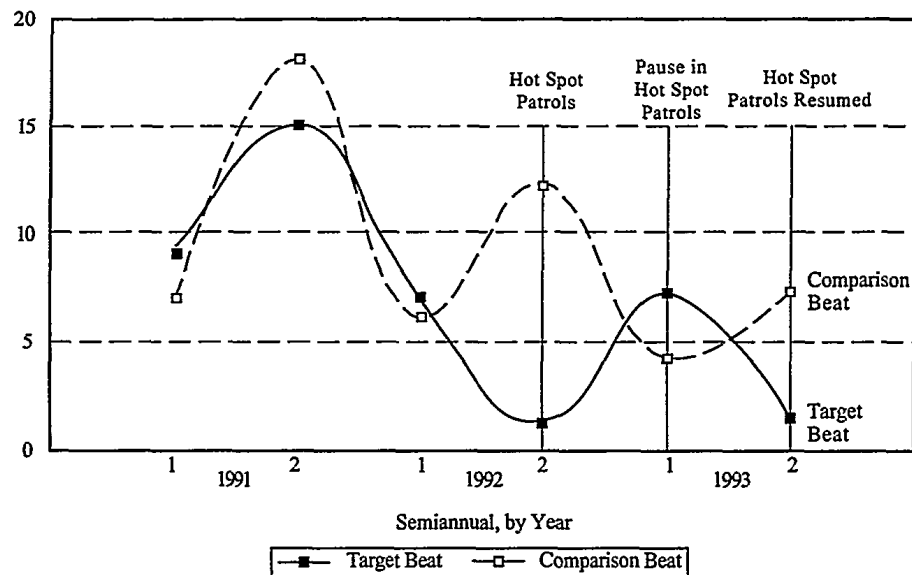
Source: Kansas City Police Offense Reports

*Crime-Specific Effects*

*Drive-by Shootings.* Analysis of variance of six-month totals for drive-by shootings showed that these shootings declined during both six-month periods of hot spot patrols (second half of 1992 and of 1993) in comparison to the periods without patrols ( $F = 4.8214, p$

= .079). Beat 242 showed no such difference ( $F = .1383$ ,  $p = .725$ ). The same analyses showed no significant differences in the beats surrounding Beat 144 ( $p < .10$ ). Figure 3 displays the target and comparison area trends in drive-by shootings, as compiled by the KCPD Perpetrator Information Center.<sup>6</sup>

**Figure 3. Number of Drive-By Shootings (6-Month Aggregates)**



*Homicides.* Homicides also were significantly lower in Beat 144 during the two six-month program periods than in other half-years from 1991 through 1993 (Mantel-Haenszel chi-square = 4.65,  $P = .03$ ). We found no significant differences in homicide across those periods in comparison Beat 242 (Mantel-Haenszel chi-square = .36,  $P = .54$ ) or in any of the contiguous beats ( $P < .05$ , although  $P < .10$  in three of the seven).

*Other crimes.* Neither total calls for police service, calls about violence, property, or disorder crimes, total offense reports, nor property or violent offenses showed any effect of the increased patrol (data are not displayed). We found no significant changes in t-tests of these measures in either the target or the comparison area. The target area hot spot patrols focused primarily on guns, and their effects were limited to gun crimes. Thus, although we cannot say whether the effects were due to crime-specific deterrence or to incapacitation through loss of guns, we can refute the hypothesis of general deterrence due to more visible patrol presence.

<sup>6</sup> KCPD data on drive-by shootings in beats contiguous to the target beat are not displayed, but are available from the authors on request.

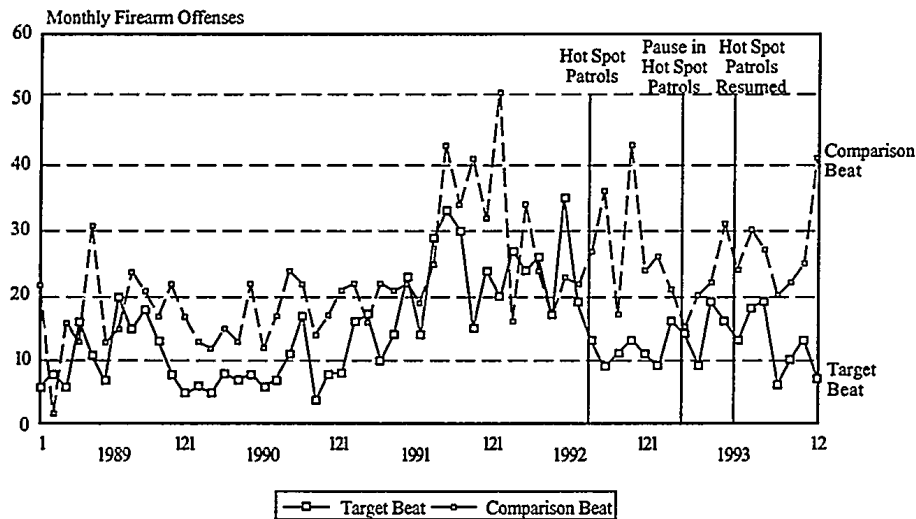


*Community attitudes.* Community surveys before and after the intensive patrols showed that respondents in target Beat 144 became less fearful of crime and more satisfied with their neighborhood than respondents in comparison Beat 242 (Shaw 1994, 1995). Target beat respondents were only marginally more likely to say that the shooting problem had improved and were no more likely to report improvement in overall crime problems, but they were significantly more likely than comparison beat respondents to say that neighborhood drug problems had improved.

#### *Effects of Program Cessation and Resumption*

Crimes involving firearms gradually increased again for five months in the first half of 1993 (see Figure 4), in keeping with the usual pattern of police crackdowns (Sherman 1990). After the program resumed in July 1993, however, gun crimes declined in the target area while they rose in the comparison area.

**Figure 4. Total Offenses with Firearms, by Month, Target and Comparison Beats**



## DISCUSSION

Why should seizing 29 more guns in 29 weeks make any difference in gun crime in an 8-by-10-block area? If we assume that there are tens of thousands of handguns in Kansas City,<sup>7</sup> the

<sup>7</sup> Kleck (1991:18) estimates that there are at least 65 million handguns in the United States, with a population of more than 250 million people. A rough application of this ratio to the Kansas City population of some 400,000 people suggests the presence of at least 100,000 handguns in that city.

seizure of 29 handguns might be viewed as a drop in the bucket, an implausible reason for any significant reduction in gun crime. At least three plausible theories, however, explain how the patrols may have caused a reduction in gun crime: high-risk guns, high-risk offenders, and deterrence.

Wilson (1994) argues that most guns are not at immediate risk of being used in crime. Guns seized by police in areas of high gun crime at times of high crime may be at far greater risk of imminent use in crimes than the average handgun. Cook (cited in Reiss and Roth 1993:282) estimates that for each new cohort of 100 guns, 33 uses of those guns in crime are reported. Those uses could be concentrated heavily in the small fraction of that cohort which is carried in gun crime hot spots. In those areas, each gun could be used in hundreds of crimes especially in light of anecdotal evidence about gun loaning and renting.

Even so, criminals may easily replace guns seized by police. Connecting the 29 guns seized to the 83 gun crimes prevented may thus require a further assumption that gun crime is more opportunistic than planned, and is relatively infrequent in any criminal's career. The high proportion of gun crime consisting of aggravated assaults in both the target and the comparison areas, as noted above, is consistent with this argument.

Some gun carriers may be far more frequent gun users than others. If even 10 percent of the 170 state and federal arrests made by the directed patrols captured high-frequency gun users, and if the arrestees spent the next six months in jail on serious charges from outstanding warrants, then the incapacitation of those 17 offenders by the program may have prevented 83 gun crimes for a not implausible average of five gun offenses each, or less than one per month. This effect would not be tied directly to gun seizures, but it may well be an important component of targeting hot spot patrols in an area of high gun crime.

Deterrence of gun carrying may be an even more plausible explanation of reduced gun crime. The 29 extra gun seizures, the 1,434 traffic and pedestrian stops, or the total of 3,186 arrests, traffic citations, and other police encounters could have specifically deterred potential gun criminals who encountered police. Visibility of police encounters in the hot spots also may have deterred gun carrying as a general effect among those who were not checked by police. This argument appears at least plausible enough to suggest that directed patrols can reduce gun crime, regardless of the exact theoretical mechanism. It is not clear, however, whether this explanation is more plausible than attributing the reduction in gun crime to a handful of high-risk offenders who were kept off the

streets or at least away from gun crimes by the temporary loss of their guns.

Much is still to be learned about the entire process of gun detection and seizure by police. Until recently, neither police administrators nor researchers showed a strong interest in understanding or encouraging the factors leading to gun seizures. We know little about differences across police agencies or police officers as to their respective rates of gun detection. We do not even know how many more guns could be detected if patrol officers generally received more direction and training in locating guns in the course of their routine activities. We do know from Kansas City that a focus on gun detection, with freedom from answering calls for service, can make regular beat officers working overtime very productive.

### *Reducing Gun Crime*

Now that we know how to increase gun seizures in target areas, the key question is whether that policy will reduce gun crime without total displacement. The Kansas City evidence suggests that it can reduce gun crime without *local* displacement. Only repeated tests of the hypothesis, however, will show whether the policy can produce that result predictably. Even before replications are completed, many cities must decide how to respond to increasing gun crime. Although some might counsel taking no action until more research is completed, it is not certain that inaction is safer. Just as much harm may be done and just as many lives may be lost by waiting for more research as by acting prematurely.

This situation is analogous to the problem of domestic violence arrests in 1984, when an unreplicated experiment suggested that arrest was an effective deterrent (Sherman and Berk 1984). Replications completed eight years later showed that the effects of arrest varied across cities and between employed and unemployed suspects, deterring violence in some instances but increasing it in others (Sherman 1992). Even before these later findings were reported, some scholars have argued that unreplicated experiments with major policy implications should not even be published (Binder and Meeker 1988; Lempert 1989). We offer the present unreplicated quasi-experiment for publication in the belief that police agencies, not scholars, are most able to judge the policy implications of these findings.

### *Cautions*

Nonetheless, we offer our findings with appropriate cautions. Intensified gun patrols in some neighborhoods conceivably could harm police-community relations (but see Shaw 1995), even though

no complaints and no legal challenges to the experiment were ever filed in Kansas City. Such efforts at least may waste time and money, especially if they do not reach any minimum threshold of detection probability. Gun hot spot patrols could pose great risks to officers' safety, although no officers were injured in the limited period of the experiment. Most worrisome is the possibility that field interrogations could provoke more crime by making young men subjected to traffic stops more defiant toward conventional society (Sherman 1993) and thus commit more crimes.

All of these hazards are possible but unknown. The trade-off is the well-known risk of gun violence, which is extremely high and is still rising in many inner cities. Firearms crimes in Indianapolis, for example, increased by 220 percent from 1988 to 1993 (also see Rand 1994). The need for action makes the central questions for further research all the more urgent:

1. Can other cities obtain similar results with similar methods in similar target areas?
2. Can cities without federal funding obtain similar results with far fewer patrol hours in the target area?
3. Can city-wide programs obtain the same results Kansas City obtained in one small area, or would displacement erode the apparent benefit?

Answering these questions through further research will require the support of many public officials, but the evidence suggests that support is well-justified.

## REFERENCES

- Barr, R. and K. Pease (1990) "Crime Placement, Displacement and Deflection." In M. Tonry and N. Morris, (eds, *Crime and Justice: A Review of Research, Vol. 12*, pp.277-318. Chicago: University of Chicago Press.
- Binder, A. and J. Meeker (1988) "Experiments as Reforms." *Journal of Criminal Justice* 16:347-58.
- BMDP (1990) *BMDP2T—Box-Jenkins Time-Series Analysis*. Los Angeles: BMDP Statistical Software, Inc.
- Brill, S. (1977) *Firearm Abuse: A Research and Policy Report*. Washington, DC: Police Foundation.
- Cook, P. (1991). "The Technology of Personal Violence." In M. Tonry (ed.), *Crime and Justice: A Review of Research, Vol. 14*, pp.1-72. Chicago: University of Chicago Press.
- Felson, M. (1994) *Crime and Everyday Life*. Thousand Oaks, CA: Pine Forge Press.
- Kleck, G. (1991) *Point Blank: Guns and Violence in America*. New York: Aldine.
- Lempert, T. (1989) "Humility Is a Virtue: On the Publicization of Policy-Relevant Research." *Law and Society Review* 16:513-68.
- Lewis, N. (1995) "Court Cases Reveal Arms Buildup among D.C. Youths." *Washington Post*, January 1, p. A1.
- Loftin, C., D., McDowall, B. Wiersema, and T. J. Cottey (1991) "Effects of Restrictive Licensing of Handguns on Homicide and Suicide in the District of Columbia." *New England Journal of Medicine* 32:1615-20.

- McDowall, D. (1991) "Firearm Availability and Homicide Rates in Detroit, 1951-1986." *Social Forces* 69:1085-1101.
- Moore, M. (1980) "The Police and Weapons Offenses." *Annals of the American Academy of Political and Social Sciences* 452:22-32.
- Pierce, G. and W. Bowers (1979) "The Impact of the Bartley-Fox Gun Law on Crime in Massachusetts." Unpublished manuscript, Northeastern University, Center for Applied Social Research.
- Rand, M. (1994) *Guns and Crime*. Washington, DC: Bureau of Justice Statistics.
- Reiss, A.J., Jr. and J.A. Roth, eds. (1993) *Understanding and Preventing Violence*. Washington, DC: National Academy of Sciences.
- Shaw, J.W. (1994) "Community Policing Against Crime: Violence and Firearms." Doctoral dissertation, University of Maryland, College Park, Department of Criminology and Criminal Justice.
- (1995) "Community Policing against Guns: Public Opinion of the Kansas City Gun Experiment" *Justice Quarterly*, this issue.
- Sherman, L.W. (1990) "Police Crackdowns: Initial and Residual Deterrence." In M. Tonry and N. Morris (eds.) *Crime and Justice: A Review of Research*, Vol. 12. pp.1-48. Chicago: University of Chicago Press.
- (1992) *Policing Domestic Violence: Experiments and Dilemmas*. New York: Free Press.
- (1993) "Defiance, Deterrence, and Irrelevance." *Journal of Research in Crime and Delinquency* 30:445-73
- (1995) "Carrying Concealed Weapons: Community Policing against Guns." NIJ research in progress, presented to the U.S. Attorney General, April.
- Sherman, L.W. and R.A. Berk (1984). "The Specific Deterrent Effects of Arrest for Domestic Assault." *American Sociological Review* 49:261-72.
- Sherman, L.W., P.R. Gartin, and M.E. Buerger (1989) "Hot Spots of Predatory Crime: Routine Activities and the Criminology of Place." *Criminology* 27:27-55.
- Sherman, L.W. and C. Bridgeforth (1994) *Getting Guns off the Streets, 1993: A survey of Big-City Police Agencies*. Washington, DC: Crime Control Institute.
- Sherman, L.W., J.W. Shaw, and D.P. Rogan (1995) *The Kansas City Gun Experiment*. Washington, DC: National Institute of Justice.
- Wilson, J.Q. (1994) "Just Take Away Their Guns: Forget Gun Control." *New York Times Magazine*, March 20, pp. 46-47.

### CASES CITED

- Michigan v. Long* 436 v.s. 1032 (1983)  
*Pennsylvania v. Mimms* 434 U.S. 106 (1977)  
*Terry v. Ohio* 392 U.S. 1 (1968)

