



# Police Officers on Drug Corners in Philadelphia, Drug Crime, and Violent Crime: Intended, Diffusion, and Displacement Impacts

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On May 1, 2002, the Philadelphia Police Department launched Operation Safe Streets, stationing officers at 214 of the highest drug activity locations in the city 24 hours a day, 7 days a week. Interrupted time series (AutoRegressive Integrated Moving Average) models on weekly data isolated citywide and local program impacts on all violent crimes, murder, and reported drug crimes. Results showed no significant impacts on citywide weekly counts for drug crimes, homicides, or all violent crimes. Geographically focused analyses showed significant localized intervention impacts for both violent and drug crimes. Analyses of high-drug-activity non-intervention sites suggest: the program impacts seen were not an artifact of history or local history; significant spatial diffusion of preventive benefits for violent crime; and probably significant spatial displacement for drug crime. Stationary targeted drug-enforcement interventions like Operation Safe Streets may differentially affect the locational selection processes behind violent crime versus drug crime.

**AQ1**      *Keywords*    drug corners; drug crime; Philadelphia; police officers; violent crime

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## Introduction

Police crackdowns often take place in highly political contexts, usually in response to a crime problem seen as increasingly serious, and requiring something beyond "business as usual" policing. Given such a context, these efforts are often closely scrutinized by media, speedily proclaimed as successful by their sponsors, and critically questioned by political opponents. Extensive crackdowns often require considerable additional financing as well from already hard-pressed city coffers, ratcheting up further the levels of concern and scrutiny, and the pressure for payoffs.

In Philadelphia, the core county of the fourth largest primary metropolitan area at the time, "business as usual" policing was no longer effective in the late winter and early spring of 2002.<sup>1</sup> Mayor John Street, former civil rights activist and City Council President, was about halfway through his first term. His nationally respected police commissioner, John Timoney, had departed for Miami, Florida after successfully policing the protests surrounding 2000 Republican presidential convention. In December 2000, the city's largest one day multiple homicide—the "Lex Street Massacre"—had left behind seven dead bodies and three injured persons in a crack house in a deteriorated west-side neighborhood. Authorities characterized it as a drug turf battle. In June of that year (2002), the city would learn, as the Mayor probably had earlier, that the four people held as suspects for 18 months were the wrong people. They were released just as the trial was about to start.

There were several weeks in this period when the city saw 12 homicides, and one where 13 took place. Even though the police department, under the Acting Commissioner Sylvester Johnson, a 38-year department veteran, was continuing the COMPSTAT process started under Timoney, something more was needed. Mayor Street, an African American from a section of North Philadelphia where drugs and violence were problematic, was under mounting pressure to do something dramatic and effective.

In 1998, under Timoney, the Philadelphia Police Department conducted Operation Sunrise,<sup>2</sup> a targeted, anti-drug crackdown and clean-up effort in one section of North Philadelphia, Hunting Park. Street and Johnson, looking to the successful Operation Sunrise as a model, knew they needed something similarly targeted and effective. But it also had to be citywide so that no drug-afflicted neighborhoods were left behind. The Police Department identified high-drug-use locations using crime data, arrest data, firearms-seizure data, informant data,

1. According to the 2000 Census, the population of the Philadelphia primary metropolitan statistical area (PMSA) was 5.1 million, smaller than the New York City-New Jersey, Chicago, and Los Angeles PMSAs but larger than Dallas-Forth Worth at 3.5 million.

2. "(A) police-led effort to fight drugs and blight in Kensington and North Philadelphia, in 1998 ... The campaign, in which police cooperated with other city agencies cleaning up abandoned houses and towing abandoned vehicles, has been widely credited with improving the quality of life in those neighborhoods. Despite a record number of arrests, police were careful in choosing their targets, avoiding the mass sweeps that provoked civil-liberties questions about some previous police crackdowns" (Marcus 2002).

and ongoing investigations, and obtained partial funding from the Bureau of Justice Assistance for police overtime pay.

On May 1, 2002, the Philadelphia Police Department launched Operation Safe Streets, stationing officers at 214 of the highest drug-activity locations in the city. This crackdown received extensive public attention and was widely touted by the Police Department and Mayor Street as a success. "Anniversary" events on May 1, 2003 referred to enhanced neighborhood safety and reduced fear in neighborhoods throughout the city. Johnson noted, "Safe Streets is not only about statistics, even though our statistics are good. The main thing Safe Streets is accomplishing is improving the quality of life. I think that is more important than any statistic. You can't measure statistics" (Wexler, 2003). The Police Department, in regular briefings, however, during this period attributed citywide reductions in several crime classifications to Operation Safe Streets. "Philadelphia's murder rate this year could be lowest since the mid-80's ... All of our major crime numbers are going down" (Davies, 2002).

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This research examines this assertion, focusing on 121 weeks of pre-treatment and 18 weeks of treatment data. Because close to 3 years of citywide violent crime data are available, it is possible to apply time series models to "control" for the natural variation in weekly violent crimes and gauge the net impact of Safe Streets during its first 18 weeks of operation.

More specifically, it was possible to determine if immediate, citywide crime reductions followed the program's implementation. In addition, these data permit us to determine whether the program was implemented after a crime peak, a common sequence of events surrounding crackdowns (Glass, 1968). Additionally, because geocoded crime data were available, it was possible to examine geographically *localized* program impacts, seeing if areas immediately surrounding the target locations became safer. If they did, we also ask two questions. Did this increased safety link to spatial displacement, with the same crime type going up in nearby locations? Or did the increased safety link to a diffusion of benefits, with the same crimes also decreasing in locations somewhat farther away from the intervention sites?

### Problem-Oriented Policing

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Geographically targeted crackdowns represent examples of problem-oriented policing (Goldstein, 1990). Problem-oriented policing is "... (An approach where the) police go beyond individual crimes and calls for service, and take on the underlying problems that create them" (Eck & Spelman, 1987). Placing a police officer on a drug corner represents an attempt to suppress a localized drug market, an underlying set of activities thought to spawn not only drug crimes but other serious crimes, and local neighborhood quality-of-life problems as well.

Wilson and Kelling (1982) suggested that police can be more effective if their role is more place-specific. Crackdowns, if they focus on hot spots of crime (Sherman, Gartin, & Buerger, 1989) can be so focused. These alternatives appear

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more promising than traditional policing (Weisburd, 1997). This is not to suggest, however, that once implemented these interventions always have proven successful or that initially successful crackdowns remain successful over time.

Numerous studies have examined the effects of patrol levels. Much work suggests varying patrol levels or types do not affect crime rates (Felson, 1994; Kelling, Pate, Dieckman, & Brown, 1974; Klockars, 1983). At the same time, however, ecological research shows crime is distributed unevenly in space and time (Cohen & Felson, 1979; Ratcliffe, 2002; Sherman et al., 1989), in part because motivated offenders are attracted to areas where large numbers of potential victims and/or crime targets are available (Rhodes & Conley, 1991). This decision to offend in a particular area is partially based on a cost-benefit analysis by the offender given available alternatives (Clarke & Cornish, 1985; Eck & Weisburd, 1995). The amount of police in the vicinity would seem a relevant consideration for potential offenders.

Therefore, it makes sense to concentrate police in areas previously or potentially vulnerable to offending (Sherman & Weisburd, 1995). Increasing concentrations of police officers in "hot spots" should increase the offender probabilities of being detected and/or apprehended. Certainty of punishment may be a greater deterrent to criminal activity than severity of punishment (Blumstein, Cohen, & Nagin, 1978). Using a problem-oriented SARA model, police-deployment decision-makers can target intervention locations, gauge results, and make modifications as needed (Eck & Spelman, 1987).

The Operation Safe Streets strategy examined here used a variety of sources to select initial target locations and the intervention mode—pairs of officers on corners. Later in the first year of intervention, however, the police department changed the mix of target sites as the crime problems shifted, and added new deployment tactics such as bike police in a few of the locations. In short, beyond the period documented here, police modified the intervention as they sought to both contain costs and respond to shifts in offender locations and activities. But during the implementation period covered by our data, the program remained in its original form.

As the program proceeded, its outcome focus also evolved. Initially, police personnel focused on declining murders as a result of the intervention. But as some questions surfaced about these numbers, and as it proved difficult to keep those numbers going down, Mayor Street and Commissioner Johnson shifted some of the focus to improved neighborhood life as a result of decreased drug activity and less disorderly behavior on the targeted blocks. According to Johnson,

The way I measure how we are doing is by going out to the communities, talking to people and asking how they feel about the safety in their neighborhoods. No matter how many arrests we make, if people still cannot come out of their houses and children cannot go outside and play, the we really haven't done anything. (Wexler, 2003)

Such claims are fully in line with "broken windows" models of policing (Wilson & Kelling 1982). This thesis claims that reductions of physical and

AQ2 social problems in a neighborhood both reduce residents' fear and cause later crime drops. The thesis is highly controversial (Harcourt, 2001; Taylor, 2001). We do not seek to evaluate outcomes related to that thesis here. We simply point out that some current theorizing behind policing strategies points to these other outcomes, and some of the architects of the current effort made reference to gains on these other outcomes. Regrettably, we cannot gauge such claims.

### Specific Crackdown Studies

The term "crackdown" has been applied to a wide range of police actions. For the purpose of continuity, this research adopts Davis and Lurigio's (1996) definition of a crackdown as "... abrupt escalations in proactive enforcement activities that are intended to increase the perceived or actual threat of apprehension for certain offenses occurring in certain situations or locations" (p. 86). Crackdowns usually involve high police visibility and some publicity.

The research on crackdowns covers a range of topics from speeding to DUI crackdowns to, more recently, drug markets and crime or drug hot spots. This work suggests three points:

- (1) Sometimes the crackdown comes after the problem has peaked (e.g., Glass, 1968).
- (2) Crackdown effects, if there are any, are often transient and disappear relatively quickly (Ross, 1982).
- (3) The effects of crackdowns also can vary from location to location.

Looking across outcomes, and events within outcomes, the empirical work suggests mixed results. Some programs have been found to have a positive impact on drug and violent crimes (Kleiman, 1988). Other research has found little impact on reported crime, but a strong reduction in disorders (Sherman & Weisburd, 1995).

Some work suggests geographically targeted crackdowns work better on violent or property crimes than drug crimes. For example, Braga *et al.* (1999) measured the impact of a problem-oriented policing program to control violent places in Jersey City, New Jersey. Areas with consistently reported high levels of violent crime over time were identified. Compared to control areas, locations receiving the program experienced a significant decrease in robbery and property crimes. No impacts on narcotics crimes or disorder problems, however, appeared. These results are contrary to Gabor's (1990) suggestion that geographically targeted interventions will have less impact on violent crimes due to the fact that violent crimes are less likely to be clustered in time and space. Other studies, however, have documented impacts on physical disorder problems and drug problems. Green (1995, 1996) found that police intervention complemented by multi-agency support services resulted in less physical disorder and drug

activity. Her work also showed, using individual-level contact data, a spatial diffusion of benefits suppressing drug activity.

Other research has examined the impact of crackdowns on calls for services. Weisburd and Green's (1995) longitudinal analysis of problem-oriented policing efforts in Jersey City found a marked decrease in calls for service following the police intervention. Further, their research suggested little spatial displacement but rather a spatial diffusion of benefits. Sherman *et al.* (1995) found subsequently lower call levels and offences after police raids on blocks reporting numerous calls for service. Impacts of this intervention, however, diminished after 2 weeks.

Varying the length of police presence results in different impacts. In high-crime areas, Koper (1995) found that simply having officers drive through an area had little impact on criminal activity. The extended presence of an officer for more than 10 minutes, however, resulted in a marked decrease in criminal activity in the immediate area.

Crackdowns provide a visible and dramatic gesture in response to serious, increasingly serious, or increasingly political crime problems. The strategy has been applied across a range of outcomes. It holds considerable appeal as an "immediate" solution. Several difficulties deserve attention before overall positive benefits can be confirmed. First is the timing question. Is crime reduction occurring naturally after a problem has peaked, or is it a response to the intervention (Glass, 1968)? Addressing this question requires an analytic technique such as an Autoregressive Integrated Moving Average (ARIMA) interrupted time series. Second is the question of effects over time. Several studies found immediate positive responses to crackdowns (Braga *et al.*, 1999; Koper, 1995; Weisburd & Green, 1995). But others found effects fading over time (Ross, 1982). Our dates here permit examining program impacts for a third of a year following implementation, but no further. We recognize, therefore, that the conclusions we reach are limited to this period. But within this period, we can see if benefits are occurring and, if so, if they are continuing. Third is the question of displacement effects versus diffusion of benefits. Because we were able to geocode our data, we can examine possible spatial externalities, and see if Safe Streets made matters better or worse for those slightly further from the intervention sites. Before considering this possibility, however, this research will examine jurisdiction-wide impacts, as were claimed by the program planners.

Of particular interest is whether the implementation of a program such as Operation Safe Streets will have similar impacts across a broad range of offenses. Will violent crime and drug crime be affected in the same manner by the police effort? On this topic, there are questions of both overall impact and spatial patterns of displacement vs. diffusion of benefits.

Drug offenders involved in drug activity carefully choose the location in which to commit their offenses (Eck, 1995; Hagedorn, 1994; Simon & Burns, 1997) and are sensitive to temporal changes in conditions. They are setting up markets. Simon and Burns (1997) even describe drug sellers timing their peak activity hours to coincide with police district shift changes. If this is so, it seems plausible

that we might *not* see a diffusion of benefits with drug crimes, especially since the initial intervention fixed officers “on post” on specific corners, and the operation received widespread publicity upon launch. Rather, the drug sellers may move just a little bit away and show a pattern of spatial displacement.

By contrast, violent criminals are a more diverse group of offenders, and generally their crime planning is less detailed. Events are more likely to be spur of the moment (Wright & Decker, 1997), and they travel shorter distances from home to commit their offenses (Brantingham & Brantingham, 1981). Therefore, their offense location selection processes, being less “fine-tuned,” may not attend as closely to specific officer locations. They make think it wisest to give any areas near a stationed officer a wide berth. It seems plausible, therefore, that we could see a spatial diffusion of benefits for violent offenses.

In sum, our focus is as follows. First, we will see if the intervention led to citywide reductions in all violent crimes, murder separately, and drug crimes, as claimed by its architects. Second, we will see if violent crimes and/or drug crimes were reduced in the intervention sites themselves. Third, we will explore patterns of spatial displacement vs. diffusion of benefits, anticipating we may see the former for more attentive drug marketers, and the latter for violent offenders who may, in comparison, engage in less “rational” and less fine-grained spatial planning.

## Methods

Data were provided by the Philadelphia Police Department to the *Philadelphia Daily News*, a leading daily newspaper. Individual data were aggregated to weekly counts of reported crime using Philadelphia’s Uniform Crime Report codes.<sup>3</sup> The crime category of homicide is composed of all reported homicide and manslaughter crimes. The violent-crime category includes all rapes, robberies, and aggravated assaults in addition to homicide and manslaughter. The drug crimes include all types of selling, possession, and manufacturing of illegal drugs. Interrupted time-series analysis ARIMA was used to analyze weekly counts of homicides, violent crimes, and drug crimes. For those geographically focused analyses, weekly crime counts were transformed into weekly counts/km<sup>2</sup> given the different size areas in target and adjoining buffers.

The dataset included all reported crimes in these categories from January 1, 2000 through August 31, 2002. The Operation Safe Streets intervention began

3. Although Philadelphia was widely known to have misreported UCR crime data in 1997-1999, reform efforts spearheaded by Timoney during his tenure as Commissioner appears to have improved the reliability of police crime reporting. We are not aware of major, systemic complaints about Philadelphia Police Department crime reporting from this data period. Further, for such a potential threat to internal validity to provide a plausible counter-explanation for the impacts we observe here, one would have to presume that whatever department-level reporting problems existed dramatically intensified just when Operation Safe Streets was launched, and especially in the specific intervention areas. This seems less than plausible because it would require coordinated, systematic collusion among a very large number of officers. .

the week of April 29, 2002.<sup>4</sup> This provided 139 weeks of data, with 121 of these weeks measuring reported crime before the intervention of Operation Safe Streets, and 18 weeks after. The substantial number of weeks pre-intervention permits identification of relatively complex model structures with this set. The limited 4 month period of time post-intervention means the analyses will be unable to observe if program effects later waned.<sup>5</sup>

AQ2 ARIMA requires a process of model identification followed by one of model estimation.<sup>6</sup> Identification compares errors associated with different model fits, based on the Autoregressive function, the differencing function and the Moving Average of the model (see Box & Jenkins, 1976). The most parsimonious initial model, with just ARIMA parameters, is determined through fit indices (AIC, SBC). In addition, such a model should have autocorrelation and partial autocorrelation structures which are only "white noise" with non-significant Box-Ljung statistics and partial autocorrelations of less than two standard errors. In the second step using the previously selected model, a dummy variable is added to test for the Safe Streets intervention impact. The models include a seasonal control operationalized as a temperature variable.<sup>7</sup>

Initial ARIMA models examine program impacts across the city. Additional analyses search for local impacts on target and adjoining locations. For the latter two analyses, all homicides, all violent crimes, and drug crimes were geocoded. Geographic Information Software (GIS) results revealed a 97.4 percent hit rate.<sup>8</sup>

Circular buffers of 0.1 (one tenth) mile were constructed surrounding the Operation Safe Streets sites, and from 0.1 to 0.2 mile from the intervention location. One tenth of a linear mile is roughly equivalent to one street block length in many parts of Philadelphia, so crime changes here are "in" the treatment area. Those from 0.1 to 0.2 mile are in the adjoining area, changes there potentially reflecting spatial crime displacement or spatial diffusion of a prevention benefit. Incidents that fell within multiple buffers due to the close proximity of

4. The intervention started 2 days later on May 1. Additional analyses were conducted leaving the week of 4/29 as a pre-treatment week; results were not markedly different. To test for a lag in benefits or an anticipatory effect of the program, the startup date was allowed to vary for several weeks both before and after the initial startup date, with no enhancement of impact coefficients.

5. Repeated requests made by the Philadelphia Daily News to the Philadelphia Police Department to obtain additional data and extend the observation period were denied.

6. One reviewer of an earlier draft suggested the traditional ARIMA two-step model specification and estimation is no longer a favored method of analysis, because the two-step procedure relies too heavily on model specification by the researchers and can be viewed as a form of data mining. To allay these concerns, we conducted additional analyses using the simplest ARIMA model (1,0,0), a simple serial auto-correlation model. In this way, we were still able to focus on the longitudinal aspect of the data with minimal researcher specification. Results were very similar despite the lack of model specification, with the exception of an observed county-wide reduction in violent crime and a localized reduction in drug crimes in the adjoining areas.

7. Additional ARIMA models were run in which a time variable was included in lieu of the temperature variable. Monthly dummy variables failed to improve the model's explanatory power. Although a model with weekly dummy variables did result in a marginally better fit, it failed to significantly alter the coefficients of the independent variables. For parsimony's sake, we report here only the model using the temperature variables.

8. Chi-square analyses showed no significant relationships between geocoded status and crime type.



several Safe Streets sites were included only in a single buffer and linked to the intervention site, the shortest distance away.

The Operation Safe Streets intervention took place at 214 locations across the city. Thirty-four of these locations were defined as the cross-section of two streets, with the remaining 180 sites defined as a single address.

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Operation Safe Streets identified high-drug-activity locations as intervention points.<sup>9</sup> Serendipitously co-occurring changes in local drug activity and related crime patterns might cause the localized analyses to show as a program impact something due to history or local history in the most drug-afflicted parts of the city. To check for such a possibility, we identified high-drug-activity locations, prior to the implementation of Safe Streets, which were *not later* identified as initial program target sites. Using *Crimestat*<sup>10</sup> (2002), we identified 73 “matched”<sup>11</sup> comparison sites, after eliminating those that were program sites. The localized analyses should not show comparably sized crime-prevention impacts in these comparison locations. If we see an elevation effect following implementation in these comparison sites, it might be indicative of more spatially distant displacement effects. If we see crime-prevention impacts in comparison sites, of roughly equal size to the prevention impacts in target sites, it suggests we are seeing a timed, history effect, or timed local history effect, not a program impact. It tells us how much crimes were coming down “anyway” during this period either citywide (history) or for comparison locations only (local history).

In short, in the localized analyses, we will test for spatial displacement or diffusion effects in the areas just beyond each intervention site. Further, we will test for history or local history as threats to potential validity looking at drug-crime activity in high-activity but not “treated” comparison sites which are demographically similar.

### Model Identification

For the citywide ARIMA analyses of the weekly counts of homicide, violent crime and drug crime white-noise residuals and the best-fit measures were generated

9. The correlation between drug crimes in a blockgroup and the number of Operation Safe Streets intervention sites was .506 ( $p < .01$ ). This suggests planners were relatively successful at targeting high-drug-crime areas for program sites.

10. Using *Crimestat*'s hierarchical nearest-neighbors hot-spot identifier, we set an alpha of .05 and a minimum of 10 drug crimes to categorize a “hot spot.” The center of the hot spot was identified and then buffered within 1/10 of a mile. Only those buffers which did not intersect the Operation Safe Streets buffers were included in the comparison-site analyses.

11. Although the comparison areas were identified using “hot spot” analyses, we also compared differences between the target areas and the comparison areas on demographic data. Using 2000 US Census data, we compared blockgroups in which target areas were nested with blockgroups in which comparison areas were nested. Comparing on demographics such as percent African American, percent female-headed households, percent single-parent households with children, household median income, household median value, percent owner-occupied households, percent of the population below the poverty line, and percent population 16 years and up unemployed, the target and comparison areas only differed significantly on percent owner-occupied households. Target blockgroups reported a higher percentage of owner-occupied households.

by 1,0,1 ( $p,d,q$ ) ARIMA models.<sup>12</sup> This indicates that an autoregressive parameter with a lag of one, and a moving average parameter with a lag of one, were "driving" the ongoing crime series. The single moving average parameter suggests that the errors, or shocks, surfacing between week <sup>$t$</sup>  and week <sup>$t-1$</sup>  were linked to the errors or random shocks surfacing between week <sup>$t-1$</sup>  and week <sup>$t-2$</sup> . The single autoregressive value suggests the trend from week <sup>$t$</sup>  to week <sup>$t-1$</sup>  links to the trend from week <sup>$t-2$</sup>  to week <sup>$t-1$</sup> .

Some comments on modeling strategy deserve mention. The (1,0,1) ARIMA model was appropriate for the citywide analyses because the two included ARIMA components ( $p,q$ ) were statistically significant (Cook & Campbell, 1979, p. 251) and because none of the residuals (PACF) were significant and thus represented white noise (McDowall, 1980, p. 51). For analyses done at the localized level, and with crime areal rates, it seemed likely, conceptually, that the same processes were at work. Further, one reviewer raised the concern that ARIMA can devolve to data mining, leading us to "stick" with the same (1,0,1) model for localized analyses. To gauge if we had over- or under-modeled with the (1,0,1) ARIMA models, for all localized models, we ran ARIMA models ranging from (1,0,0) through (2,2,2), inclusive, and compared fit measures (SSE, AIC, SBC). Fit was markedly better for the (1,0,1) models.

When working with drug-crime areal rates, however, either for the whole city or for some of the localized models, sometimes the (1,0,1) ARIMA model was no longer unequivocally the best. The (1,0,1) ARIMA model *did* generate white-noise residuals, thus satisfying McDowall's dictum: "The residuals of the tentative model *must* not be different than white noise (McDowall, 1980, p. 51, emphasis added)." "Straight" autoregressive models, (1,0,0), strongly favored by one reviewer, *failed* to generate white noise residuals. So, on the white noise criterion, the (1,0,1) ARIMA models appear better than a simpler autoregressive model. Unfortunately, the drug areal rate (1,0,1) ARIMA models did *not* always generate a statistically significant moving average (MA) parameter. A non-significant parameter "amounts to overmodeling and will generally lead to an invalid test of the intervention effect" (Cook & Campbell, 1979, p. 251). Therefore, on the requirement that all included ( $p,d,q$ ) ARIMA parameters be significant, the (1,0,1) ARIMA model is *not* the best. In short, for these outcomes, there is no one clear model that is arguably most satisfactory. Therefore, for the drug areal rate models, we show the results of both the (1,0,1) and the (1,0,0) models, and discuss when their results diverge significantly.

12. Since the outcomes in question are based on count data, concern was raised about whether the residual variance was constant across the time period, an assumption made by the ARIMA analyses. We performed a series of analyses in which the 139-week period was broken down into three (46-47 weeks), four (34-35 weeks), and finally five (27-28 weeks) time periods. An ANOVA for each of the breakdown arrangements tested the homogeneity of the variance. For both the three and four time period frameworks, there was no significant variation. Only for the five time periods, and only for drug crime, was there a significant difference in the homogeneity of the variance. We concluded from these analyses that heteroskedasticity was not a pervasive problem. We also tried a number of models estimating a 9/11 effect, with both a steady and a declining impact. The parameters were not significant.

## Descriptive Statistics

During the period examined, Philadelphia averaged 6.86 homicides (median = 7.0) per week, 407.22 violent crimes (median = 407.00) per week, and 234.53 drug crimes (median = 235) per week. Weekly homicide counts ranged from 0 to 13 incidents, violent crimes ranged from 307 to 525, and drug crimes ranged from 99 to 304. There were a total of 954 homicides, 56,603 violent crimes and 32,599 drug crimes. The distribution of all three crime types appeared quite normal when we examined histograms. Skewness statistics failed to exceed  $\pm 1$ .

## Results

### Citywide

Weekly Statistics for Crime Counts are provided in Table 1, and citywide models predicting weekly crime counts for homicide, all violent crimes, and drug crimes, appear in Table 2.

### Homicide

In the citywide homicide ARIMA (1,0,1) model, both ARIMA components were significant ( $p < .001$ ) and had values slightly below 1. The seasonal control used was a temperature difference from the prior week, and it demonstrated that increasing temperatures were linked to fewer homicides. Late winter into early spring of both 2000 and 2002, times of increasing temperatures were times of declining homicide counts. Fall of 2001, a time of declining temperature, also appeared to be a time of increasing homicides. The Safe Streets intervention dummy variable was associated with about one less homicide every 2 weeks.

**Table 1** Descriptive statistics for weekly crime counts in Philadelphia, January 3, 2000 through August 31, 2002

	Homicide	Violent crime	Drug crime
<i>M</i>	6.86	407.22	234.53
Median	7	407	235
Mode	7	390	230
<i>SD</i>	2.62	41.74	34.26
Skewness	0.22	0.24	-1.00
Minimum	0	307	99
Maximum	13	525	304
Sum	954	56,603.00	32,599.00

Note.  $N = 139$  weeks.

**Table 2** Citywide impacts of operation safe streets on weekly crime counts in Philadelphia

	Homicide		Violent		Drug (1,0,1)		Drug (1,0,0)	
	<i>b</i>	<i>SE</i>	<i>b</i>	<i>SE</i>	<i>b</i>	<i>SE</i>	<i>b</i>	<i>SE</i>
Autoregressive function	.97**	.05	.96**	.05	.50**	.17	.44**	.08
Moving average	.90**	.08	.80**	.09	.06	.19	–	–
Week temp diff <sup>a</sup>	–.01	.02	–	–	–	–	–	–
Temp of the first day <sup>b</sup>	–	–	1.09**	.26	–.16	.24	–.18	.24
Safe streets	–.41	1.02	–15.53	17.03	–9.98	14.20	–9.46	13.85
Constant	7.74**	1.11	345.08**	17.96	245.22**	14.01	246.08**	13.72

*Note.* The unit of analysis is the weekly count of crime. The time period covered is 1/1/00-8/31/02. \* $p < .05$ ; \*\* $p < .001$ .

<sup>a</sup>This is the difference in temperature between the first day of the current week and the first day of the previous week.

<sup>b</sup>This is the temperature of the first day of the week.

The impact, however, was far from significant. The intervention failed to have a significant citywide impact on homicides.

### *Violent crime*

The citywide ARIMA (1,0,1) for all violent crimes showed that the intervention was associated with about 16 fewer violent crimes each week. Although this impact was in the expected beneficial direction, it was not significant. The two ARIMA parameters were both significant with values slightly below 1.

The seasonal proxy variable used in this model, because it had the strongest impact, was maximum temperature on the first day of the week. Consistent with the literature on heat and violence (Anderson, 2001), we see that each additional degree was associated with about one additional violent crime per week. The January through June periods for both 2000 and 2001 were times of increasing violence. In sum, reported violent crime declined citywide following Operation Safe Street's implementation, but not significantly.

### *Drug crime*

AQ6 In the citywide ARIMA (1,0,1) model for drug crime, only the autoregressive parameter was significant. Operation Safe Streets reduced approximately 10 drug crimes a week, but the coefficient was not significant. Various temperature variables attempted failed to explain significant variation in weekly drug-crime counts; nonetheless, we included the maximum temperature of the first day of

the week as a seasonal proxy measure. The ARIMA (1,0,0) autoregressive model, with the nonsignificant moving average parameter removed, provided an almost equivalent intervention impact (−9.5 drug crimes per week).

These city-wide results were consistent. Although the Police Department claimed significant impacts of Operation Safe Streets across the city, the impacts we saw, although beneficial, were not statistically significant.

### Localized

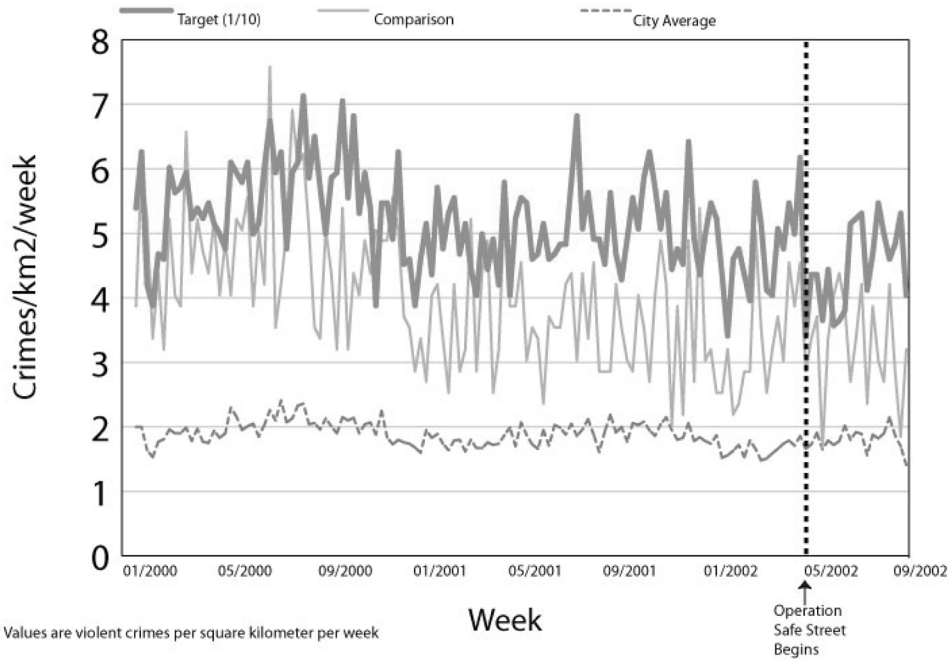
Geographically localized analyses consider the possibility that the intervention may have reduced crime, but only in the locations immediately adjoining the target site. In most of Philadelphia, street blocks are roughly a tenth of a mile long. Therefore, we defined each target's "impact area" as the zone within a tenth of a mile of the targeted site. Thus, a meaningful comparison can be made between changes in crime within the target zone and changes in the "ring" or buffer one tenth of a mile further out: 0.1-0.2 miles from the intervention point. If a spatial diffusion of benefits was taking place, crime should have decreased in both the inner target zone and outer adjoining areas. In contrast, if spatial displacement was taking place, crime should have decreased in the target zone but increased in the outer adjoining areas. In the tables, we label these inner and outer zones "target areas" and "adjoining areas", respectively. Because target and adjoining areas have different-sized geographic areas, crime counts were converted to crime rates per week per km<sup>2</sup>. We used these rates for all localized analyses.

We completed comparable analyses for a "matched" set of comparison areas. These were clusters of high drug activity—"hot spots"—not targeted for the Safe Streets intervention. If either the history or local history threat to internal validity was operating, we should see significant crime drops in *both* target and more distant comparison areas. Some other processes may have been driving down crime in the comparison areas (local history threat) or in both the target and comparison areas (history threat). Because of floor effect problems, analyses of homicide counts were not possible.<sup>13</sup> We report results for total reported violent crime, and drug crimes, using the same classes of drug crimes as were examined city-wide. The violent rates are shown in Figure 1 for the city, the target areas, and the comparison areas.

### *Violent crime*

Table 3 reports the effect of the intervention on areal violent crime rates in the targeted area (0-0.1 mile), the adjoining areas slightly further away (0.1-0.2 mile); the high crime, non-intervention comparison areas; and the county as a whole.

13. The average week in Philadelphia resulted in approximately seven homicides. We are unable to perform any meaningfully localized analyses with these few cases.



**Figure 1** Philadelphia weekly violent-crime rates: citywide, target areas ( $N = 213$ ), and comparison areas ( $N = 73$ ).

Operation Safe Streets had a significant negative impact on violent crime within 0.1 mile of the intervention site. Here, the intervention prevented on average one violent crime a week per  $\text{km}^2$  (See Table 3). Results suggest there was a modest diffusion of benefits. At 0.1-0.2 miles from the intervention site, the results reveal a smaller but still significant reduction of approximately 0.3 violent crimes a week per  $\text{km}^2$ . As in the earlier citywide, violent crime analyses,

**Table 3** Localized impacts of operation safe streets on violent crime rates

	Target areas		Adjoining areas		Comparison areas		Countywide	
	<i>b</i>	<i>SE</i>	<i>b</i>	<i>SE</i>	<i>b</i>	<i>SE</i>	<i>b</i>	<i>SE</i>
Autoregressive function	.97**	0.05	-.465	0.38	.99**	0.16	.96**	0.05
Moving average	.90**	0.08	-.614	0.33	.90**	0.05	.80**	0.09
Temp of the first day	.02**	0.01	.015**	0.01	.01*	0.01	.01**	0.01
Safe streets	-.99**	0.26	-.363*	0.15	-.64	0.41	-.07	0.08
Constant	4.10**	0.26	2.749**	0.17	3.28**	0.54	1.59**	0.08

*Note.* The dependent variable was crime counts/week ( $N = 139$ )/ $\text{km}^2$ . Countywide: crimes across the Philadelphia County. Target areas: crimes 0-0.1 miles from target sites ( $N = 214$ ). Adjoining areas: crimes 0.1-0.2 miles from target sites. Comparison areas: high-drug-crime comparison sites, where no intervention took place ( $N = 73$ ). ARIMA (1,0,1) model. \* $p < .05$ ; \*\* $p < .001$ .

both ARIMA coefficients were significant ( $p < .001$ ) with values slightly below one for the target area analyses. Also, consistent with earlier citywide analyses, a higher temperature at the beginning of the week was linked to higher violent-crime levels, and there was no significant countywide impact.

Table 3 also shows the results for the comparison areas; here, there was a co-occurring decrease of almost 0.6 violent crimes per week per km<sup>2</sup> during the program period. This drop, however, was not significant. The nonsignificant violent crime decline in the comparison areas strengthens our inference that declines, in target and adjoining locations, resulted from the program. For reference, the last set of figures in table 3 shows county-wide results, but now using crimes/week/km<sup>2</sup> rather than counts.

Figure 1 shows the trend for weekly violent crimes per km<sup>2</sup> for the immediate areas around the intervention sites, for the "matched" comparison areas, and for the whole city. The intervention and comparison series shadow each other closely through May of 2001, diverge prior to the intervention, and then co-vary closely during the initial intervention period. During the latter part of the treatment period, the comparison areas appear to trend somewhat lower than the target areas.

Figure 2 displays the same variable as Figure 1 but contrasts the target area (0.1 mile) with the adjoining (0.1-0.2 mile buffer) areas. Again, for reference, we show the citywide values. The target and adjoining series are, until the intervention starts, trending similarly, although the target areas are much higher. When

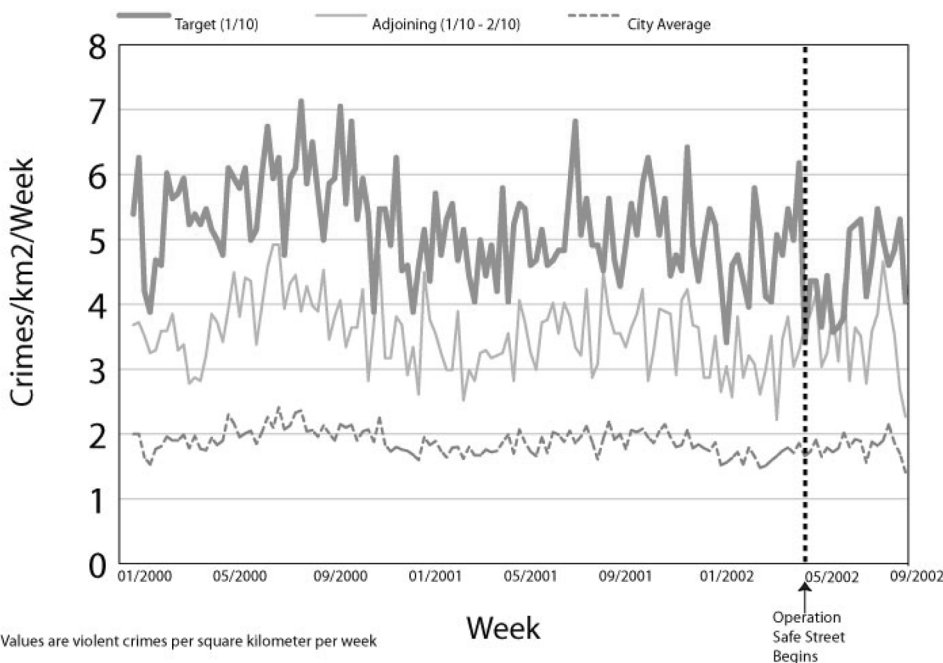


Figure 2 Philadelphia violent-crime rates: citywide, target areas ( $N = 213$ ), and adjoining areas.

AQ7 the program started, target area rates dropped dramatically, but the adjoining areas rates did not.

### *Drug crime*

Table 4 reports the results of the ARIMA models of drug crime rates for target areas (0-0.1 mile), adjoining areas (0.1 to 0.2 miles), the more distant comparison sites, and also the county.<sup>14</sup> Looking at the ARIMA (1,0,1) results, the program resulted in significantly less drug crime, about 3 per week per km<sup>2</sup>, in the target areas. Offsetting the drug crime declines immediately around the target sites are significant co-occurring increases slightly further away (0.1-0.2 miles). About 0.7 more drug crimes per week per km<sup>2</sup> were observed in these adjoining locations. About one quarter of the crime-reduction benefit appears to have been spatially displaced. The localized net reduction, therefore, is about 2.3 drug crimes per week per km<sup>2</sup>.

Because the MA parameter in the (1,0,1) ARIMA model was not significant, the parallel autoregressive (1,0,0) ARIMA model results deserve mention. The latter also showed a significant, comparably sized impact in the target area (-2.9). In the adjoining areas, however, it showed significant spatial diffusion of benefits rather than spatial displacement (-.9), resulting in a larger net reduction (-3.8).

AQ8 Table 4 also shows the for the ARIMA (1,0,1) results from high-drug-crime nonintervention sites, our matched comparison areas. Although the drug crimes within 0.1 mile of the Operation Safe Streets decreased (-2.961/week/km<sup>2</sup>), drug crimes in the nonintervention sites increased (0.542/week/km<sup>2</sup>), albeit not as much as in the adjoining areas (0.711/week/km<sup>2</sup>). This increase in non-intervention comparison areas may suggest a more distant spatial displacement effect, or it may be an unrelated, co-occurring increase. Co-occurring drops arising from a history or local history threat to internal validity cannot, therefore, "explain away" our results here.

The three intervention effects for drug crimes suggest: a significant program-related decrease in target areas; some nearby and significant spatial displacement according to the (1,0,1) ARIMA model, but spatial diffusion of benefit according to the (1,0,0) ARIMA model; and either some more modest distant spatial displacement or a "naturally occurring" increase in the comparison areas.

As noted earlier, the citywide drug crime analysis reveals a drop of about 10 drug crimes per week. When these data are converted to areal drug-crime rates, analyses show about 0.05 to 0.04 fewer drug crimes per week per km<sup>2</sup>, depending on the specific ARIMA model.

In sum, Operation Safe Streets has worked to reduce drug crimes at the targeted sites. Depending on which ARIMA model one chooses, (1,0,1) or (1,0,0), it also may have spatially displaced some drug crime, or created a spatial diffusion of benefits. Thinking about the former, it seems plausible that some drug-market

14. As with the violent crime, the crimes are reported per km<sup>2</sup> per week.

Table 4 Localized impacts of operation safe streets on drug-crime rates

	Target areas			Adjoining areas			Comparison areas			Countywide						
	(1,0,1)			(1,0,1)			(1,0,1)			(1,0,1)						
	b	SE		b	SE		b	SE		b	SE					
Autoregressive function	.54*	.16	.43**	.08	.37	.40	.44**	.08	.99**	.01	.40**	.08	.50*	.17	.44**	.08
Moving average	.13	.19	-	-	.18	.43	-	-	.87**	.05	-	-	.06	.19	-	-
Temp of the first day	-.01	.01	-.02	.01	-.01	.01	-.01	.01	-.01	.01	-.01	.01	-.01	.01	-.01	.01
Safe streets	-2.96**	.59	-2.93**	.56	.71**	.16	-.89*	.41	.54	.46	-.30	.42	-.05	.07	-.04	.06
Constant	8.99**	.58	9.07**	.56	2.24**	.17	7.21**	.41	4.46**	.73	4.17**	.42	1.13**	.06	1.13**	.06

Note. The dependent variable was crime counts/week ( $N = 139$ )/ $\text{km}^2$ . Countywide: crimes across Philadelphia County. Target areas: crimes 0-0.1 miles from target sites ( $N = 214$ ). Adjoining areas: crimes 0.1-0.2 mile from target sites. Comparison areas: high-drug-crime comparison sites, where no intervention took place ( $N = 73$ ).

\* $p < .05$ ; \*\* $p < .001$ .

entrepreneurs who were sensitive to the post-intervention enforcement landscape may have moved their operations slightly in response to the initiative.

The more distant post-intervention increases are more difficult to interpret, but they are non-significant. At least they assure us that demographically similar high-drug-activity areas were not experiencing a “natural” drop in drug crimes when Safe Streets was implemented.

Figure 3 shows the trend for weekly drug crimes per km<sup>2</sup> in the target areas as well as the nonintervention comparison areas. Unlike for violent crime, the trends over time are dissimilar both prior to the intervention and following. For about a year prior to the intervention target area, drug-crime rates were trending up, and comparison areas were trending down. Recall that target areas were initially identified because they were such high drug crime areas. It appears, in general, that the most appropriate places were targeted. It should come as no surprise that the program has such a marked impact on drug crimes, since the key catalyst for the intervention was drug crimes.

Figure 4 compares the target area and adjoining-area drug-crime rates. The two trends closely co-vary prior to the program implementation, and generally the target areas are high than adjoining areas. Although we do see a drop in the adjoining areas following program implementation, it is not as drastic a drop as we see in the target areas, in part because the adjoining areas were consistently lower before the intervention started. During the intervention phase, however, the rates of target and adjoining areas are reversed, with adjoining

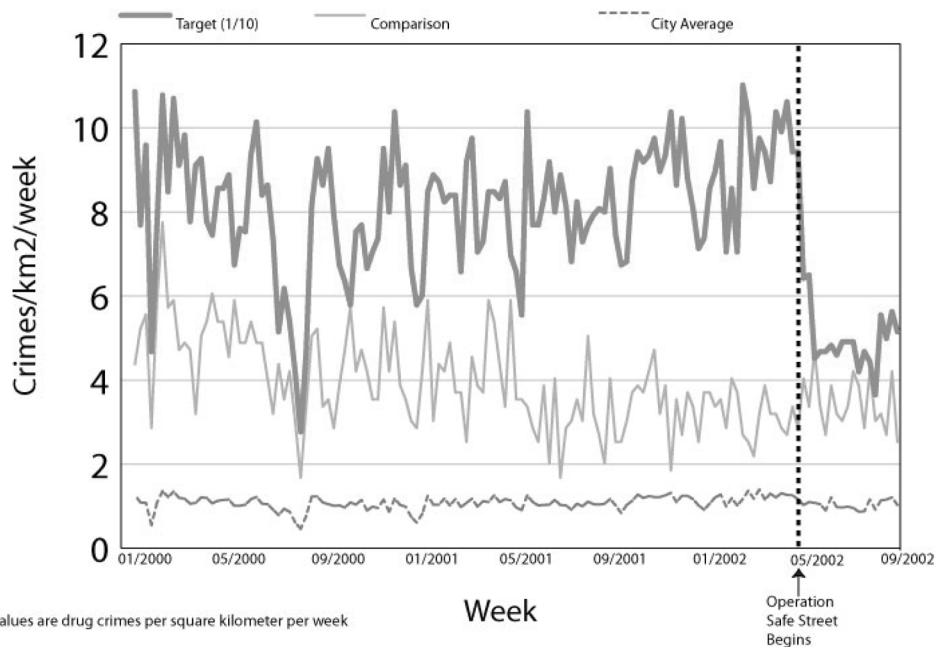
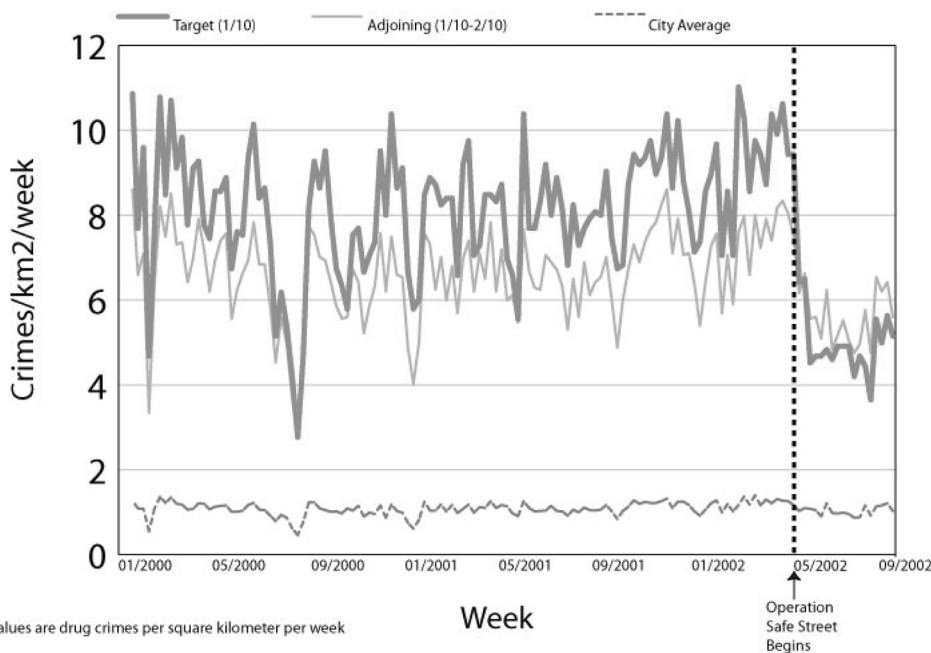


Figure 3 Philadelphia weekly drug-crime rates: citywide, target areas ( $N = 213$ ), and comparison areas ( $N = 73$ ).



**Figure 4** Philadelphia drug-crime rates: citywide, target areas ( $N = 213$ ), and adjoining areas.

areas now consistently higher. Although this visual reversal is not, strictly speaking, relevant statistically, it increases our willingness to lean toward the (1,0,1) ARIMA model of localized drug crime rates and the spatial displacement effect suggested by that model.

### Discussion

The present study has limitations deserving mention. First, posttest observations extend only over 4 months. Efforts to obtain data permitting a longer follow-up proved unsuccessful. Thus, it is unclear how or in what ways the impacts of Operation Safe Streets may have weakened or strengthened after our posttest period. Additionally, detailed information regarding officers assigned to the intervention sites was not available. We do know that officers were stationed at the drug sites, on post, throughout the intervention period used here. It seems plausible that some officers may have used their time on post more effectively than others: observed more, or conversed more with locals to gain a better picture of problems in the area. We have no way of obtaining such information and thus cannot address questions of varying program impacts across sites. Additionally, our study, despite the rigorous analytic tools brought to the data, is in essence a case study of one crackdown carried out by one police department at one point in time. And finally, for the drug-crime outcomes, there is no unequivocally “best” model.

Several strengths of the study may offset some of these limitations. First, data are available for a lengthy period, with the number of observations well above that required for identifying the best-fitting model, and thus permitting over-fitting. The lengthy series also covers the entire city, allowing us to gauge citywide effects, as claimed by program proponents, as well as localized effects. Further, for localized models, tests for possible displacement or diffusion effects were employed, allowing us to gauge program impacts in areas slightly further away from the target sites. These tests examined both violent crimes and drug crimes. Additionally, to gauge history or local history as possible threats to internal validity, this study has examined the results for high drug crime, non-intervention sites.

The program took place amidst ongoing natural variation in crime levels. Figure 3 shows that drug activity was at one of its highest rates prior to the implementation of the Operation Safe Streets intervention. That peak spurred the creation of the intervention (Glass, 1968). Some of the decrease observed at the program's launch could be attributable to a regression towards the mean. The ARIMA analyses, however, control for that particular threat to internal validity.

The most important results are fourfold. First, Operation Safe Streets failed to have significant citywide impact on homicides, violent crime, or drug crimes. Program planners began publicly proclaiming murder-reduction benefits of the program a few weeks after it started. Those claims were not supported by these analyses.

Nonetheless, the second result is that we did see significant localized crime reductions. Localized analyses show significant impacts of the program on violent crimes as well as drug crimes. Areas within one tenth of a mile of the target site experienced significantly lower weekly areal crime rates. The program was creating "bubbles" of relative safety near officer locations. The intervention was working in a spatially delimited fashion, and benefiting residents in the immediate vicinity. The program was a success but, spatially, a small-scale success.

Partially counterbalancing these small-scale successes, however, was our third result suggesting a partial displacement of drug-crime activity, at least according to the ARIMA (1,0,1) model. What is encouraging, however, is that it was not total displacement. Approximately 0.7 of the 3 "prevented" weekly drug crimes per km<sup>2</sup> (see Table 4), or about 23 percent, were "re-appearing" slightly further from the intervention sites.

Our finding of a drug-crime spatial-displacement effect contrasts with Green's (1995) finding of a spatial diffusion of benefits around targeted drug-intervention sites in Oakland. Bear in mind, however, that her data were individual-level, and her intervention was multi-agency. Nonetheless, we do agree with her that our aggregated crime data for the adjoining areas probably reflect a mixture of some diffusion of benefit and some displacement, with the latter prevailing here with drug crimes. It is worth remembering however, that if drug offenders are committing crimes at varying rates (crime-specific lambdas), that

alone might cause a divergence between aggregate vs. individual offender-based data gauging displacement vs. diffusion of benefits. Because we do not have access to individual-level crime data (cf. Green, 1995) we cannot specify further details about the spatial displacement. We do not know, for example, if the displaced crimes taking place in the areas adjoining the program target areas were committed by those who had moved outward and away from the stationed officers, or by different offenders moving in to the adjoining areas.

To reiterate, however, the reader should bear in mind that if the autoregressive model's results are followed, the result is a spatial diffusion of benefits for this outcome, rather than displacement. If we follow the autoregressive (1,0,0) ARIMA model's results, our localized drug-crime rates would parallel Green's (1995) results, and both studies would suggest spatial diffusion of drug-crime prevention efforts.

Thinking specifically about drug-crime changes within the target areas, contrasting results from the sociodemographically and increasingly drug-active comparison areas showed that *generally* falling drug crime rates can *not* "explain away" the intervention effect seen in these target areas. In the comparison areas, drug-crime areal rates went up slightly but nonsignificantly during the posttest period. These analyses would seem to eliminate plausible variants of both a history threat to internal validity—drug crime just happened to start falling citywide as the program was implemented—and a local history threat to internal validity—drug crime just happened to start falling in the high-drug-crime locations just as the program was being implemented.

Unfortunately, again because we do not have more detailed individual-level crime data, we cannot interpret the slight rise in areal drug crime rates more definitively in the comparison areas. That elevation might reflect either more spatially distant spatial displacement effects of drug crime, on the order of about 18 percent of the immediate localized prevention effect, or a "naturally" co-occurring rise in drug-crime activity in the comparison areas.

Additionally, and this is our fourth but perhaps most intriguing point theoretically, displacement vs. diffusion of benefit effects appear perhaps to be crime-specific. Although the localized analyses showed spatially limited program benefits in target areas for both violent-crime rates and drug-crime rates, they suggest spatial diffusion of benefits predominates for the former and spatial-displacement predominates—if you follow the ARIMA (1,0,1) results—for the latter. In the target and adjoining areas combined, the average overall benefit was  $(-0.99 + -0.36)$  1.35 fewer violent crimes per week per km<sup>2</sup>. The comparison areas experienced declining violent crime at the same time, but the drop there was not statistically significant, thereby eliminating both the history—city-wide violent crime started going down just when the program was implemented—and local history—violent crime started going down in high-drug-activity locations just as the program was implemented—threats to internal validity.

Of course, many other types of displacement besides spatial displacement may have taken place once the program was implemented (Barr & Pease, 1990; Hakim & Rengert, 1981; Repetto, 1976). Some but not all of these other types of

displacement might be ruled out. Assuming drug sellers did not change their mode of operation from the pretest period to the posttest period, we can probably rule out a citywide displacement of type of criminal activity, driven by those who were drug sellers during the pretest period experiencing drug-crime-specific deterrence or discouragement (cf. Green, 1995, p. 752), due to extensive publicity surrounding the Safe Streets initiative. Recall that drug-crime rates went up slightly in the comparison areas. But we probably cannot rule out other types of displacement. We think it likely that the mode of drug selling shifted during the posttest period to more interior activity. Additionally, some initial analyses suggest that drug sellers may have shifted the time of their drug selling, moving it to times when officers would have a harder time spotting drug activity due to darkness (Luongo, Lawton, & Taylor, 2004), but again, our speculations are limited due to lack of data.

The pattern of displacement for drug crimes shown by the ARIMA (1,0,1) model does, however, fit with the theoretical literature on drug selling mentioned earlier. Since drug offenders carefully position their open-air markets close to their customers (Eck, 1995; Rengert, 1996), this makes sense. By moving only slightly away, they can leave behind touts to direct their original customers a couple of blocks down or around the corner, thereby avoiding the Safe Streets officers assigned to remain "on post" at this period of the intervention. By contrast, since violent crime is generally less planned than drug crimes, generally less likely to be location-specific, potential violent offenders probably "tune in" less closely to the immediate surroundings. So, for these potential violent offenders, simply seeing an officer on location would lead them to generally give that locale a wide berth. Putting aside specific fixed opportunity structures that might generate victims, like ATM machines and check cashing establishments, the violent offenders, in contrast to the drug sellers, are less "in need" of a particular location.

The nonintervention sites, although higher than the city average, are not nearly as high crime as the Operation Safe Streets intervention sites. The 214 sites chosen for the program intervention appear to have been well chosen. Figure 3 shows that Operation Safe Streets has lowered the drug crime rate in these areas to a level comparable to the comparison areas. This might suggest that the program is a solid first step in fighting criminal drug activity, but clearly it was not the complete solution that Mayor Street and police wished. Drug activity is still about four times the citywide rate in these targeted areas.

The Operation Safe Streets program has shown local beneficial drug-crime impacts, and localized violent-crime impacts. But at what cost? And how have these costs been justified?

In its initial form, the intervention was aimed at reducing drug-related crimes within high-drug neighborhoods. Mounting attention to its costs, however, forced officials to justify its effectiveness. The benefits of Operation Safe Streets were then generalized to include other types of more serious criminal activity.

Also in response to cost concerns, program tactics have shifted. In the version of the program implemented in 2003, a single officer patrols multiple high-crime

areas from within a motor vehicle. The high costs have led to tactical modifications of the original program by city officials and police leadership, suggesting that it was not feasible to continue in its initial form on such a large scale. Changing offender practices probably contributed to these shifts in tactics as well.

Crackdowns respond to current crises. Because they are "out of the ordinary", they cost a lot, they attract attention, and as we show here, they get results. But they are rarely sustainable because of high costs. The irony is that although they are dramatic gestures and, as we have suggested here, *effective* ones, they are not long-term solutions. The goal then is to engineer more cost-effective crackdowns, which can be sustained over time. The evidence needed to guide this engineering probably requires individual, offender-level data (e.g., Green 1995) combined with localized analyses of crime rates such as those shown here.

### Acknowledgements

The authors acknowledge their debt to the Philadelphia Daily News. This project would not have been possible without the support of Nicole Egan Weisensee. The authors received support from the Temple University Office of the Provost for the Philadelphia Area Survey during this time. Opinions stated herein reflect neither the official policies nor views of the Philadelphia Daily News nor Temple University. Portions of an earlier version were presented by the first author at the 2003 annual meeting of the American Society of Criminology. Peter Jones, Jerry Ratcliffe, George Rengert, Ron Davis, two anonymous referees, and the editor provided extraordinarily helpful suggestions on earlier drafts.

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