

Police crackdowns on illegal gun carrying: a systematic review of their impact on gun crime

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Abstract. This paper presents a systematic review of the impact of police strategies to reduce illegal possession and carrying of firearms on gun crime, including directed patrols, monitoring of probationers and parolees, weapon reporting hotlines, and others. Four studies met the inclusion criteria, reporting a total of seven nonrandomized tests of directed patrols focused on gun carrying in three American cities (five tests) and two Colombian cities (two tests). Six of the seven tests (not all of which were independent) suggest that directed patrols reduced gun crime in high-crime places at high-risk times. However, conclusions and generalizations must be qualified based on the small number of studies, variability in study design and analytic strategy across the studies, preintervention differences between intervention and comparison areas, and limited data regarding factors such as implementation, crime displacement, and long-term impact.

Key words: Campbell Collaboration, crackdowns, firearms, guns, patrol, policing, systematic review, violence

Introduction

Criminal misuse of firearms is among the world's most serious crime problems. In the United States, for example, there were roughly 11,000 murders with firearms in 2003 (calculated from *Crime in the United States, 2003*, Federal Bureau of Investigation 2004, p. 19) and 367,000 nonfatal violent crimes with guns (Catalano 2004, p. 10). Violent crimes with guns are about three times as likely to be deadly as crimes committed with knives and nearly 44 times as likely to be deadly as crimes involving no weapons (Alba and Messner 1995, pp. 397–402; see also Cook 1991; Zimring 1968). The prevalence of guns may contribute to particularly high levels of homicide in the United States (e.g., Hoskins 2001; Zimring and Hawkins

Although the protocol for this review was registered and approved by the Campbell Collaboration Crime and Justice Group (<http://www.campbellcollaboration.org/CCJG/>), this paper represents an independent effort of the authors and has not been reviewed by the Campbell Collaboration. Evan Mayo-Wilson conducted research for this project while he was Coordinator of the Campbell Crime and Justice Group at the University of Pennsylvania's Jerry Lee Center of Criminology and a student at the Fels Institute of Government.

1997), where the total costs of gun violence—including medical, criminal justice, and other costs—are at least \$6–12 billion per year and, by more controversial estimates, could be as high as \$80 billion per year (Cook and Ludwig 2000).

Nations such as Colombia, Brazil, Mexico, and South Africa have firearm homicide rates that exceed those of the United States (Krug et al. 1998; United Nations 1997; Villaveces et al. 2000). Guns are also involved in roughly a quarter to a third of homicides in a number of countries not known for having serious gun violence problems, including Canada, France, Israel, the Netherlands, Norway, and New Zealand (calculated from Fingerhut et al. 1998, p. 18). In England and Wales, where gun possession is strictly regulated relative to the United States, handgun crimes increased 50% between 1998 and 2001 (Travis 2003).

Strategies for reducing gun violence range from restrictions on the manufacture and sale of firearms to educational efforts that promote safe storage and use of firearms or that discourage firearm ownership. In this paper, we review research on the effectiveness of selected law enforcement strategies for reducing gun crime and gun violence.

Background: law enforcement strategies for reducing gun violence

Strategies to reduce gun violence may attempt to interrupt the illegal supply of guns, deter gun possession, reduce gun carrying in public places, toughen responses to illegal gun use, reduce demand for firearms, promote responsible ownership of guns, and address community conditions that foster gun crime (e.g., see Center to Prevent Handgun Violence 1998; Office of Juvenile Justice and Delinquency Prevention 1999). Law enforcement agencies are integral in all of these domains.

Police typically handle gun crimes reactively, investigating violent gun crimes and making arrests for illegal possession or carrying when they encounter violations during routine activities. To varying degrees, police also use proactive strategies to reduce gun crime. Among others, these include disrupting the illegal supply of firearms through investigation of illicit gun trafficking, gun theft, and suspicious activities by retail gun dealers; focusing intensive investigative and enforcement activities on violent gun offenders and people at high risk for gun violence (such as gang members and career gun offenders); implementing educational and preventive activities in conjunction with schools and other community groups (for example, teaching students about gun safety); and collaborating with other criminal justice, government, and community organizations on comprehensive initiatives that combine various enforcement, prosecutorial, and prevention activities.

This review examines evidence for law enforcement strategies that aim to reduce illegal gun possession and carrying through gun detection patrols in high-crime areas, enhanced surveillance of probationers and parolees, weapon reporting hotlines, consent searches, and other tactics.¹ In the United States, gun possession is common among persons prohibited from lawful gun ownership, including those with prior convictions for serious crimes. Nationally, over 80% of incarcerated gun offenders appear to have possessed guns illegally prior to confinement; more than a

third were already on probation or parole when they were arrested for a gun crime (Harlow 2001, p. 10).²

In the United States, many juveniles (i.e., those under the age of 18) also possess guns despite various legal restrictions on their access to firearms. A survey of incarcerated juvenile offenders and inner city high school students in four states during the early 1990s found that 83% of the inmates possessed guns prior to confinement, as did 22% of the students (Sheley and Wright 1993, p. 4). Persons under the age of 18 committed 16% of gun murders in the United States from 1990 to 1997 (calculated from Fox and Zawitz 1998).

Carrying of firearms is central to the commission of gun crimes in public locations, which is where many violent and predatory crimes occur. Almost all gun robberies, for example, are likely to involve gun carrying. In the U.S. city of Philadelphia, 76% of homicides from 1996 through 1999 occurred in a nonresidential location, and 80% were committed with guns; this implies that many, if not most, homicides were committed by offenders carrying firearms in public places (Tierney et al. 2001). Despite legal restrictions on gun carrying, survey evidence suggests that 30–55% of serious adult and juvenile offenders in the United States carry guns regularly for defense and to be prepared for criminal opportunities (Sheley and Wright 1993, p. 5; Wright and Rossi 1986, pp. 99–102). For all of these reasons, police strategies to reduce illegal possession and carrying of firearms are important for preventing gun violence.

Methodology

This paper is drawn from a review undertaken for the Crime and Justice Group of the Campbell Collaboration, an international nonprofit organization that coordinates rigorous assessments of research evidence on the effectiveness of various interventions for social problems (Farrington and Petrosino 2001; Petrosino et al. 2001). Campbell reviews are conducted using systematic methods. Compared with standard literature reviews, systematic reviews use more rigorous and formal methods for identifying, screening, evaluating, and synthesizing studies. Systematic reviews are characterized by explicit objectives and eligibility criteria, search methods that minimize bias (e.g., searching for published and unpublished studies), collection of detailed data from eligible studies, quantitative analysis of data where appropriate, and detailed presentation of procedures and results (Farrington and Petrosino 2001). In the sections below, we discuss our criteria for selecting studies, our search strategy, and our methods for data collection and synthesis.³

Criteria for inclusion of studies

Types of interventions

This review sought to examine evidence on a range of police interventions to reduce the illegal possession and carrying of firearms, including the following: directed or saturation patrols; roadblock checkpoints; enhanced monitoring of

probationers, parolees, and other suspected gun offenders; use of new gun (and gunshot) detection technologies (such as portable, magnetic gun detection devices)⁴; weapon reporting hotlines; searches of school lockers; and zero tolerance/crackdown initiatives. Interventions had to represent departures from normal practice (for instance, instituting new gun detection patrols).

To isolate the effects of these tactics, the review does not include studies in which these interventions were implemented simultaneously with other new crime reduction efforts, whether by police or other organizations (e.g., Braga et al. 2001). Further, because the review emphasizes police action, we excluded legislative, prosecutorial, and judicial initiatives to enhance penalties for gun possession and carrying (i.e., efforts to increase the severity of punishment for these offenses).⁵ Finally, the review does not include studies of gun buy-back programs. Although these programs encourage illegal gun possessors to relinquish their firearms, they function more broadly as supply-side efforts to reduce the availability of guns in high-risk areas (e.g., see National Research Council 2005, pp. 95–96).

Study design

Campbell reviews are intended to provide the most rigorous assessment possible of the effectiveness of studied interventions (Farrington and Petrosino 2001; Petrosino et al. 2001). Accordingly, they generally focus on randomized experiments or nonrandomized quasi-experiments, particularly those involving a control group (Campbell and Stanley 1966). Such designs present fewer threats to a study's internal validity than do other methods of assessing the effects of interventions, such as correlational studies or before-and-after comparisons, which are not included in Campbell reviews.

For this review, studies using randomized designs or designs involving a nonintervention condition were eligible for inclusion. Eligible studies had to include pre- and postintervention measurements of the outcome measure(s) for an intervention area(s) or group(s) and at least one comparison area or group without the intervention. However, we also included studies involving repeated interventions with one group or area in which the intervention and comparison units consisted of samples of time with and without the intervention (the time-equivalent samples design; Campbell and Stanley 1966). Studies not utilizing random assignment had to have comparison units of the same type (e.g., patrol zones or neighborhoods), preferably matched to the intervention units on relevant characteristics, including preintervention crime rates. If the comparison and intervention units were not matched, the studies had to include statistical controls to account for crime-related differences in the units.⁶

Outcome measures

Eligible studies had to measure gun-related crime (e.g., murders, shootings, gun robberies, gun assaults). Arrests for illegal possession and carrying of guns were not analyzed as outcomes because they were considered measures of program implementation.⁷

Search strategy

The following 14 national and international databases were searched for published and unpublished literature:

- Criminal Justice Abstracts
- National Criminal Justice Reference Service Abstracts Database
- Sociological Abstracts
- Medline
- Dissertation Abstracts
- U.S. Government Printing Office Monthly Catalog
- Legal Resource Index
- Educational Resources Information Clearinghouse
- United States Department of Housing and Urban Development Bibliographic Database
- International Bibliography of the Social Sciences
- Current Index of Urban Documents
- Policyfile
- Public Affairs Information Service International
- TRANweb (Transportation Article File)

In addition, searches were conducted on the Web sites of four prominent police and criminal justice organizations in the United States: the Police Executive Research Forum, the International Association of Chiefs of Police, the Office of Community Oriented Policing Services (United States Department of Justice), and the Justice Research and Statistics Association.⁸ Finally, we examined numerous reviews of research on policing, gun control, and violence reduction (Braga 2001; Centers for Disease Control and Prevention 2003; Center to Prevent Handgun Violence 1998; Cook and Moore 1995; Eck and Maguire 2000; Harcourt 2003; Jacobs 2002; Kleck 1997; Ludwig and Cook 2003; National Institutes of Health 2004; National Research Council 2004, 2005; Office of Juvenile Justice and Delinquency Prevention 1999; Reiss and Roth 1993; Sherman 1990, 1992, 1997, 2001; Wintemute 2000; Wright et al. 1983).

Data management and extraction

From each included study, both authors extracted data pertaining to research design, subject characteristics, intervention(s), and outcome measure(s). Tables 1, 2, and 3 summarize these features.

Data synthesis

In the sections below, we present a narrative summary and assessment of each included study. We did not conduct a statistical meta-analysis of the results for reasons considered in the [Discussion](#) section.

Table 1. Studies of directed patrol and gun crime: research design and subject characteristics.

Study	Location	Study Design	Target Area	Target Characteristics	Comparison	Comparison Characteristics
Sherman and Rogan (1995)	Kansas City (USA)	Nonequivalent control group design (matched comparison group), interrupted time series for each area	Patrol beat: 80 square blocks, 4,528 residents	1.77 homicides and 40 gun crimes per 1,000 residents, 92% nonwhite, median parcel value = \$14,181	Patrol beat: 150 square blocks, 8,142 residents	1.35 homicides and 31 gun crimes per 1,000 residents, 85% nonwhite, median parcel value = \$23,953
McGarrell et al. (2000)	North district, Indianapolis (USA)	Nonequivalent control group design (matched comparison group), interrupted time series for each area	Two patrol beats: 2.79 square miles, 16,612 residents	0.9 murders and 33.8 violent crimes per 1,000 residents, 88% black, 38% income <\$10,000	Two patrol beats: 4.74 square miles, 19,305 residents (same comparison used for East)	0.5 murders and 22.5 violent crimes per 1,000 residents, 86% black, 25% income <\$10,000
McGarrell et al. (2000)	East district, Indianapolis (USA)	Nonequivalent control group design (matched comparison group), interrupted time series for each area	Two patrol beats: 1.69 square miles, 14,645 residents	0.5 murders and 38.4 violent crimes per 1,000 residents, majority white (14% black), 30% income <\$10,000	Two patrol beats: 4.74 square miles, 19,305 residents (same comparison used for North)	0.5 murders and 22.5 violent crimes per 1,000 residents, 86% black, 25% income <\$10,000
Cohen and Ludwig (2003)	Zone 1, Pittsburgh (USA)	Multiple time series analysis with nonmatched areas	Patrol zone: 8.9 square miles, 54,595 residents	0.75 shots-fired calls and 0.155 gunshot injuries per day (average for target areas), 29.6% black, 23.4% poverty	Other four patrol zones: 27,782–86,002 residents, 3.8–12.9 square miles (same as comparisons for zone 5)	0.274 shots-fired calls and 0.054 gunshot injuries per day (average for comparison areas), 8.4–64.5% black, 13.6–42.5% poverty

Cohen and Ludwig (2003)	Zone 5, Pittsburgh (USA)	Multiple time series analysis with nonmatched areas	Patrol zone: 9.4 square miles, 79,797 residents	0.75 shots-fired calls and 0.155 gunshot injuries per day (average for target areas), 48% black, 24.5% poverty	Other four patrol zones: 27,782–86,002 residents, 3.8–12.9 square miles (same as comparisons for zone 1)	0.274 shots-fired calls and 0.054 gunshot injuries per day (average for comparison areas), 8.4–64.5% black, 13.6–42.5% poverty
Villaveces et al. (2000)	Bogotá (Colombia)	Interrupted time series with multiple replications (not conducted at random intervals)	City: 5,639,328 residents	68 homicides per 100,000 person-years; high-risk days selected for intervention	Days without intervention	Lower-risk periods
Villaveces et al. (2000)	Cali (Colombia)	Interrupted time series with multiple replications (not conducted at random intervals)	City: 1,803,662 residents	124 homicides per 100,000 person-years; high-risk days selected for intervention	Days without intervention	Lower-risk periods

Table 2. Studies of directed patrol and gun crime: implementation measures.

<i>Location</i>	<i>Nature of Intervention</i>	<i>Period</i>	<i>Times</i>	<i>Officers</i>	<i>Officer Hours</i>	<i>Vehicle Stops</i>	<i>Person Contacts</i>	<i>Arrests</i>	<i>Gun Seizures</i>	<i>Other Activities</i>
Beat 144, Kansas City (USA)	1- or 2-car nightly gun patrols; officers sought to maximize visibility and personal contacts	7 days a week over 29 weeks (Jul. 1992–Jan. 1993)	1900–0100	4	4,512	1,090 traffic citations; 948 car checks	532 pedestrian checks	616	29 by patrols, 79 total; 65% over baseline	Door-to-door visits to residents, anonymous tips hotline
North district, Indianapolis (USA)	Gun patrols targeting specific times, locations, and suspicious persons	90 days (Jul. 1997–Oct. 1997)	Not reported	Not reported	1,975	1,417	Not reported	434	12 by patrols, 42 total; 8% over baseline	Community meetings, 126 probation checks conducted
East district, Indianapolis (USA)	Gun patrols using general deterrence strategy by maximizing vehicle stops	90 days (Jul. 1997–Oct. 1997)	Not reported	Not reported	2,905	3,836	Not reported	558	13 by patrols, 45 total; 50% over baseline	Community meetings, similar intervention in the year and a half prior
Zone 1, Pittsburgh (USA)	3-car nightly gun patrols during Wed.–Sat. portion of week	Twice weekly for 14 weeks (Jul. 1998–Oct. 1998)	2000–2400	5	About 500	12	57	6	2 by patrols	None reported

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Zone 5, Pittsburgh (USA)	3-car nightly gun patrols during Wed.-Sat. portion of week	Twice weekly for 14 weeks (Jul. 1998–Oct. 1998)	2000–2400	5	About 500	27	118	12	5 by patrols	None reported
Bogotá, Colombia	Gun carrying banned on particular weekends, holidays, election days; checkpoints, searches, traffic stops, and other activities	22 occasions covering 67 days from Dec. 1995–Apr. 1997	1800 day before until 0600 day after	Not reported	Not reported	Not reported	Not reported	Not reported	Not reported	Media announcements
Cali, Colombia	Gun carrying banned on particular weekends, holidays, election days; checkpoints, searches, traffic stops, and other activities	34 occasions covering 89 days from Nov. 1993–Dec. 1994	1800 day before until 0600 day after	Not reported	Not reported	Not reported	Not reported	Not reported	307 (4 per day); 400% over rate on nonban days	Media announcements

Table 3. Studies of directed patrol and gun crime: outcome measures, statistical analyses, and results.

Location	Outcomes	Statistical Analyses	Primary Results	Displacement
Patrol beat 144, Kansas City (USA)	Gun crimes	Separate <i>t</i> -tests of mean change in target and comparison areas using 29 weeks pre and post; ARIMA interrupted time series for each area using 52 weeks pre and post	Target area weekly mean reduced 2.9 (49%) in <i>t</i> -test ($p < .05$) and 2.6 (44%) in ARIMA analyses ($p < .05$); no significant changes in control area ANOVA: gun crimes down 22 (29%) in target area and up 4 (8%) in comparison ($p > .05$). (Differences at $p < .05$ for gun assaults and $p < .1$ for armed robberies); ARIMA: sum of homicides, gun assaults, armed robberies down 1.72–1.93 per week in target area ($p < .05$) and up 1.46–1.54 per week in comparison area ($p < .05$) ANOVA: gun crimes up 15 (36%) in target area and up 4 (8%) in comparison ($p > .05$); ARIMA: sum of homicides, gun assaults, armed robberies up 0.41–0.55 per week in target area ($p > .05$) and up 1.46–1.54 per week in comparison area ($p < .05$)	No significant displacement to surrounding areas
North district, Indianapolis (USA)	Total gun crimes, homicides, gun assaults, armed robberies	Pooled ANOVA analysis of changes in target and comparison areas relative to same 90-day period of prior year; ARIMA interrupted time series for each area using 132 weeks pre and 13–26 weeks post		No significant displacement to surrounding areas
East district, Indianapolis (USA)	Total gun crimes, homicides, gun assaults, armed robberies	Pooled ANOVA analysis of changes in target and comparison areas relative to same 90-day period of prior year; ARIMA interrupted time series for each area using 132 weeks pre and 13–26 weeks post		No significant displacement to surrounding areas

Zone 1, Pittsburgh (USA)	Medical injury data, shots-fired calls	Difference-in-difference-in-differences (DDD) regressions comparing changes during patrol and nonpatrol days in target and comparison areas from 6-week pre- to 14-week postperiods	Shots fired reduced 0.435 per day ($p < .05$, one-tailed) and assault gunshot injuries reduced 0.015 per day ($p > .05$, one-tailed) in zone 1; for both areas, average daily reductions of 0.347 (34%) in shots fired ($p < .05$, one-tailed) and 0.222 (71%) in gunshot injuries ($p < .10$, one-tailed)	Not examined, but patterns suggested possible temporal and geographic displacement
Zone 5, Pittsburgh (USA)	Medical injury data, shots-fired calls	Difference-in-difference-in-differences (DDD) regressions comparing changes during patrol and nonpatrol days in target and comparison areas from 6-week pre- to 14-week postperiods	Shots fired reduced 0.26 per day ($p < .05$, one-tailed) and assault gunshot injuries reduced 0.428 per day ($p < .05$, one-tailed) in zone 5; for both areas, average daily reductions of 0.347 (34%) in shots fired ($p < .05$, one-tailed) and 0.222 (71%) in gunshot injuries ($p < .10$, one-tailed)	Not examined but patterns suggested possible temporal and geographic displacement
Bogotá, Colombia	Gun and nongun homicides	Stratified analysis using indirect standardization. Negative binomial regression. Missing data on time of incident imputed for 22% of cases. Used data from Jan. 1995 through Aug. 1997	Standardized method: homicides reduced 8% ($p \leq .05$); regression method: total homicides reduced 13% ($p \leq .05$) and gun homicides reduced 15% ($p \leq .05$)	No significant displacement to 7-day period following each intervention
Cali, Colombia	Gun and nongun homicides	Stratified analysis using indirect standardization. Negative binomial regression. Used data from Jan. 1993 through Dec. 1994	Standardized method: homicides reduced 17% ($p \leq .05$); regression method: total homicides reduced 14% ($p \leq .05$) and gun homicides reduced 10% ($p > .05$)	No significant displacement to 7-day period following each intervention

Results

Our search identified no randomized controlled trials, but it did yield four nonrandomized studies testing police crackdowns on gun carrying in three U.S. cities—Kansas City, Missouri (Sherman and Rogan 1995; also see Shaw 1994, 1995; Sherman et al. 1995), Indianapolis, Indiana (McGarrell et al. 2000; also see McGarrell et al. 2001, 2002), and Pittsburgh, Pennsylvania (Cohen and Ludwig 2003)—and two Colombian cities—Cali and Bogotá (Villaveces et al. 2000). All of the studies examined directed patrols, which involve assigning additional officers to high-crime areas at high-risk times and allowing them to focus on proactive investigation and enforcement (e.g., intensified traffic enforcement and field interrogations of suspicious persons) rather than on answering calls for service (McGarrell et al. 2001, p. 120).

Police and researchers have long recognized that crime is concentrated in particular neighborhoods within cities (e.g., Shaw and McKay 1942).⁹ For example, the intervention areas in the studies conducted in Kansas City and Indianapolis were relatively small areas of 0.6–2.8 square miles with 4,000–17,000 persons and homicide rates 7–20 times the national average. The two intervention areas in Indianapolis accounted for 19% of that city’s homicides in 1996 while having only 8% of its population. Within such areas, crime is further concentrated in particular street blocks, addresses, and intersections that are nodes for various business, leisure, and travel activities. In large U.S. cities, about 50% of crime occurs within less than 5% of the street blocks and addresses (e.g., Sherman et al. 1989; Weisburd et al. 2004). Crime also follows temporal patterns according to season, day of the week, and time of day. Violent crime, for instance, tends to be higher in warm weather, on weekends, and during evening hours (e.g., Cohen and Ludwig 2003; Tierney et al. 2001; Zawitz et al. 1993: 28).

Several studies conducted during the last few decades suggest that greater numbers of police and higher levels of proactive patrol activity can reduce crime in high-risk areas and at high-risk times, thus enhancing police efficiency and effectiveness (e.g., Boydston 1975; Sampson and Cohen 1988; Schnelle et al. 1977; Sherman and Weisburd 1995; also see reviews in National Research Council 2004; Sherman 1997).¹⁰ In recent years, practitioners and researchers have increasingly emphasized directed patrols as a means to reduce gun crime in such contexts. It is not known how commonly these strategies have been used, but there are anecdotal indications that crackdowns on gun carrying became more common during the 1990s (e.g., see Center to Prevent Handgun Violence 1998; Dunworth 2000; Sherman 2000). In the studies reviewed here, officers sought to detect and deter illegal gun carrying—the suppression of which is thought to be a key mechanism for reducing gun crime at high-risk places and times—by enhancing their visibility and initiating greater numbers of traffic stops and field interrogations.¹¹

Two of the U.S. studies (Indianapolis and Pittsburgh) involved interventions in multiple locations (i.e., patrol areas). Combining these studies with the Kansas City and Colombian studies thus provides seven tests of the effects of directed patrols

on gun crime.¹² However, the tests were not all independent; two intervention areas in Indianapolis were compared to a single area and two areas in Pittsburgh were compared to the same areas.

No studies explicitly testing other police strategies for reducing illegal gun possession and carrying met the criteria for inclusion.¹³ However, the included studies used some of these tactics in addition to directed patrols: roadblock checkpoints were used in Bogotá and Cali; enhanced monitoring of probationers occurred in one area in Indianapolis; and a weapon reporting hotline preceded the introduction of directed patrols in Kansas City. In the sections below, we summarize key aspects of the interventions, the study designs, and the results of the included studies.

Kansas City

Design

From July 1992 through January 1993, police in Kansas City implemented evening gun patrols in a 0.6-square-mile patrol beat with 4,528 residents and a homicide rate roughly 20 times the national average (Sherman and Rogan 1995; see also Sherman et al. 1995). Changes in gun crime were examined during the 29 intervention weeks relative to the prior 29 weeks. The study also examined changes based on 52-week pre- and postintervention periods (the 52-week postintervention period included the 29 intervention weeks and 23 weeks after the patrols ended).

Changes in gun crime in the target area were contrasted with changes in a comparison area several miles away. Both areas had homicide rates many times the national average, had virtually identical numbers of drive-by shootings in 1991, and were overwhelmingly black. Moreover, the areas had similar gun crime trends during the three and a half years prior to the intervention: oscillating but generally stable levels from 1989 through the first half of 1991, followed by an upswing before the beginning of the intervention period.

However, the areas also differed in numerous ways. The comparison area was nearly twice the size of the target area (150 versus 80 square residential blocks) and had a lower population density (4,308 versus 7,075 residents per square mile), higher land values (\$23,958 versus \$14,181 median parcel value), and a better-educated population (73% versus 53% of adults with a high school degree). Furthermore, although the areas had similar numbers of gun crimes, their rates differed substantially; relative to the target area, the comparison area had a 24% lower rate of homicide (1.4 versus 1.8 per 1,000 residents), a 40% lower rate of drive-by shootings (3.1 versus 5.3 per 1,000 residents), and a 23% lower rate of overall gun crime (31.0 versus 40.4 per 1,000 residents). (Table 1 highlights selected features of the areas.)

Intervention

Prior to the gun patrols, officers conducted a 10-week program of door-to-door visits in the target area, during which they informed residents of the upcoming

crackdown on gun carrying and asked them to report gun offenders to an anonymous tips hotline. The hotline received only two calls.

After the preintervention campaigns, patrols were done on 200 nights, usually by four officers in a pair of two-officer cars (see Table 2). Patrols involved a total of 4,512 officer-hours, during which the officers were freed from answering radio calls and were engaged in proactive gun detection via car and pedestrian stops. Officers issued 1,090 traffic citations and made 948 car checks, 532 pedestrian checks, and 616 arrests. In the process, officers seized 29 guns, which increased total gun seizures in the area by 65% over the prior 6-month period. The authors reported that “regular policing activities” increased in the area by 260% (Sherman and Rogan 1995).

Main results

Total gun crimes—which consisted primarily of violent crimes with guns (i.e., robberies and assaults) but also included property crimes committed with guns (primarily destruction of property)—fell by 49% in the target area, from 169 in the 29 weeks prior to the patrols to 86 during the 29-week intervention period (see Table 3). This change was statistically significant in a *t*-test of weekly means. Similarly, an interrupted time series analysis based on the 52 weeks before the program and the 52 weeks following the start of the program suggested a drop of 2.6 gun crimes per week (or about 44%) after controlling for temporal trends.¹⁴ As reported below, there was some evidence that the patrols may have reduced homicides and drive-by shootings, but there were no reported changes in total crimes, overall violent or property crimes, or disorderly behavior. Substitution of crimes without firearms was not assessed.

Though gun seizures did decline in the comparison area, there were no statistically significant changes in gun seizures or gun crime in the comparison beat. Direct statistical tests between the target and comparison areas were not reported.

It is possible that the postintervention change in the intervention area was an artifact of regression to the mean; the intervention followed the highest levels of crime in the 5 years for which data were reported. After the intervention, gun crime in the target area dropped to a level comparable to what it had been from 1989 through mid-1991, raising the possibility that the evaluators missed a regression artifact by analyzing only the 29- and 52-week periods prior to the intervention. The fact that the crime drop in the target area coincided closely with the start of the intervention while no such regression occurred in the comparison area mitigates this concern to some degree, but it is hard to rule out a regression artifact in light of the short analysis period and the noted differences between the target and comparison areas.

Other results

To investigate geographic displacement of gun crime, the investigators used both *t*-tests (for 29-week pre- and postperiods) and interrupted time series models (for 52-week pre- and postperiods) to examine pre-post changes in the seven beats

adjacent to the target beat, both individually and collectively (see Table 3). None of these beats experienced a statistically meaningful increase in gun crime. Collectively, the adjacent beats had a nonsignificant net increase of 52 gun crimes (7%) during the 29 intervention weeks—which would not wholly offset a reduction of 83 in the target area— and a nonsignificant drop for the 52 weeks spanning the intervention and the follow-up period. Further, the city as a whole had a 2% drop in gun crime during the 29 intervention weeks, providing no obvious sign of displacement elsewhere.

Although not significant, the time series impact estimate for all contiguous areas (impact = -2.577 , $p > .05$) was similar to the impact estimate for the target area (impact = -2.558 , $p < .05$), and both were greater than the nonsignificant impact estimate for the comparison area (impact = $-.751$, $p > .05$). Further, impact estimates were negative for five of the seven surrounding areas, and two of the negative estimates were statistically significant. This could be interpreted as evidence of diffusion of benefits or as evidence that the area as a whole was on a downward trend.

After a 5-month pause, the patrols were reintroduced from July 1993 to December 1993. The evaluators reported that gun crime again declined in the target area while rising in the comparison area, though they did not present details regarding the patrols or regarding crime trends during this second phase. In addition, contrasts of the two 6-month intervention periods with all other half-year periods from 1991 through 1993 suggested that the patrols reduced both homicides (based on chi-square tests) and drive-by shootings (based on analysis of variance) in the target area while these crimes remained unchanged in the comparison area.¹⁵ Finally, surveys suggested that gun patrols had strong community support and had favorable impact on residents' perceptions of neighborhood conditions and fear of crime (Shaw 1995).¹⁶

Indianapolis

Design

For 90 days during the latter part of 1997, police in Indianapolis implemented directed patrols in two target areas, each of which included two patrol beats (McGarrell et al. 2000, 2001, 2002). One target area in the east part of the city covered 1.7 square miles, with a primarily white and low-income population of about 14,600 residents and a violent crime rate 96% higher than the citywide rate (38.4 versus 19.6 per 1,000 residents; see Table 1). Another target area in the north part of the city was a low-income area of about 16,600 residents, mostly black, and 2.8 square miles with a violent crime rate 72% higher than the citywide rate (33.8 versus 19.6 per 1,000 residents). Although preintervention crimes trends were not presented in detail, both areas appeared to have rising levels of violence. Violent crime rose 25% in the east area from early 1996 to early 1997 and 23% in the north area from early 1995 to early 1997 (McGarrell et al. 2000, Table 5–1).

An evaluation of the project contrasted changes in crime in the target areas to those in a comparison area that did not receive gun patrols (and where gun seizures

declined during the study period). The comparison area consisted of two patrol beats in the east part of the city that together covered 4.7 square miles with a largely black population of about 19,300 persons. However, the area's violent crime rate was 33–41% lower than those of the respective target areas prior to the intervention. The population density in the comparison area (4,073 people per square mile) was much lower than the population density in either the east or north intervention areas (8,666 and 5,954 people per square mile, respectively). The comparison area was, therefore, more like the city as a whole than the target areas, which were relatively more violent and dense.

It is not clear that the comparison area offers a reasonable counterfactual of what would have happened in the target areas had the patrols not been in effect. Gun crime was much lower in the comparison area than in the north area (preintervention trends in the comparison area were not presented), and may have been low enough to create a *floor effect*; given the preintervention levels of crime, it is possible that the north area would appear to improve by comparison merely as a result of chance. To illustrate, the north target area had about six violent gun crimes per week (as measured by homicides, gun assaults, and armed robberies) during the 90-day period 1 year prior to the intervention. The east target area had four per week, and the comparison area had only three per week.

Gun crime was more comparable (in terms of numbers though not rates) in the east area and comparison area than in the north area and comparison area, and the east and comparison areas may have shared other similarities by virtue of their geographic proximity. However, they were very different demographically, and it is not clear whether their preintervention crime trends were similar. Because the comparison area was not a particularly good match to the target areas, the authors also looked for changes in gun crime during the intervention period for the city overall (minus the target areas). Using time series analyses (described below), they found no significant changes.

Intervention

Officers implemented different styles of patrol in the two target areas (see Table 2). In the east target area, officers pursued a *general deterrence* strategy in which they sought to maximize traffic stops, thereby seizing more guns and creating a general sense of enhanced police presence. In the north area, officers made pedestrian and vehicle stops more selectively, utilizing a *targeted offender* approach that focused on particularly suspicious persons and vehicles. Officers in the north area also paired with probation officers to conduct home visits of probationers. During the 90-day intervention period, officers spent 2,905 hours patrolling the east/general deterrence area, making 3,826 vehicle stops and 558 arrests. In the north/targeted offender area, officers spent 1,975 hours on patrol, making 1,417 vehicle stops, 434 arrests, and 126 probation checks.¹⁷

The gun patrols produced 12 gun seizures in the north/targeted offender area and 13 gun seizures in the east/general deterrence area, increasing total gun seizures by 50% in the east/general deterrence area but by only 8% in the north/targeted offender area relative to the same 90-day period of the prior year.¹⁸

Although gun seizures rose less (in percentage terms) in the north/targeted offender area than in the east/general deterrence area, there were more gun recoveries per officer-hour, per vehicle stop, and per pedestrian stop in the north area. Meanwhile, gun seizures fell 40% in the comparison area between these same periods (from 45 to 27).

Main results

During the intervention period, total gun crime dropped 29% in the north/targeted offender area relative to the same 90-day period of the prior year (from 75 to 53; see Table 3).¹⁹ In addition, gun assaults and armed robberies (including those with guns and other weapons) both declined about 40%, and homicides dropped from seven to one. Based on analysis of variance tests, the reductions in gun assaults and armed robberies were statistically significant relative to the comparison area, where gun and weapon offenses increased and homicides remained unchanged.

In the east/general deterrence area, homicides dropped from four to zero, but other gun crimes rose. Contrasts of the east/general deterrence and comparison areas produced mixed and nonsignificant results. Total gun crimes, for example, increased 36% in the former but only 8% in the latter. On the other hand, total homicides, gun assaults, and armed robberies rose 22% in the east/general deterrence area (from 54 to 66) and 89% in the comparison area (from 38 to 72).²⁰

The investigators supplemented these tests with interrupted time series analyses of weekly violent gun crimes (approximated by the sum of homicides, gun assaults, and armed robberies) in each area over a 158-week span covering the 132 weeks (i.e., 2.5 years) prior to the intervention, the 13 intervention weeks, and 13 postintervention weeks. Controlling for temporal trends, results suggested a drop of nearly two gun crimes per week during the intervention in the north/targeted offender area (a result consistent with results in Kansas City), no meaningful change in the east/general deterrence area, and an increase of 1.5 gun crimes per week in the comparison area.²¹ The time series analyses suggest that gun crime dropped in the north/targeted offender area and that the increase in the east/general deterrence area was due to normal variation or a preintervention trend.²²

Taking the results at face value, the targeted offender gun patrols may have been successful, but the general deterrence patrols appear to have made little difference. The targeted offender patrol style may have enabled officers to focus their attention more efficiently and effectively on people and places at highest risk for gun violence; however, one might question whether the patrol styles interacted with differences between the target areas in crime, racial composition, and other factors.²³

Other results

Focusing on other results from the north/targeted offender area, the gun patrols did not appear to affect violent crimes without firearms.²⁴ Nor was there clear evidence of crime displacement or diffusion of benefits to nearby areas; collectively, the five patrol beats surrounding the north target area experienced a 10% increase in homicides, gun assaults, and armed robberies (relative to the same 90-day period of

the prior year), which was statistically nonsignificant, was spread across the areas, and was too small to completely offset the corresponding reduction in the north/targeted offender area.²⁵

Community surveys administered by the evaluators indicated that the initiative had a high level of public support, which may have been due in part to the efforts of police managers to emphasize professional and respectful treatment of citizens and to secure the support of neighborhood leaders prior to the intervention.

Pittsburgh

Design

For 14 weeks spanning July 1998 to October 1998, police in Pittsburgh conducted evening gun patrols two nights a week in two of the city's six patrol zones (Cohen and Ludwig 2003). Compared with the interventions tested in Kansas City and Indianapolis, this project was less intensive, involving fewer officer-hours spread over larger areas. The target patrol zones (zones 1 and 5) were both approximately 10 square miles with about 55,000 and 80,000 residents, respectively. These zones had the highest crime rates in the city, and each had over a dozen neighborhoods that were described as diverse in their demographic characteristics and crime problems (see Table 1).

The comparison areas were the city's four remaining patrol zones. Relative to the target zones, the comparison zones were on average less dense (6,494 versus 7,312 persons per square mile) and had a lower percentage of black residents (24.8% versus 38.8%), but they had comparable home ownership rates (47.1% versus 44.1% of residents owned their own homes) and poverty levels (23.3% versus 24.4% of residents were poor) (Cohen 2002, Exhibit 5).²⁶

Given the relatively short study period (6 preintervention weeks and 14 intervention weeks) and the large preintervention differences in crime between the intervention and comparison zones, regression artifacts and floor effects must be considered throughout the analyses. For example, daily gunshot injuries, one of the study's primary outcome measures, were 187% higher in the target zones than in the comparison zones prior to the intervention (.155 versus .054). This difference was due to the Wednesday to Saturday portion of the week, which is when police conducted the intervention (as discussed below). Gun injuries averaged 0.028 per day for both intervention and comparison zones on Sunday through Tuesday, but from Wednesday to Saturday the target areas averaged 0.25 per day while the comparison areas averaged 0.073 per day. Hence, even a naturally occurring reduction in injuries (i.e., a regression artifact) would have been most likely to occur in the intervention zones on the intervention days.

Intervention

Gun patrols were conducted in the target areas during the Wednesday to Saturday portion of each week, though the specific days varied week to week and between zones (see Table 2). Five additional officers working in three cars patrolled the areas between 8 p.m. and midnight on the selected evenings. The patrols

emphasized traffic stops and “stop and talk” contacts, reportedly using a mix of place-based and person-based targeting. In sum, 51 patrols were implemented across the two intervention areas for a total of nearly 1,000 officer-hours, increasing the usual number of officers by 50% in zone 1 and by 25% in zone 5. In addition to making over 200 contacts and 18 arrests (most of which occurred in zone 5), the officers seized seven guns (two in zone 1 and five in zone 5). It is not clear whether gun seizures increased over normal levels.

Main results

An evaluation of the program compared changes in calls to an emergency dispatcher reporting “shots fired” and hospital reports of assault-related gunshot injuries in the target zones with those in the city’s other patrol zones (see Table 3). Most analyses were based on daily time series data covering the 6 weeks prior to the intervention and the 14 intervention weeks. This review counts the intervention in each area as a separate trial, but many of the analyses presented by the authors were based on results combined for the two target areas.

Averaged across the target zones, shots-fired calls declined 0.066 per day (9%) and gunshot injuries declined 0.048 per day (31%) during the intervention. In contrast, shots-fired calls increased by 0.053 per day (19%) and gunshot injuries rose by 0.026 per day (48%) in the other patrol zones. Changes in gunshot injuries were significantly different between the intervention and comparison areas, leading the authors to a differences-in-differences²⁷ estimate of 0.073 injuries prevented per day.

The investigators also conducted a differences-in-differences-in-differences (DDD) analysis based on changes in gun crime during the program nights (Wednesday to Saturday) and nonprogram nights (Sunday to Tuesday) between areas.²⁸ The authors argued that results averaged across the intervention areas suggested that the patrols reduced shots-fired calls by 0.347 per day (34%) on the program days and gunshot injuries by 0.222 per day (71%).²⁹ Separate DDD estimates for each target zone (see Table 3) suggest that both had significant reductions in shots-fired calls, but only zone 5 experienced a significant reduction in gunshot injuries (the authors reported that the proportional decline in gunshot injuries was similar in both zones). The authors also reported that gunshot injuries increased in zone 5 after the program ended.

Other results

These estimates may overstate the program’s impact in a number of ways. The validity of the shots-fired measure was questionable; officers were unable to verify an incident in three out of every four calls. Moreover, a sensitivity analysis that substituted the program year data with data from the same calendar period of the year before the program (1997) also produced significant DDD estimates for shots-fired calls (the authors referred to these estimates as “phantom program effects”). This strongly suggests the estimated drop in shots-fired incidents was due at least in part to a preintervention trend, a seasonal pattern, or chance.

As discussed previously, regression artifacts and floor effects may have also influenced the gunshot injuries analysis. Indeed, the authors found a similar though smaller DDD “phantom” effect in zone 5 by replacing the program year data with data from the same weeks of the year after the program (1999), during which the gun patrols were no longer in effect.³⁰

Geographic and temporal displacements of crime are also a concern. As noted earlier, gunshot injuries decreased by 0.048 per day in the program areas but simultaneously increased by about half that value (0.026 per day) in the comparison areas. While injuries declined by 0.161 per day during the program days in the program areas, they rose by 0.103 per day on nonprogram days in these same areas. Similar patterns appeared in the shots-fired analyses. Therefore, it is possible that crime was displaced from the target areas to the comparison areas and/or that crime was displaced from intervention days to nonintervention days within the target areas. The former would have increased both the DD and DDD estimates of the patrols’ impact, and the latter would have increased the DDD estimates.

Colombia (Cali and Bogotá)

Design

The Colombian cities of Cali (1.8 million residents in 1994) and Bogotá (5.6 million residents in 1996) implemented intermittent bans on all gun carrying at different times in the 1990s (Villaveces et al. 2000). During the early 1990s, these cities had overall homicide rates comparable to those of the highest crime areas of some American cities (see Table 1); together, they accounted for less than 20% of the population and nearly a third of homicides in Colombia, which had a homicide rate over nine times higher than that of the United States³¹

An evaluation of these programs compared homicide rates during ban and nonban days over multiple years in each city.³² Because the bans were implemented primarily at high-risk times for homicide (e.g., weekends following paydays), the researchers used statistical adjustments (described below) to compensate for preexisting differences between the intervention and nonintervention days.

Intervention

The bans were implemented primarily on weekends following paydays, holidays, and election days (all of which were high-risk times for homicide; see Table 2). However, bans were not established on all such days during the intervention periods, and bans were not limited to just these days.³³ In Cali, bans were implemented on 34 occasions for a total of 89 days spanning from November 1993 through 1994. In Bogotá, there were 22 bans covering a total of 67 days during the following periods: December 1995 through March 1996, December 1996 through February 1997, and March 1997 through April 1997. Cali used the bans during a

period of rising homicide rates, and Bogotá employed them during a period of falling homicide rates and rapid population growth.

The bans were advertised through the media and applied to all people, including those with permits to carry guns. When the bans were in effect, police enforced them through roadblock checkpoints (which were usually established in high-crime areas), searches during traffic and pedestrian stops, searches of patrons in bars, and other routine activities.³⁴ However, the study provided very little specific information about police activities. The evaluators reported that police in Cali recovered four guns per day during intervention times and 0.8 guns per day during nonintervention times. These figures seem remarkably low given Cali's size and homicide rate, perhaps suggesting that police enforcement efforts were focused on relatively small areas of the city and/or that enforcement efforts were modest.³⁵ No further implementation data were available for either city. Hence, the contexts, types, dosages, and durations of police activities are unclear.

Main results

An evaluation of these programs compared homicide rates during ban and nonban days in Cali from 1993 through 1994 and in Bogotá from 1995 through August 1997 (see Table 3). As noted above, the bans were generally implemented on days when homicides were most likely to occur. Despite the bans, the unadjusted homicide rate was higher on intervention days than on nonintervention days in both cities; Cali's homicide rate was 51% higher on intervention days (161.8 versus 107.5 per 100,000 person-years), and Bogotá's homicide rate was 37% higher on intervention days (81.3 versus 59.3 per 100,000 person-years). In short, police were able to identify and implement bans at times that were considerably more violent than normal.

To adjust for preexisting differences between ban and nonban days, the researchers stratified nonintervention days according to several temporal dimensions (such as type of day, time of day, and season) to estimate risk-adjusted, expected homicide rates for the intervention periods and then compared these to homicide rates observed during intervention days. They also estimated negative binomial regression models controlling for the effects of time of the week, payday weekends, holidays and election days, month of the year, and citywide time trends. Results were similar with both analytical methods.

Based on regression analyses, the authors estimated that the bans reduced homicides 13–14% when they were in effect. In Bogotá, the authors estimated that the bans had comparable effects on gun and nongun homicides, reducing the former by 15% and the latter by 12%. In Cali, the estimated reductions were 23% for nongun homicides and 10% for gun homicides, though this was not a statistically significant difference.

Other results

Evidence thus suggests that nongun homicides also decreased during the bans. There was no measurable displacement of homicides to the 7-day periods immediately following each ban, but other patterns of temporal and geographic

displacement were not examined. Consequently, the study may overstate the impact of the bans, particularly since they occurred on a regular rather than random basis and were advertised through the media, giving potential offenders a clear sense of when detection risks were greatest. Further, homicides actually rose 18% in Cali from 1993 to 1994 despite the use of the bans (the investigators did not explore whether this was an improvement over prior trends).

Discussion

Summary and caveats

With one exception, these tests suggest that directed patrols focused on illegal gun carrying reduce gun violence at high-risk places and times. Inferences are limited, however, by the small number of available trials (which were not all independent), variability in study design and analytical strategy, and the absence of randomized trials. Results of nonrandomized studies sometimes differ from results of randomized trials (e.g., see Deeks et al. 2003); nonrandomized studies of criminal justice interventions in particular are more likely than randomized trials to find favorable results (Weisburd et al. 2001). Despite careful efforts by the studies' authors, conclusions must also be qualified due to ambiguities in the evidence and confounding factors that have received limited attention. We review a number of these considerations in the sections below.

The results of this review may not be widely generalizable. Outcomes included in this review are mostly from trials conducted in the United States. The results may not apply to other countries with different crime problems, gun laws, and systems of justice. Further, all of the trials were conducted in high-crime urban areas and at high-crime times. Even if directed patrols do reduce crime under those conditions, they may not reduce crime in places or at times with lower levels of crime. The long-term effects of this strategy are also unknown.

Statistical outcomes

The Colombian studies, which used the same outcome measures and statistical procedures, estimated that crackdowns on gun carrying reduced firearm homicides 10–15% (based on regression estimates). Results from the two locations were statistically indistinguishable based on their confidence intervals.

Estimated effects were generally larger and more variable in the American studies. The most similar of the American studies were conducted in Kansas City and Indianapolis. Gun crime dropped 29–49% in the successful trials in those cities. The Pittsburgh study, which involved larger and more diverse areas, lower intervention dosages, different outcome measures, and different statistical techniques, suggested reductions in gun crime ranging from 34% (for shots-fired calls) to 71% (for gunshot injuries) averaged over two target areas.³⁶ Taken

together, the American studies suggest that directed patrols reduced gun crime by roughly a third or more. The exception to this pattern was the unsuccessful effort in the Indianapolis east target area, where gun crime did not decline. In the American studies, gun crime generally rose or remained unchanged in the comparison areas. A note of caution is that there are some indications that enforcement efforts decreased in the comparison areas during the Kansas City and Indianapolis studies. The results of these trials are very promising, but they are more likely to overestimate than underestimate the effects of directed patrols.

Differences in the estimated impacts across studies may be attributable to a number of factors. The American studies were based on city patrol areas with as few as 4,500 people. The Colombian studies, in contrast, were based on entire cities with millions of residents. Police may be able to raise apprehension risks (real and perceived) more successfully in small areas such as a neighborhood than in larger areas such as an entire city (e.g., see Sherman and Weisburd 1995). Other potentially important differences between the American and Colombian studies include legal and cultural differences, differences in implementation and tactics, and differences in research design, outcome measures, and statistical techniques.

Although we were able to present a narrative synthesis, a statistical synthesis or meta-analysis would have been unnecessary and inappropriate.³⁷ The evidence is generally favorable to the interventions, so there is little need to improve statistical power or reconcile conflicting results. Furthermore, if the extremely large benefits reported in these trials prove to be reliable indications of what localities can expect from directed patrols focused on guns, there may be little need to increase the precision of effect estimates large enough to have immediate and urgent implications for policy and practice.

More importantly, these studies differ notably in several respects that cannot be quantified; among others, these include differences in their units of analysis, intervention delivery, comparison groups, and statistical techniques to control for bias. This diversity arguably precludes meaningful comparisons of statistically standardized effects across studies and could produce potentially misleading results if such estimates were combined (Deeks et al. 2005, pp. 98–99). Finally, the Campbell Collaboration generally discourages meta-analyses that do not include randomized trials (Shadish and Myers 2004).³⁸

Comparability of intervention and comparison areas/times

Comparisons between intervention and comparison groups (i.e., areas and times) in these nonrandomized trials are problematic. The intervention areas were chosen for their high and often rising levels of gun crime—levels that were often substantially higher than those in the comparison areas. This makes it likely that gun crimes in the intervention areas would have fallen or followed otherwise different trends from those in the comparison areas, even in the absence of the intervention. In other words, it is unlikely that the interventions were the only important difference between the intervention and comparison conditions.

Implementation and intermediate outcomes

It is hard to draw generalizations regarding dosage and other implementation factors due to the small number of studies, the poor documentation of some efforts (particularly those in Colombia), and other differences in the types of areas and analyses involved. Further, available activity measures often fail to capture potentially important differences in program delivery. In Kansas City, for example, officers reportedly spent only 27% of their time actually patrolling the target area. The remainder was spent processing arrests and “performing other patrol-related duties, as well as in patrol work outside the target area” (Sherman and Rogan 1995).

Complementary activities also differed among the trials. These activities included the probation/parole checks in Indianapolis (it was not reported whether these resulted in gun seizures), the door-to-door citizen contacts in Kansas City (these sorts of contacts have been evaluated favorably as a policing strategy in other contexts), and the media announcements in Colombia. How these activities affected the reported results is unclear.

The most extensive and comparable of the studies are those done in Kansas City and Indianapolis, and the authors of the Indianapolis study made extensive comparisons of police effort in the Kansas City and Indianapolis target areas (McGarrell et al. 2000, 2001). Standardizing officer-hours, arrests, and gun seizures by person-weeks and square-mile-weeks, the Kansas City intervention was most intensive, followed by Indianapolis east and Indianapolis north. Yet the apparent outcomes did not correspond to this ranking, as the reported reductions in gun crimes were greatest in Kansas City and Indianapolis north. Similarly, rates of gun detection were highest in the Kansas City and Indianapolis north trials; the Kansas City officers seized one gun per 156 officer-hours, and the Indianapolis north officers seized one per 165 officer-hours. By contrast, in Indianapolis east officers seized one per 223. The authors of the Indianapolis report have suggested that the more patrols focus on high-risk places (as in Kansas City) and persons (as in Indianapolis north), the better the outcomes (McGarrell et al. 2000, 2001).

A related caveat is that the studies did not directly measure changes in gun carrying, which is the presumed mechanism through which the patrols reduced gun crime. For example, they did not examine changes in the number of gun seizures per traffic and pedestrian stop during the interventions, nor did they measure gun carrying using other methods (such as offender surveys). Although the inferred link between gun carrying and gun crime has a strong theoretical basis, documenting changes in gun carrying more explicitly would strengthen the evidentiary links between gun patrols, gun carrying, and gun crime.

Crime displacement

The included studies did not consistently address crime displacement in its various manifestations (for an extended discussion of displacement and its varieties, see

Barr and Pease 1990). The included studies provide limited evidence of crime displacement to nearby areas that was statistically insignificant and not sufficient to offset declines in the target areas. It is difficult, of course, to completely rule out displacement to areas not adjacent to the target areas because the number of crimes that may be displaced in these types of studies is typically small enough to be lost in the normal variability of crime at the city level. However, crime tends to be concentrated at places and times that bring together motivated offenders, suitable targets, and an absence of capable guardians (Cohen and Felson 1979; see also Sherman et al. 1989). To the extent that crime is displaced, therefore, it may move to areas having a similar constellation of these features. Such areas are often in close proximity.

Short-term temporal displacement (for example, displacement to different times of the day or different days of the week) was a potential if not documented problem in the Pittsburgh and Colombian studies. Displacement to crimes without firearms did not appear to occur in Indianapolis north, Cali, or Bogotá (it was not addressed elsewhere).

Community reaction

Directed patrol is an aggressive policing strategy that raises concerns about legality, racial profiling, and community relations, particularly in the minority neighborhoods where such interventions are most likely to occur. Nevertheless, no legal challenges were raised against the patrols in any of the American studies (see note 11). Surveys of residents in Kansas City and Indianapolis revealed that residents were supportive of the patrols. Both programs involved police efforts to notify and consult the communities and to train officers. By some measures, the patrols also improved residents' perceptions of the target areas. All of this suggests that programs of this sort can be implemented without harming police–community relations, but this cannot be taken for granted.³⁹

Cost-effectiveness

In the American studies, officers working overtime conducted the gun patrols. In Pittsburgh and Indianapolis, the costs of the patrols were approximately \$35,000 and \$120,000, respectively. By way of comparison, the average lifetime medical costs of a single assault-related gunshot injury were estimated to be almost \$19,000 dollars in 1994 (Cook et al. 1999), and some estimates, though controversial, suggest that the full societal costs of each gunshot victimization may be as high as \$1 million (Cook and Ludwig 2000). The evaluators of the Indianapolis program noted that, if their estimates are accurate, patrols would pay for themselves by preventing just one murder and two robberies and aggravated assaults, even without considering personal costs to victims (McGarrell et al. 2000, p. 85).

At first glance, therefore, gun patrols would seem to be cost-effective. Nevertheless, as the Indianapolis investigators noted, a more complete examination of this issue must account for other costs and benefits associated with these initiatives, including the costs of processing arrests and the revenues from issuing traffic tickets. Given that directed patrols are intensive and expensive interventions, a better understanding of their long-term effects (i.e., those that occur during a sustained crackdown and those that follow a crackdown) and optimal length would also be relevant to cost-benefit considerations (Sherman 1990).

Future directions

Subject to the caveats we have noted, a small number of nonrandomized, quasi-experiments suggest that directed patrols focused on illegal gun carrying prevent gun crimes, saving lives and public resources at the same time. Future studies could improve on prior research and answer some of the questions raised above by using better and more control groups, by controlling for confounding variables through randomization, and by carefully measuring variables related to context and program implementation (e.g. officer hours, pedestrian stops, and complementary activities such as probation/parole checks) (Montgomery et al. 2005). Multisite randomized trials using numerous patrol beats from several cities, as Sherman (1997) has advocated, appear justified for this strategy. Barring such ambitious efforts, it might be feasible to rotate gun patrol crackdowns at random intervals across a number of high-crime areas in a single city over an extended period (Sherman 1990). Evaluators could improve evidence obtained from all trials, randomized or not, by devoting more formal attention to long-term differences in crime levels and trends between target and intervention areas and by collecting implementation and outcome measures like those used in previous studies to facilitate comparisons across studies.

Even randomized controlled trials, however, will not answer all of the outstanding questions about directed patrols. Studies of offenders (using official records and/or surveys) might tell researchers more about the characteristics of people who illegally carry weapons in gun crime hot spots, including their prior and current offending patterns, areas of residence, and dispositions, all of which could yield useful data about deterrence, incapacitation, displacement, and diffusion effects associated with gun patrols. Such information might also provide insights into how gun patrols could be better structured to reduce crime. More broadly, qualitative research involving community members, offenders arrested in crackdowns, and other stakeholders should be included in future experiments. Such research should highlight the strategies that individuals living in violent communities believe will reduce crime without harming or alienating the communities meant to benefit.

Police interventions are, of course, a limited response to gun violence, and they do not address many of the underlying social conditions that contribute to crime and violence. Yet there is evidence that directed patrols may help break the cycle

of gun violence in troubled communities and establish the conditions necessary for long-term improvements. To use a medical analogy, one must stop the bleeding before one can heal the patient.

Acknowledgements

This research was supported by grant no. 2004-DD-BX-0003 awarded by the National Institute of Justice (Office of Justice Programs, U.S. Department of Justice) and by funding from the Campbell Collaboration and the Jerry Lee Center of Criminology (University of Pennsylvania). Points of view in this document are those of the authors and do not necessarily represent the official position or policies of the U.S. Department of Justice, the trustees of the University of Pennsylvania, or the Campbell Collaboration. The authors thank Daniel J. Woods for research assistance. They also thank David Weisburd and anonymous reviewers for helpful comments on the manuscript.

Notes

- 1 This is not the first review of police crackdowns on illegal gun carrying (e.g., see National Research Council 2005; Sherman 1997). This paper expands on previous work by systematically reviewing the literature, by examining studies in detail, by incorporating research from outside the United States, and by considering strategies other than directed patrols for reducing illegal gun possession and carrying (e.g., enhanced monitoring of probationers and parolees and consent searches of at-risk youth).
- 2 Studies of murderers in some U.S. cities have revealed similar patterns (Kennedy et al. 1996; Moran 2006; Tierney et al. 2001).
- 3 Further details about the research protocol are provided in Koper (2003). We want to emphasize that the paper published here is an independent effort of the authors and has not been reviewed by the Campbell Collaboration.
- 4 A related intervention is the installation of metal detectors in places such as airports, schools, and government buildings. Although the use of metal detectors is intended to discourage weapon carrying, it is arguably a form of target hardening with limited applicability to general police work, particularly the reduction of street crime. Consequently, the review does not include studies of fixed metal detectors.
- 5 For example, the review does not include evaluations of Project Exile, a federally driven, prosecutorial intervention to enhance penalties for gun violators in Richmond, Virginia (Raphael and Ludwig 2003; Rosenfeld et al. 2005); the Bartley-Fox gun carrying law that raised penalties for illegal gun carrying in Massachusetts in the mid-1970s (Deutsch and Alt 1977; Pierce and Bowers 1981); or gun courts (Gendreau and Surridge 1978; Sheppard and Kelly 2002).
- 6 Statistical controls could include multivariate regressions controlling for factors related to crime (e.g., demographic and other socioeconomic characteristics of the areas) or other methods adjusting for different crime levels and trends in the intervention and comparison areas.
- 7 Studies that examined “weapons” or “armed” offenses without separate consideration of gun offenses were not formally included in the review because it is conceivable that a police intervention could reduce nongun weapon offenses without reducing gun offenses.

- 8 The Justice Research and Statistics Association maintains a database of reports by crime and justice statistical analysis centers operated (or funded) by state governments throughout the United States.
- 9 These areas typically have high levels of poverty, family disruption, population density, residential instability, and racial segregation (Sampson 1995).
- 10 This is thought to occur by raising offenders' real and perceived risks of apprehension.
- 11 In many countries, police cannot stop vehicles and pedestrians on an arbitrary basis. In the United States, police can stop vehicles and persons when they observe traffic violations or other suspicious activities. Once this occurs, police may find other evidence that justifies a search: they may see guns or other contraband in plain view, find that the stopped person is wanted for other crimes, or observe other signs (e.g., a motorist trying to hide something under the seat) that justify a search of the car and/or a frisk of the individual(s). In some cases, stopped motorists may even consent to having their cars searched if asked by police.
- 12 In Kansas City, the intervention occurred twice in the same area. As discussed below, however, the second intervention was not reported in sufficient detail to be counted as a separate test for this review.
- 13 We found several additional studies that examined the impact of directed patrols and other eligible strategies on gun crime but that did not meet all criteria for inclusion in the review. A few examples include a nonexperimental, correlational study of weapons arrests and gun homicides in New York City (Fagan and Davies 2003); a study that described a St. Louis, Missouri, program to search the homes of juveniles suspected of having firearms but that did not assess the program's impact on gun crime (Decker and Rosenfeld 2004); and a study of probationer surveillance and shootings in Wilmington, Delaware, that did not have a comparison group or area (Delaware Statistical Analysis Center 1998).
- 14 The times series estimate of 2.6 fewer gun crimes per week suggests a reduction of 44% relative to the trend-adjusted, preintervention level of 5.8 gun crimes per week (Sherman and Rogan 1995, p. 686). This is similar to the unadjusted drop of 49% between the 29-week pre- and postintervention periods. Likewise, the time series estimates imply that the patrols prevented about 74 gun crimes during the 29-week intervention period, which is comparable to the unadjusted drop of 83 gun crimes.
- 15 The change in drive-by shootings in the target area approached statistical significant ($p < .1$), while the change in homicides was significant ($p < .05$). Exact numbers were not reported, but a graphic illustration shown in Sherman and Rogan (1995, p. 688) suggests there were roughly 7–15 drive-by shootings semiannually when the gun patrols were not in operation and roughly two when the patrols were operating. The only reported figures for homicide indicate that the target area had four homicides semiannually during 1991.
- 16 For this paper, we concentrate on gun crime as the outcome of interest and do not review the attitudinal survey results in detail.
- 17 No further details were provided on the specifics of the patrols, such as the number of officers and cars involved or the times of day when the patrols took place.
- 18 For most analyses, the evaluators contrasted the intervention period with the same 90-day period from the prior year rather than the 90 days just prior to the intervention in order to control for possible seasonal effects. Total gun seizures rose only modestly in the north/targeted offender area, in part because routine gun seizures by patrol officers declined during the intervention period.
- 19 The investigators did not explicitly define the category of all gun crimes, so it is not clear whether they included property offenses committed with guns, as did the investigators in Kansas City.

- 20 The investigators also compared changes in the target areas to those in the rest of the city. This review generally focuses on results for the two-beat comparison area because it represents a unit more comparable to the target areas. It also focuses on the separate results for each intervention area rather than on results combined across the intervention areas.
- 21 Results were very similar whether defining the postintervention period as the 13 intervention weeks or as the 13 intervention weeks plus the 13 weeks after the patrols ended. Time series estimates for the north/targeted activities area suggest that the patrols prevented 22–25 violent gun crimes during the 13-week intervention period. This reduction cannot be expressed as a percentage because the authors did not present preintervention means for the full period covered by the time series analyses. However, there were six homicides, gun assaults, and armed robberies per week in the north/targeted activities area during the 90-day preintervention period defined earlier. Using this as an approximate baseline, the time series results imply that the patrols reduced weapons violence in this area by 29–32%. By comparison, the unadjusted drop in these crimes between the 90-day pre- and postintervention periods was 34, or 44%.
- 22 Considering that violent crime had been rising for at least 2 years in the north area, there is perhaps some lingering concern about a regression artifact in that area. The time series analyses mitigate that problem for the study period (1995 through early 1998), though it is hard to completely rule out the possibility of a regression phenomenon over a longer period without additional data.
- 23 A comparison of the intervention period to the 90 days just prior to the intervention showed that gun crime declined in the east area while rising in the comparison area. A very similar directed-patrols initiative that did not emphasize guns to the same degree took place in the east area for 2 months in late 1995 and was subsequently maintained at a lower level. The investigators found no indication that the effects of this earlier intervention lasted beyond late 1995.
- 24 Reductions in total aggravated assaults and robberies were smaller than those observed for gun assaults and armed robberies and were not clearly significant relative to changes in the comparison area (based on changes from the same 90-day period of the prior year). Further breakdowns show that the total number of assaults and robberies without guns in the north area remained the same (64) across the two periods (calculated from McGarrell et al. 2000, Tables 3–8 and 3–11). Also, burglaries rose by 20% in both target areas while declining 2% in the comparison area.
- 25 Separate analyses of each adjacent beat reportedly provided very little evidence of increases or decreases in total homicides, aggravated assaults, robberies, burglaries, or vehicle thefts.
- 26 A sensitivity analysis suggested that differences were more pronounced between the target areas and the three comparison areas that did not include the patrol zone encompassing the central business district.
- 27 Estimates are calculated by subtracting the change in the comparison areas from the changes in the intervention area.
- 28 Comparing changes during the program and nonprogram nights in the intervention zones avoids the potentially confounding effect of differences between the intervention and comparison areas. Examining the same changes in the comparison areas controls for the possibility that the program and nonprogram days followed different trends throughout the city.
- 29 Each estimated percentage decline is based on the estimate of crimes prevented divided by the sum of observed crimes and crimes prevented. The investigators used one-tailed tests for statistical significance; the DDD estimate for gunshot injuries would not have been significant with a two-tailed test.

- 30 Another supplemental analysis suggested that accidental gunshot injuries declined in the target areas during the intervention period. This could also signify that the estimated program impact on assault-related injuries was due in part to more general causes insofar as a link between gun patrols and accidental gun injuries is less intuitive than one between gun patrols and criminal gunshot injuries (the investigators also felt that the drop in accidental injuries was unrelated to the program).
- 31 In 1994, Cali's homicide rate was 124 per 100,000, while Bogotá's was 68 per 100,000. By way of comparison, the homicide rates in the Indianapolis north and east target areas in 1996 were 90 and 50 per 100,000, respectively.
- 32 The investigators refer to their study design as interrupted time series with multiple replications. Although the design is most appropriate when the intervention is introduced randomly rather than on a regular basis (Campbell and Stanley 1966, pp. 43–46), which was not the case in this application, the study met all criteria for inclusion in this review.
- 33 These deviations were not documented explicitly in the evaluation report.
- 34 Some of the methods used are not legal in all countries, so inferences should be made cautiously.
- 35 If the former is true, impact estimates at the city level may not reflect impacts in smaller target areas.
- 36 The Pittsburgh DDD estimates cited in the text are not directly comparable to the before-and-after changes presented for Kansas City and Indianapolis. However, the DDD estimates are not much different from the average before-and-after changes that occurred in the Pittsburgh intervention areas during the portion of the week when the gun patrols were conducted (i.e., Wednesday to Saturday); the latter changes were 32% for shots-fired calls and 64% for gunshot injuries (calculated from Cohen and Ludwig 2003, pp. 231, 235).
- 37 Meta-analysis commonly involves calculating standardized effect sizes for each study and averaging these measures across studies (Lipsey and Wilson 2001).
- 38 Exceptions may be made for areas of policy and practice in which randomized studies are not feasible or ethical. Given the rarity of randomized trials in criminal justice, particularly for area-based interventions, there may be benefits to conducting meta-analysis with some nonrandomized, criminal justice studies.
- 39 The level of lawful gun ownership and carrying in a jurisdiction is another largely unexamined factor that may affect the implementation of and community reaction to gun patrols. Most U.S. states have laws entitling persons without criminal records to permits for carrying concealed firearms, and the majority of these laws were passed during the late 1980s and 1990s. Although only a small percentage of the population has such permits, there will likely be more contacts between police and legal gun carriers when gun patrols are utilized in states with less restrictive laws on gun carrying. Indianapolis and Pennsylvania are two such states, and the Indianapolis evaluation reported that gun patrol officers encountered more firearms carried legally than illegally.

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