

POLICING CRIME GUNS
Research in Brief

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Abstract

Homicides in the United States, which disproportionately involve firearms, remain more common than in most other developed nations, despite the substantial decline in lethal violence observed in America during the 1990's. Can police patrols that are dedicated to reducing illegal gun carrying help reduce gun violence? Based on an article published by the Brookings Institution (Cohen & Ludwig 2003), this brief reports on an evaluation of a 1998 Pittsburgh policing patrol program focused on illegal gun carrying in high-crime areas during the highest-risk periods. The results of this evaluation are consistent with the idea that additional police patrols targeted against illegal gun carrying may yield substantial reductions in gun violence. The Pittsburgh policing program focused on illegally carried firearms in two of the city's highest crime areas during the peak crime periods of 8pm to midnight, Wednesday through Saturday. The additional five-officer, three-vehicle targeted patrols represented a 25 to 50 percent increase in patrol officers in these high-crime communities. The analysis suggests that the patrols contributed to as much as a 71% decline in hospital-treated assault-related gunshot injuries. No citizen complaints were filed against the Pittsburgh police as a result of the new directed-patrol program. If our evaluation results are correct, directed police patrols against illegal gun carrying may be a very cost-effective way to reduce gun violence. The \$35,000 cost of supporting the additional police patrols may yield benefits to the citizens of Pittsburgh from fewer gunshot injuries that are worth as much as \$25 million.

1. Introduction

Can targeted police patrols against illegal gun carrying reduce gun violence? The answer to this question is of considerable importance given that almost 17,000 Americans are the victims of homicide each year, nearly two-thirds of whom die as a result of firearms.¹ The cost to American society from criminal gun violence may be as high as \$80 billion per year (Cook and Ludwig, 2000).

Since most firearm homicides in the United States occur in public spaces², efforts to reduce the carrying of firearms in public may in principle reduce criminal gun use even without affecting the overall prevalence of gun ownership. Police patrols may help “incapacitate” those who would carry guns illegally by arresting such individuals and removing them from the streets, or may lead to a more narrow (but perhaps unusually effective) type of incapacitation by confiscating illegally carried guns. The threat of arrest and gun confiscation may also deter some people from carrying guns illegally, a possibility that receives some support from evidence of deterrent effects that result from threats of punishment in other contexts (Nagin, 1998, Levitt, 2002).

Previous research on the effects of police patrols against illegal gun carrying is limited but promising. Two widely-cited targeted patrol demonstrations, implemented in Kansas City and Indianapolis, respectively, find that neighborhoods which received stepped-up patrol activity experienced declines in gun crimes compared to other neighborhoods within these cities (Sherman, Shaw and Rogan, 1995, Sherman and Rogan, 1995, McGarrell *et al.*, 2001). While these findings are suggestive, there remains

¹ <http://www.cdc.gov/ncipc/wisqars/>

² Our analysis of homicide incident reports from 5 cities (Chicago, Houston, Miami, Pittsburgh, and St Louis) for the years 1985 to 1995 finds that 59 percent of firearm homicides occurred outdoors compared to 48 percent of other homicides. (These city data will soon be web-

some uncertainty about the degree to which these differences in gun crime across neighborhoods are due to the patrol programs themselves.

The following Research in Brief presents new evidence on the effects of police programs against illegal gun carrying in Pittsburgh, Pennsylvania. As with Kansas City, Indianapolis and other “non-experimental” policy evaluations where police interventions are not randomly assigned across neighborhoods, isolating the effects of the police program is challenging. However several features of Pittsburgh’s 1998 policing program provide us with a unique opportunity to distinguish the effects of the program from those of other confounding factors that cause crime rates to vary across areas and over time.

The remainder of this Research in Brief is organized as follows. The next section describes the Pittsburgh policing program. The third section describes the data used to evaluate the program. The fourth section presents the main findings, while the final section discusses policy implications. More detailed results for this evaluation are reported elsewhere.³

2. Previous Research on Policing

Policymakers who are interested in reducing gun violence through changes in policing policies have essentially two general options -- changing the resources available for police activities, or changing the ways in which existing resources are deployed. The best available research suggests that additional expenditures on police may reduce violent crime, presumably including gun crime, even without substantial changes in the ways that police are deployed (Levitt, 1997, 2002).⁴ But given the variety of pressing uses for

accessible from the National Consortium on Violence Research Data Center.

³ Cohen, Jacqueline and Jens Ludwig. “Policing Crime Guns.” In Jens Ludwig and Philip J. Cook, Eds. *Evaluating Gun Policy*. Washington, DC: Brookings Institution Press. 2002.

scarce government revenues, it is also important to know whether gun crime may be reduced by changing how police are deployed, with little or no change in expenditures.

One way to increase the efficiency of law-enforcement expenditures is to focus police resources on the highest-risk offenders, since a small share of the population seems to account for the majority of the gun-crime problem (Kennedy, Piehl and Braga, 1996). This type of focus on high-risk people is at the heart of the widely-cited Boston Gun Project, which seeks to deter violent behavior by gang-involved youth. What no one disputes is that Boston experienced dramatic declines in gun violence following implementation of this program. What remains controversial, however, is how much of the decline can be attributed to the activities of the Boston Gun Project (see, for example, Braga, Kennedy, Waring and Piehl, 2001, versus Fagan, 2002). Additional evidence on the effectiveness of this approach may eventually come from applications in other cities, such as Los Angeles (Tita, Riley and Greenwood, 2003).

An alternative approach is to focus police resources on high-risk places or times, which is the general strategy behind the Pittsburgh policing program reported on here. One widely-cited program that follows this general approach was launched in Kansas City, Missouri. This program targeted a high-crime neighborhood for additional police patrols, which were focused on maximizing the number of pedestrian and motorist stops and searching for illegal guns. Gun crimes declined by nearly 50 percent in the targeted neighborhood, while gun seizures increased by more than 65 percent; no significant changes were observed for either measure in a comparison neighborhood elsewhere in the

⁴ While the criminological has for many years been mixed regarding the effects of additional police resources on crime (see, for example, Loftin and McDowall, 1982, Cameron, 1988, Marvell and Moody, 1996, Corman and Mocan, 2000 in addition to Levitt, 1997, 2002), the best of these studies generally find that more police, or more spending on police, is associated with

city (Sherman, Shaw and Rogan, 1995, Sherman and Rogan, 1995). However because the two Kansas City neighborhoods experienced somewhat different levels and trends of firearm offenses even before the new patrols were launched, some uncertainty remains about how much the patrols themselves contributed to subsequent differences in gun crimes.

Indianapolis implemented a similar targeted policing program in 1997. An area on the East side of the city was targeted for stepped-up vehicle stops for minor violations, as with the Kansas City program. An area on the North side of the city also received additional police patrols, but these were focused on stopping the most suspicious people rather than on maximizing the number of overall stops. The number of gun seizures increased by around half on the East side, but very little on the North side. Yet only the Northern section of the city experienced a decline in gun crimes that was larger than trends observed elsewhere in the city during this period (McGarrell et al., 2001). Whether the different experiences of the two target areas reflect differences in the effectiveness of patrols targeted at “high-risk people” rather than simply “high-risk places,” or instead are due to other unmeasured differences across Indianapolis neighborhoods, remains unclear.

3. Targeted Police Patrols in Pittsburgh

Pittsburgh shares a number of characteristics with other American cities that make the policing program evaluated in this paper of national interest. During the early 1990’s Pittsburgh, like most other American cities, experienced a substantial increase in homicide, a change that was driven largely by gun homicides committed by young minority males. Given the substantial degree of residential segregation by race in

reductions in crime.

Pittsburgh and most other American cities (Glaeser and Vigdor, 2001), the concentration of criminal gun violence among young black males leads to a substantial geographic concentration of gun homicides as well. The concentrated nature of the homicide problem in Pittsburgh and other cities suggest the opportunity for an intervention that narrowly targets resources on an identifiable law enforcement target—illegal gun carrying by youth in high-risk neighborhoods.

Pittsburgh's program was designed through collaboration between local police and researchers at Carnegie Mellon University with funding from the Alfred P. Sloan Foundation. Additional patrols were assigned to police administrative zones 1 and 5 (Exhibit 1), which together contain most of the city's high-crime neighborhoods (Exhibit 2). Each of the target zones includes around 35 census tracts and 15 neighborhoods spread out over nearly 10 square miles, and are home to 55,000 and 80,000 residents, respectively.

These target zones are quite diverse. In addition to containing some of the city's highest-crime areas, each also contains many census tracts that have relatively few crimes. Each zone contains 7 census tracts in which more than one-half of all residents are African-American. However, in around one-third of the census tracts in zone 1 and one-fifth of tracts in zone 5, fewer than 5 percent of residents are black.

Under Pittsburgh's firearm suppression patrol (FSP) program one additional patrol team was assigned to both zones 1 and 5, consisting of four officers and a sergeant (all in uniform) traveling in three vehicles—usually two marked patrol cars and one unmarked car. The teams in each zone worked four-hour shifts from 8pm to midnight twice weekly for a period of 14 weeks, from July 19 to October 24, 1998. These patrols

were focused on the high-crime evenings of Wednesday through Saturday nights. Specific patrol days were designated to assure a mix of different days covered in each zone. The most common pattern (found in half the weeks) was alternating days, Wednesday and Friday or Thursday and Saturday, in individual zones. During this period a total of 51 special patrols were fielded across the two zones involving nearly 1,000 officer-hours.

Like other “directed patrol” programs, Pittsburgh’s FSP patrols were relieved of their responsibilities to respond to citizen 911 calls and instead focused on illegal gun carrying. With the assistance of maps and reports of recent shots fired activity, patrol teams identified and targeted “high-risk places at high-risk times” within the target zones (Sherman, 2001). Officers looked for opportunities to initiate citizen contacts for the purposes of soliciting information and investigating suspicious activity associated with illegal carrying and use of guns. By way of comparison, in Indianapolis the Eastern part of the city adopted a place-based strategy that focused on maximizing the number of traffic stops, while the Northern part of the city focused on stopping only the most suspicious people within the target areas. Pittsburgh’s program falls somewhere in between these two models—the program involved pedestrian as well as traffic stops, and included some focus on suspicious people but stops were not limited to this group.

Implementation of this strategy differed slightly between the two treatment areas in Pittsburgh. In zone 5 the three police vehicles typically traveled together as a unit, while in zone 1 the vehicles patrolled individually. Despite the greater dispersion of police patrols in zone 1, the number of police contacts recorded during the program was greater in zone 5—perhaps due to this area’s higher initial crime level. Exhibit 3 shows

that compared to zone 1, zone 5 experienced around twice as many vehicle stops (27 versus 12), person contacts (118 versus 57), arrests (12 versus 6), and confiscated guns (5 versus 2).

Police contacts were initiated mainly through traffic stops and “stop-and-talk” activities with pedestrians in public areas. Carrying open alcohol containers in public and traffic violations were frequent reasons for initiating contact. When warranted for reasons of officer safety (usually because of suspicious actions or demeanor), these stops sometimes moved to the types of pat-downs on the outside of clothing to check for weapons that are allowed under the Supreme Court’s 1968 decision in *Terry v. Ohio*. When there was reasonable suspicion of criminal activity, the contact might escalate to more intrusive searches inside pockets, under coats, and in waistbands as part of an arrest.

Given the size of these target zones the “dosage” levels of the intervention may seem low in absolute terms—just one four-hour patrol detail every four days in each targeted patrol zone, covering just five percent of all available hours weekly. However the patrols covered almost 20 percent of high-risk times from 7pm to 1am daily, and 33 percent of high-risk times on high-risk days (Wednesday to Saturday) weekly. Moreover the three-vehicle, five-officer teams represented a substantial increment to customary patrol resources in the target police zones. Police vehicles increased by 20 percent and patrol officers by 25 percent in target zone 5, the city’s highest crime zone located in the east end of the city. The increases were even larger in the other target area (zone 1), with a 35 percent increase in vehicles and a 50 percent increase in officers. In what follows

we seek to identify the effects of these stepped-up, targeted patrol activities in zones 1 and 5 on several measures of gun carrying and misuse.

For a variety of reasons the Pittsburgh policing initiative was implemented in a way that seems to have addressed concerns about individual rights and police-community relations that are often raised with such programs. As part of a consent decree with the U.S. Department of Justice, the Pittsburgh police department issued explicit guidelines on when officers could engage in “Terry” pat-down safety frisks. The department also imposed specific reporting requirements of the circumstances that precipitated more intrusive searches of persons or vehicles and seizures of guns or other property. Notably, officers had to articulate the basis for their suspicion about criminal activity by the person(s) being searched. Participating officers in the firearm suppression patrols were specially selected by police command staff based on their demonstrated capacities in pursuing a proactive style of law enforcement tempered by a professional attitude and demeanor in citizen encounters. Perhaps as a result of these precautionary measures, no citizen complaints were filed as a result of activities by the firearm suppression patrols.

4. Outcome Measures

Our goal is to focus on outcome measures that capture illegal gun carrying and criminal misuse in Pittsburgh. Gun homicides are an obvious choice, although such events are too rare to be useful here given our research design. More frequent events such as gun robberies or assaults provide another possibility, although in practice previous research suggests that standard police incident reports are often unreliable about whether a gun was involved in these types of criminal events (McGarrell et al., 2001). Customary police-based measures like offense and arrest reports are also subject to

possible contamination by a police program that aims in part to increase levels of detection and arrest of illegal gun carrying. We rely, instead, on citizen 911 calls reporting shots fired, and gunshot injuries treated in hospital emergency departments—measures that are largely independent of police practices, but closely related to illegal gun carrying and misuse.

Data on shots fired comes from Pittsburgh’s 911 Emergency Operations Center, and includes information about the date, time and address of the reported incident. We attempt to eliminate duplicate calls for the same incident by excluding calls that either shared the same event number in the Emergency Operations Center system or were reported within five minutes and 2,000 feet of one another. We also eliminated reports that lacked information about the exact location of the event. Taken together these criteria eliminate a total of 27 percent of the 9,884 original shots-fired reports in our sample. Some support for the validity of this outcome measure comes from the substantial correlation between the frequency of shots fired calls and youth homicides both in the cross section across Pittsburgh census tracts in a given year, and over time for the city of Pittsburgh as a whole.

Gunshot injuries treated in hospitals serve as a complementary outcome measure, one that is not subject to the same potential reporting problems as shots fired and which provides a more direct indicator of actual gun violence. Data come from the injury surveillance system developed by the Allegheny County Health Department.⁵ An audit of gunshot injuries treated in hospital emergency departments indicated that data from four trauma centers in Pittsburgh capture over 90 percent of gunshot injuries treated in

⁵ The injury surveillance system is one of several being developed as part of a nationwide project led by the Harvard Injury Control Research Center. For more information about these data see:

hospitals in the Pittsburgh area (Coben et al., 1997).⁶ We focus on gunshot injuries due to assault, since these should be most sensitive to the policing intervention. We rely on the victim's zip code of residence to identify the police administrative zone where the injury occurred, since in Pittsburgh most homicide victims and offenders live in the same zone where the murder occurred.⁷

5. Findings

In the absence of random assignment of Pittsburgh's policing program across neighborhoods, the challenge for our evaluation is to isolate the causal effects of the intervention from those of other factors that drive variation in crime rates across communities over time. As with any non-experimental study there necessarily remains some question about whether our analysis has in fact successfully identified the program's effects. Nevertheless our effort to identify the program's impact is strengthened by some unique features of Pittsburgh's policing program. Specifically, our evaluation exploits the fact that the patrols were implemented in some parts of the city (police administrative zones 1 and 5, what we call the "treatment" areas) but not others (the "control" areas), during certain periods of 1998 (7/1/98 to 10/24/98) but not others, and, within this 14-week program period, on only some days of the week (Wednesday through Saturday).

Shots Fired. Exhibit 4 presents our main findings for daily shots fired during the four hours from 8 pm to midnight. The top row of Exhibit 4 shows that the treatment zones (1

<http://www.hsph.harvard.edu/hicrc/nviss/>.

⁶ In principle, perpetrators who are shot by victims or police during the commission of a crime may avoid medical treatment for fear of being arrested, but we suspect that such cases are quite rare in practice.

⁷ In our analysis of the 328 homicides that occurred in Pittsburgh from 1990 to 1995, we find that in 81 percent of cases the victim lives within the same police zone in which the murder occurred.

and 5) averaged .979 shots fired calls per day on Wednesdays to Saturdays over the 6 week period prior to the launch of the targeted police patrols. During the 14 week period when the police FSP program was in effect (designated as the “program” period) this figure declined to .670 per day. The difference between the “pre” and “program” periods is equal to $-.310$, which is statistically significant at the 5 percent level using a one-tailed test.

Some initially suggestive evidence that this decline in shots fired may be due to the policing program rather than other factors comes from several secondary comparisons. First, the control zones in Pittsburgh, which did not receive the directed-patrol intervention, experienced a much smaller decline in the number of shots fired calls on Wednesdays through Saturdays ($-.042$ in the control areas, as shown in row three of Exhibit 4, versus $-.310$ in the treatment areas).

Of course it is possible that the treatment and control areas would have experienced different trends over this period of 1998 even in the absence of the policing patrols. However, note that the treatment and control zones experienced similar trends in shots fired from the pre-program to program periods on the “off” days of the week when the patrols were *not* in effect (Sunday through Tuesday). The daily number of shots fired in the treatment zones increased by $+.258$ from the pre to program period during the “off” days (Exhibit 4, row two). Control zones experienced a fairly similar change over this period on these days, equal to $+.179$ (Exhibit 4, row four).

These various comparisons can be used together to more formally generate an estimate for FSP effects that accounts for various unmeasured zone-specific factors that remain stable or change over time. A simple “difference-in-difference” estimate focuses

The offender lives within the same police zone as the murder incident in 69 percent of homicides.

on comparing trends over time within the treatment zones during the days when the patrols are in effect (-.310) with those observed in the same treatment area on the days of the week when the patrols are not active (+.258). If the patrols have an effect, we would expect the decline to be greater in the treatment areas during the “on” days compared to the “off” days, as is the case ($-.567 = -.310 - .258$ is statistically significant at the 5 percent level). We would also expect this relative decline during the “on” versus “off” days to be greater in the treatment than control areas, since only the former experienced patrols during the program period. Exhibit 4 shows that this is also the case: the “difference-in-difference-in-differences” DDD estimate of $-.347 = -.567 - (-.220)$ is significant at the 5 percent level. The estimate, if taken at face value, implies that the FSP program reduced shots fired in the treatment areas during the “on” days by around 34%.⁸ This reduction from expected levels is shown graphically in Exhibit 5.

However, we also find a similar pattern of changes across treatment and control areas when we calculate the DDD program estimate for 1997, the year *before* the Pittsburgh firearm suppression patrols were implemented (not shown). This raises the possibility that unmeasured confounding factors are driving our estimates for 1998. Similar changes in 1997 cannot logically be due to a policing intervention that did not exist until the following year. These findings of a “phantom effect” in 1997 make us cautious about interpreting the differences in shots fired between the treatment and

⁸ We define the proportional magnitude of the treatment effect by comparing our DDD estimate for the program’s impact (-.347) with our estimate for the number of shots fired per day that would have been observed during the “on” days in the treatment neighborhoods in the absence of the program. This counterfactual outcome is equal to the rate that is actually observed during the program period (.670) plus the estimated treatment effect (.347), so the proportional reduction associated with the treatment equals $.347/ (.670 + .347) = 34\%$.

control zones in 1998 as signs of the patrol program’s effects. The results for gunshot injuries presented in the next section are more robust to our various specification checks.

Gunshot Injuries. Exhibit 6 presents average daily rates of assault-related gunshot injuries.⁹ During the “on” days of Wednesday through Saturday (Exhibit 6, row one) the number of assault-related gunshot injuries declined from the pre to program periods by $-.161$ (statistically significant at the 5 percent level), while such injuries barely changed in the control areas over the same period during these days of the week ($+.007$ in Exhibit 6, row three). The difference in trends between the treatment and control areas during the “on” days does not appear to be driven by unmeasured neighborhood-level factors, since the areas experienced fairly similar changes during the “off” days of Sunday through Tuesday when the patrols were not in effect ($+.103$ versus $+.050$ in rows two and four of Exhibit 6). Together these results also imply that the greater decline in the treatment areas during the “on” than “off” days ($-.161 - .103 = -.264$) exceeds that observed in the control areas ($.007 - .050 = -.042$). The formal DDD estimate is shown in the last row of Exhibit 6 ($-.264 - (-.042) = -.222$, significant at the 10 percent level using a one-tailed test), and implies that the policing program has reduced gunshot injuries by 71%.¹⁰ The reduction from expected levels of gunshot injuries is shown graphically in Exhibit 7.

We also find that the estimated effect of Pittsburgh’s firearm suppression patrols on gunshot injuries is remarkably robust to a variety of specification checks. For

⁹ The daily counts exclude accidental and self-inflicted gunshot injuries—that are rarely associated with carrying guns in public places—but include injuries any time during a day.

¹⁰ As seen in the first row of Exhibit 6, the average number of assault gunshot injuries per day in the treatment areas during the target days of the week (Wednesday - Saturday) when the patrols are in place equals $.089$. The estimated program impact of $-.222$ implies that the average assault gunshots per day would have equaled $(.222 + .089)$ in the absence of the police patrols against illegal gun carrying, so that the implied program effect equals $.222 / (.222 + .089) = 71$ percent.

example, the results are quite similar when we extend the number of weeks used to define the pre-program period, or partition the program period in half. We also obtain qualitatively similar results when we use a generalized least squares approach that corrects for serial correlation, or use models that weight each observation by the zone's population, or reduce some of the day-to-day variability in gunshot injuries by averaging over the three- or four-day "off" and "on" periods. Parallel analyses find significant reductions in accidental gunshot injuries, the combination of all gunshot injuries and gunshot injuries involving youthful victims under age 25. Most importantly, we do not find any systematic evidence of phantom program effects either in the pre-program year of 1997 or the post-program year of 1999. This last finding enhances confidence that the findings for gunshot injuries during the program period of 1998 are not driven by unmeasured confounding factors.

6. Conclusions

Our central finding is that the Pittsburgh firearm suppression patrols appear to substantially reduce both citizen reports of shots fired and assault-related gunshot injuries in the target neighborhoods. While we find some evidence of a "phantom effect" for our shots fired measured in 1997, the year before the police patrols are put into place, the findings for assault gunshot injuries hold up across a variety of specification checks. The implied effect of the policing program on assault-related gunshot injuries is quite large—equal to 71 percent.

The relatively modest number of guns confiscated and arrests made as a result of the Pittsburgh directed patrols suggest that "incapacitation" of illegal guns or their owners is unlikely to drive the large reductions that accompanied the policing program.

Moreover if the patrols reduce shots fired or assault gunshot injuries primarily through an “incapacitation” effect—by taking illegal guns and/or those who carry them off the street—we might expect the effects to carry over to the days of the week when the patrols were not in effect. This type of carryover does not appear to occur. Instead, the similarity in “off-day” trends between the treatment and control zones provides at least suggestive evidence of a deterrent effect that is specific to the treatment zones during the “on days” when the program is in effect, and only reduces illegal gun carrying and misuse during those times.

The general similarity in trends between treatment and control areas in gun misuse during the “off days” also presumably rules out the possibility of significant displacement of gun carrying and misuse from “on” days to “off” days. The absence of significant trends upward in the control areas during the “on” days (Wednesday-Saturday) in the program period also seems to rule out spatial displacement where gun-carrying offenders shift their activities from the treatment to control neighborhoods.

Given the substantial costs of gun violence on American society, the results presented here confirm those in Kansas City and Indianapolis and suggest that police patrols targeted against illegal gun carrying may be very cost-effective. We estimate that the costs of the additional patrols in Pittsburgh are quite modest—something less than \$35,000 in overtime expenditures over the 14 week program period. If the program effects estimated in this paper are correct, that investment may yield benefits to the citizens of Pittsburgh on the order of \$25 million (Cook and Ludwig, 2000, Ludwig and Cook, 2001).

Yet policymakers should be aware of a number of important limitations to the research reported here before substantially expanding the scale of directed patrols against illegal gun carrying. Pittsburgh's experience shows that with enough training and care, a small-scale targeted patrol program against illegal guns can be implemented in a way that addresses community concerns about intrusive policing. This conclusion stems from the observation that no citizen complaints were filed as a result of the program. However, whether similar results could be obtained with a larger-scale effort remains unclear. Moreover our confidence that such targeted patrols are responsible for reducing illegal gun carrying and misuse would be even stronger if the findings reported here could be replicated using a similar research design across additional sites. Finally, our evaluation has not considered other potentially important outcome measures such as those related to police officer safety or morale.

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Exhibit 1
Police Zones in Pittsburgh

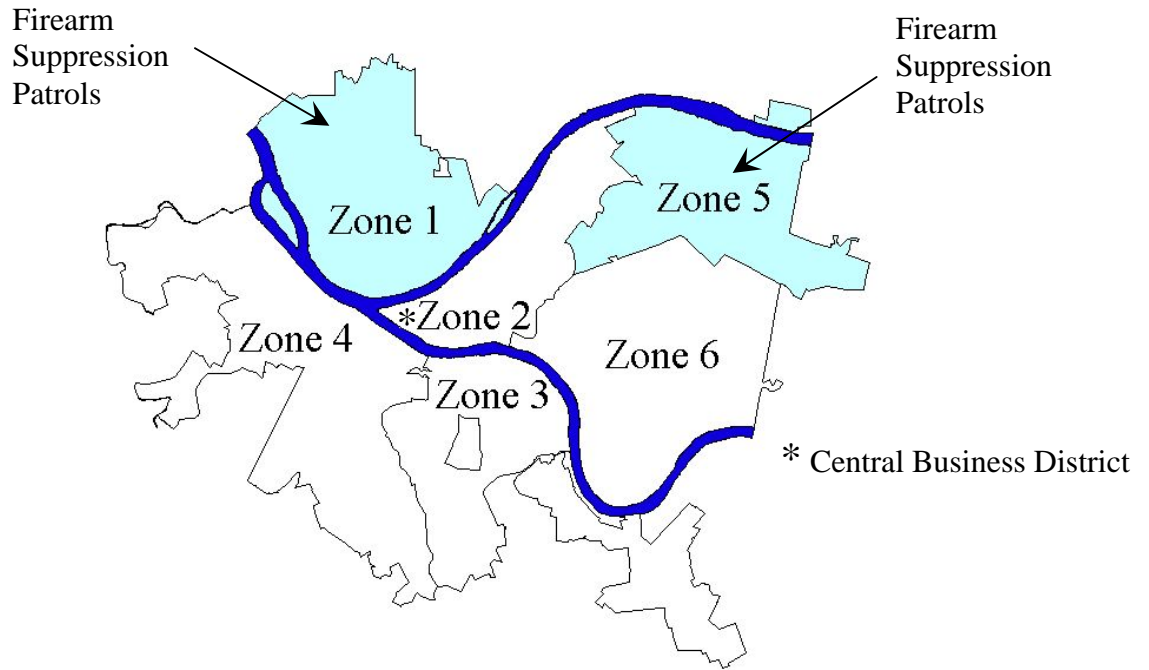
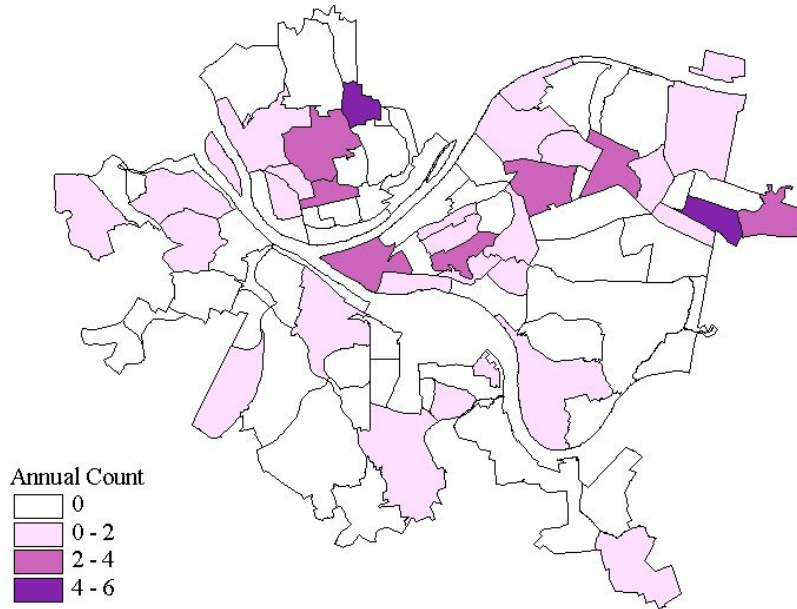


Exhibit 2
Neighborhoods with Youth Homicides in Peak Year 1993 in Pittsburgh
(Youth Incidents are 84% of Total) ^a



^a Youth incidents involve offenders or victims ages 12 to 24.

Exhibit 3 Enforcement Activities During Firearm Suppression Patrols

Activities	Zone 1	Zone 5	Total
<u>Person Contacts:</u>	57	118	175
Contact With Pat Down/Search	13	21	34
No Pat Down	44	97	141
 <u>Vehicle Stops:</u>	12	27	39
Stolen Vehicle Recovered	1	3	4
<u>Citations:</u>	4	21	25
Vehicle	3	11	14
Open Container/Alcohol	1	7	8
Disorderly/Noise/Nuisance	0	3	3
Warnings / Other ^a	17	37	54
<u>Search/Seize:</u>	2	1	3
Gun and No Other Contraband	1	1	2
Gun and Other Contraband	1	0	1
Other Contraband Alone	0	4	4
Nothing Found	11	17	28
<u>Arrests:</u>	6	12	18
With Gun	1	2	3
No Gun	5	10	15
 Guns Confiscated	2	5	7
 <u>General Activities with No Contacts:</u>			
Pursuit—No Contact ^b	17	24	41
Patrol—No Contact ^c	171	46	217
Assist Non-Firearm Suppression Unit	32	30	62
Non-Firearm Suppression Activities ^d	3	3	6
 <u>911 Calls:</u>	55	50	105
911 Disposition--No Report Necessary	51	42	93
911 Person Shot	0	2	2
911 Shots Fired	13	21	34
911 Person(s) with Gun ^e	16	8	24
911 Stolen Gun	0	0	0

(NOTES are on next page)

- ^a “Other” includes warning without citation and requests that individuals “move along.”
- ^b Combination “Viewed only” and “Pursuit—No Contact” used for actors who fled or dispersed on police arrival.
- ^c “Patrol—No Contact” also includes a few instances of stationary surveillance of an area and tactical foot patrols.
- ^d “Non-FSU Activities” include supervision of officers and investigation of possible stolen car parked on street.
- ^e “Person(s) with Gun” includes armed robbery calls.

Exhibit 4
Impact Estimates for Shots Fired (Daily Average, 8PM to Midnight)

<i>Treatment Zones</i>	Pre-Period (6 weeks before)	Program Period (14 weeks during)	<u>Estimated Impact</u>
<i>On Days:</i> Wednesday-Saturday	0.979	0.670	Difference = -0.310 (0.131)**
<i>Off Days:</i> Sunday-Tuesday	0.444	0.702	Difference = 0.258 (0.219)
			Difference-in-differences = -0.567 (0.088)**
<i>Control Zones</i>			
<i>On Days:</i> Wednesday-Saturday	0.323	0.281	Difference = -0.042 (0.072)
<i>Off Days:</i> Sunday-Tuesday	0.208	0.387	Difference = 0.179 (0.129)
			Difference-in-differences = -0.220 (0.120)*
	Difference-in-difference-in-differences = -0.347 (0.133)**		

NOTES: Results come from estimating daily shots fired during the four hours from 8 PM to midnight in each of Pittsburgh's 6 police zones. Standard errors in parentheses are adjusted to account for non-independence of observations drawn from same police zone. * = statistically significant at the 5 percent level; ** = statistically significant at the 1 percent level (one-tail z test for reductions).

Exhibit 5
Reduction in Shots Fired During
Police Firearm Suppression Patrols (FSP) in Pittsburgh, PA

(Average Daily Count of Shots Fired from 8 pm to Midnight in Each Treatment Area)

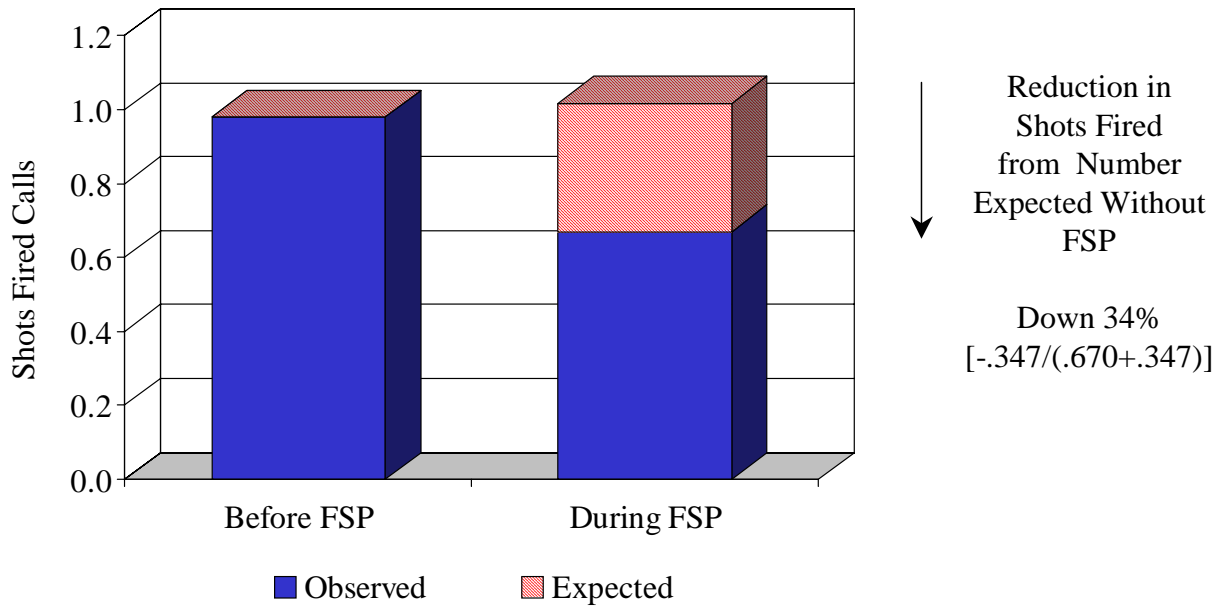


Exhibit 6
Impact Estimates for Assault-Related Gunshot Injuries (Daily Average)

<i>Treatment Zones</i>	Pre-Period (6 weeks before)	Program Period (14 weeks during)	<u>Estimated Impact</u>
<i>On Days:</i> Wednesday-Saturday	0.250	0.089	Difference = -0.161 (0.096)**
<i>Off Days:</i> Sunday-Tuesday	0.028	0.131	Difference = 0.103 (0.112)
			Difference-in-differences = -0.264 (0.208)*
<i>Control Zones</i>			
<i>On Days:</i> Wednesday-Saturday	0.073	0.080	Difference = 0.007 (0.033)
<i>Off Days:</i> Sunday-Tuesday	0.028	0.077	Difference = 0.050 (0.027)
			Difference-in-differences = -0.042 (0.042)
	Difference-in-difference-in-differences = -0.222 (0.165)*		

NOTES: Results come from estimating daily gunshot injuries from assaults in each of Pittsburgh's 6 police zones. Standard errors in parentheses are adjusted to account for non-independence of observations drawn from same police zone. * = statistically significant at the 10 percent level; ** = statistically significant at the 5 percent level (one-tail z test for reductions).

Exhibit 7
Reduction in Assault Gunshot Injuries During
Police Firearm Suppression Patrols (FSP) in Pittsburgh, PA

(Average Daily Count of Assault-Related Gunshot Injuries in Each Treatment Area)

