PROBLEM-ORIENTED POLICING, DETERRENCE, AND YOUTH VIOLENCE: AN EVALUATION OF BOSTON'S OPERATION CEASEFIRE

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Operation Ceasefire is a problem-oriented policing intervention aimed at reducing youth homicide and youth firearms violence in Boston. It represented an innovative partnership between researchers and practitioners to assess the city's youth homicide problem and implement an intervention designed to have a substantial near-term impact on the problem. Operation Ceasefire was based on the "pulling levers" deterrence strategy that focused criminal justice attention on a small number of chronically offending gang-involved youth responsible for much of Boston's youth homicide problem. Our impact evaluation suggests that the Ceasefire intervention was associated with significant reductions in youth homicide victimization, shots-fired calls for service, and gun assault incidents in Boston. A comparative analysis of youth homicide trends in Boston relative to youth homicide trends in other major U.S. and New England cities also supports a unique program effect associated with the Ceasefire intervention.

Although overall homicide rates in the United States declined between the 1980s and 1990s, youth homicide rates, particularly incidents involving firearms, increased dramatically. Between 1984 and 1994, juvenile (younger than 18) homicide victimizations committed with handguns increased by 418 percent, and juvenile homicide victimizations committed with other guns in-

This research was supported under award 94-IJ-CX-0056 from the National Institute of Justice, Office of Justice Programs, U.S. Department of Justice. Points of view in this article are those of the authors and do not necessarily represent the official position of the U.S. Department of Justice. The authors would like to thank Paul Evans, James Jordan, Gary French, and the members of the Boston Gun Project working group for their support in this research enterprise; Glenn Pierce and Baron Briggs for the acquisition of some of the data; and Stephen Brimley, Christiana Briggs, and Lisa Sanbonmatsu for their excellent research assistance. We would also like to thank Lois Mock for her sound advice and patience in the completion of this research.

JOURNAL OF RESEARCH IN CRIME AND DELINQUENCY, Vol. 38 No. 3, August 2001 195-225

creased 125 percent (Fox 1996). During this time period, adolescents (ages 14 to 17) as a group had the largest proportional increase in homicide commission and victimization, but young adults (ages 18 to 24) had the largest absolute increase in numbers, and there was a good deal crossfire between the two age groups (Cook and Laub 1998). All of the increase in youth homicide was in gun homicides (Cook and Laub 1998). For many cities, the bulk of this dramatic increase in youth homicide occurred in the late 1980s and early 1990s. In Boston, youth homicide (ages 24 and younger) increased more than threefold—from 22 victims in 1987 to 73 victims in 1990 (see Figure 1). Youth homicide remained high even after the peak of the epidemic; Boston averaged about 44 youth homicides per year between 1991 and 1995.

At the same time that the United States was experiencing this sudden increase in youth violence, the capacity of police departments to design and implement creative new operational strategies also increased through the advent of "community" and "problem-oriented" policing (Goldstein 1990; Sparrow, Moore, and Kennedy 1990). In Boston, an interagency problem-solving intervention, based in part on a tight link between research, the design of interventions, and operations, has shown much promise in reducing youth homicide (Kennedy, Braga, and Piehl 1997; Kennedy, Piehl, and Braga 1996). Nationally, without the support of a formal evaluation, the Boston program has been hailed as an unprecedented success (see, e.g., Butterfield 1996; Witkin 1997). This article describes the results of a National Institute of Justice-funded evaluation of Boston's youth homicide reduction initiative. Our analyses of Boston's youth homicide prevention program suggests that it was a very effective intervention; not only was the intervention associated with a significant reduction in youth homicide victimization, it also was associated with significant reductions in shots-fired calls for service and gun assault incidents.

THE BOSTON GUN PROJECT AND THE OPERATION CEASEFIRE INTERVENTION

Problem-oriented policing holds great promise for creating a strong local response to youth homicide problems. Problem-oriented policing works to identify why things are going wrong and to frame responses using a wide variety of often untraditional approaches (Goldstein 1979). Using a basic iterative approach of problem identification, analysis, response, evaluation, and adjustment of the response, problem-oriented policing has been effective against a wide variety of crime, fear, and order concerns (Braga, Weisburd et al. 1999; Eck and Spelman 1987; Goldstein 1990). This adaptable and

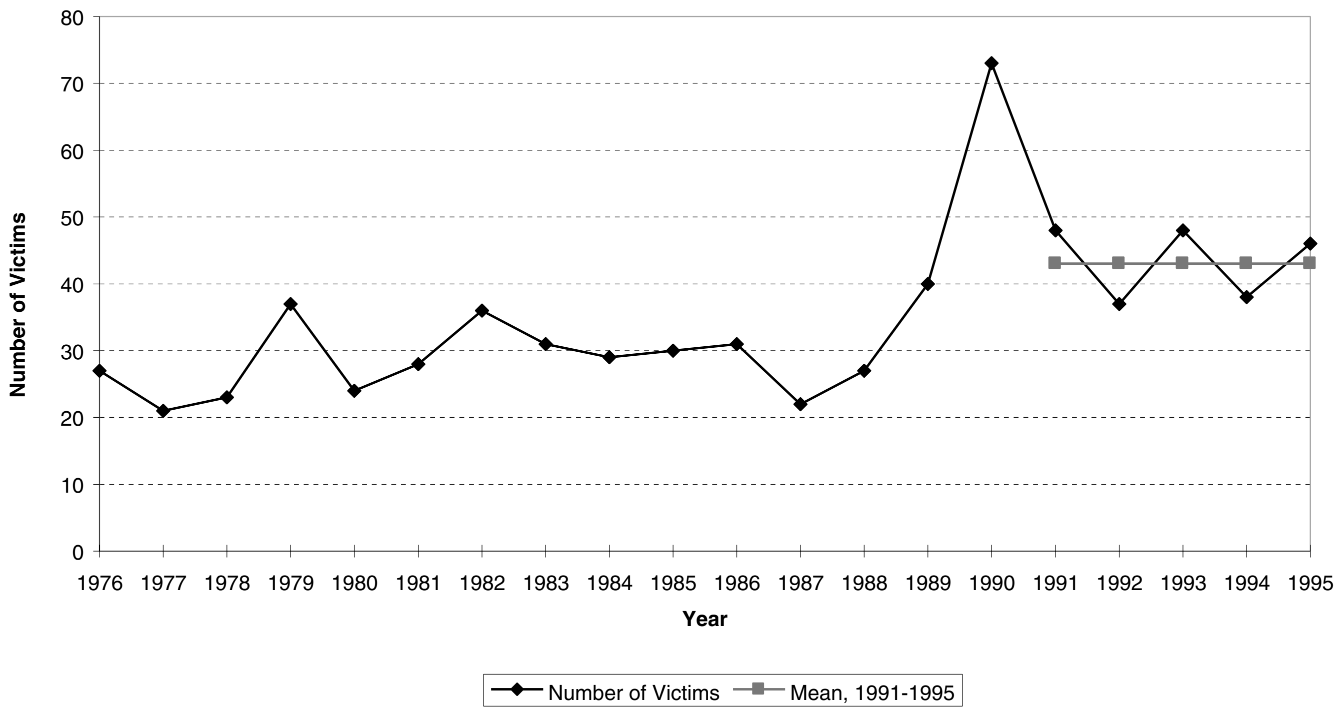

Figure 1: Boston Homicide Victims Ages 24 and Younger

dynamic analytic approach provides an appropriate framework to uncover the complex mechanisms at play in youth homicide and develop tailor-made interventions to reduce youth homicide victimization.

The Boston Gun Project is a problem-oriented policing initiative aimed at reducing homicide victimization among young people in Boston. *Youth* was initially defined as "age 21 and under" and, as the project developed, "age 24 and under."[1] Sponsored by the National Institute of Justice, the project was designed to proceed by (1) assembling an interagency working group of largely line-level criminal justice and other practitioners; (2) applying quantitative and qualitative research techniques to create an assessment of the nature of, and dynamics driving, youth violence in Boston; (3) developing an intervention designed to have a substantial, near-term impact on youth homicide; (4) implementing and adapting the intervention; and (5) evaluating the intervention's impact. The project began in early 1995 and implemented what is now known as the Operation Ceasefire intervention beginning in the late spring of 1996.

Core participating agencies, as defined by regular participation in the Boston Gun Project Working Group over the duration of the project, included the Boston Police Department; the Massachusetts departments of probation and parole; the office of the Suffolk County District Attorney; the office of the U.S. Attorney; the Bureau of Alcohol, Tobacco, and Firearms; the Massachusetts Department of Youth Services (juvenile corrections); Boston School Police; and gang outreach and prevention "streetworkers" attached to the Boston Community Centers program. Other important participants, either as regular partners later in the process or episodically, have included the Ten Point Coalition of activist Black clergy, the Drug Enforcement Administration, the Massachusetts State Police, and the office of the Massachusetts Attorney General.

Project research showed that firearms associated with youth, especially with gang youth, tended to be semiautomatic pistols, often ones that were quite new and apparently recently diverted from retail (Kennedy et al. 1996; Kennedy et al. 1997). Many of these guns were first sold at retail in Massachusetts, and others were smuggled in from out of state. Project research also showed that the problem of youth homicide was concentrated among a small number of chronically offending gang-involved youth.[2] Only about 1,300 gang members—less that 1 percent of their age group citywide—in about 61 gangs were responsible for at least 60 percent of all youth homicides in the city. These gangs were well known to the authorities and streetworkers; gang members were also often well known and tended to have extensive criminal records (Kennedy et al. 1996). Chronic disputes, or "beefs," among gangs appeared to be the most significant driver of gang violence (Braga, Piehl, and Kennedy 1999).

The research findings were discussed and analyzed within the working-group problem-solving process and were instrumental in the development of an operational strategy. The research findings and the working-group process thus led to the Operation Ceasefire intervention (for a complete discussion of the program development and implementation process, see Kennedy, Braga, and Piehl 1999). Operation Ceasefire included two main elements: (1) a direct law-enforcement attack on illicit firearms traffickers supplying youth with guns and (2) an attempt to generate a strong deterrent to gang violence. The working group framed a set of activities intended to systematically address the patterns of firearms trafficking identified by the research. These included the following:

- Expanding the focus of local, state, and federal authorities to include *intrastate* trafficking in Massachusetts-sourced guns, in addition to interstate trafficking.
- Focusing enforcement attention on traffickers of those makes and calibers of guns most used by gang members.
- Focusing enforcement attention on traffickers of those guns showing short time to crime and thus most likely to have been trafficked. The Boston Field Division of ATF set up an in-house tracking system that flagged guns whose traces showed an 18-month or shorter time to crime.
- Focusing enforcement attention on traffickers of guns used by the city's most violent gangs.
- Attempting restoration of obliterated serial numbers and subsequent trafficking investigations based on those restorations.
- Supporting these enforcement priorities through analysis of crime gun traces generated by the Boston Police Department's comprehensive tracing of crime guns and by developing leads through systematic debriefing of, especially, arrestees involved with gangs and/or involved in violent crime.

The "pulling levers" strategy, as the second element came to be known by working-group members, involved deterring violent behavior by chronic gang offenders by reaching out directly to gangs, saying explicitly that violence would no longer be tolerated, and backing that message by "pulling every lever" legally available when violence occurred (Kennedy 1997, 1998). Simultaneously, streetworkers, probation and parole officers, and later churches and other community groups offered gang members services and other kinds of help. The Ceasefire working group delivered this message in formal meetings with gang members, through individual police and probation contacts with gang members, through meetings with inmates of secure juvenile facilities in the city, and through gang outreach workers. The deterrence message was not a deal with gang members to stop violence. Rather, it was a promise to gang members that violent behavior would evoke an immediate and intense response. If gangs committed other crimes but refrained from violence, the normal workings of police, prosecutors, and the rest of the criminal justice system dealt with these matters. But if gang members hurt people, the working group focused its enforcement actions on them.

When gang violence occurred, the Ceasefire agencies addressed the violent group or groups involved, drawing from a menu of all possible legal levers. The chronic involvement of gang members in a wide variety of offenses made them, and the gangs they formed, vulnerable to a coordinated criminal justice response. The authorities could disrupt street drug activity, focus police attention on low-level street crimes such as trespassing and public drinking, serve outstanding warrants, cultivate confidential informants for medium- and long-term investigations of gang activities, deliver strict probation and parole enforcement, seize drug proceeds and other assets, ensure stiffer plea bargains and sterner prosecutorial attention, request stronger bail terms (and enforce them), and focus potentially severe federal investigative and prosecutorial attention on, for example, gang-related drug activity. The multitude of agencies involved in the working group assessed each gang that behaved violently and subjected them to such crackdowns. These operations were customized to the particular individuals and characteristics of the gang in question and could range from probation curfew checks to DEA investigations.[3]

The Ceasefire crackdowns were not designed to eliminate gangs or stop every aspect of gang activity but to control and deter serious violence. To do this, the working group explained its actions against targeted gangs to other gangs, as in "this gang did violence, we responded with the following actions, and here is how to prevent anything similar from happening to you." The ongoing working-group process regularly watched the city for outbreaks of gang violence and framed any necessary responses in accord with the Ceasefire strategy. As the strategy unfolded, the working group continued communication with gangs and gang members to convey its determination to stop violence, explain its actions to the target population, and maximize both voluntary compliance and the strategy's deterrent power.

A central hypothesis within the working group was the idea that a meaningful period of substantially reduced youth violence might serve as a "firebreak" and result in a relatively long-lasting reduction in future youth violence (Kennedy et al. 1996). The idea was that youth violence in Boston had become a self-sustaining cycle among a relatively small number of youth, with objectively high levels of risk leading to nominally self-protective behavior such as gun acquisition and use, gang formation, tough street behavior, and the like: behavior that then became an additional input into the cycle of violence (Kennedy et al. 1996). If this cycle could be interrupted, a new equilibrium at a lower level of risk and violence might be established, perhaps without the need for continued high levels of either deterrent or facilitative intervention.

DETERRENCE AND CRIME PREVENTION

The Operation Ceasefire intervention is, in its broadest sense, a deterrence strategy. Deterrence theory posits that crimes can be prevented when the costs of committing the crime are perceived by the offender to outweigh the benefits of committing the crime (Gibbs 1975; Zimring and Hawkins 1973). Most discussions of the deterrence mechanism distinguish between "general" and "special" deterrence (Cook 1980). General deterrence is the idea that the general population is dissuaded from committing crime when it sees that punishment necessarily follows the commission of a crime. Special deterrence involves punishment administered to criminals with the intent to discourage them from committing crimes in the future. Much of the literature evaluating deterrence focuses on the effect of changing certainty, swiftness, and severity of punishment associated with certain acts on the prevalence of those crimes (Blumstein, Cohen, and Nagin 1978; Cameron 1988; Cook 1977, 1980; Paternoster 1987; Sherman 1990; Sherman and Berk 1984; Weisburd, Waring, and Chayet 1995). In addition to any increases in certainty, severity, and swiftness of sanctions associated with youth violence, the Operation Ceasefire strategy sought to gain deterrence through the advertising of the law enforcement strategy and the personalized nature of its application. It was crucial that gang youth understood the new regime that the city was imposing.

The pulling-levers approach attempted to prevent gang violence by making gang members believe that consequences would follow on violence and gun use and choose to change their behavior. A key element of the strategy was the delivery of a direct and explicit "retail deterrence" message to a relatively small target audience regarding what kind of behavior would provoke a special response and what that response would be. Law enforcement agencies in Boston increased the cost of gang-related violence. The deterrence principles applied in the Operation Ceasefire intervention could be regarded as a "meso-deterrence" strategy. Beyond the particular gangs subjected to the intervention, the deterrence message was applied to a relatively small audience (all gang-involved youth in Boston) rather than a general audience (all youth in Boston) and operated by making explicit cause-and-effect connections between the behavior of the target population and the behavior of the authorities. Knowledge of what happened to others in the target population was intended to prevent further acts of violence by gangs in Boston.

The effective operation of general deterrence is dependent on the communication of punishment threats to the public. As Zimring and Hawkins (1973) observe, "the deterrence threat may best be viewed as a form of advertising" (p. 142). One noteworthy example of this principle is an evaluation of Massachusetts' 1975 Bartley-Fox amendment, which introduced a mandatory minimum one-year prison sentence for the illegal carrying of firearms. The high degree of publicity attendant on the amendment's passage, some of which was inaccurate, was found to increase citizen compliance with existing legal stipulations surrounding firearm acquisition and possession, some of which were not in fact addressed by the amendment (see Beha 1977). Zimring and Hawkins further observe that "if the first task of the threatening agency is the communication of information, its second task is persuasion" (p. 149). In his article on the misapplication of deterrence principles in gang suppression programs, Malcolm Klein (1993) suggests that law enforcement agencies do not generally have the capacity to "eliminate" all gangs in a gang-troubled jurisdiction, nor do they have the capacity to respond in a powerful way to all gang-offending in such jurisdictions. Pledges to do so, though common, are simply not credible. The Operation Ceasefire working group recognized that, for the strategy to be successful, it was crucial to deliver a credible deterrence message to Boston gangs. Therefore, the Ceasefire intervention targeted those gangs that were engaged in violent behavior rather than expending resources on those who were not.

IMPACT EVALUATION

Like most evaluations of crime prevention programs (Ekblom and Pease 1995), our evaluation design departs from the desirable randomized controlled experimental approach. The Operation Ceasefire strategy was aimed at all areas of the city with a serious youth violence problem. There were no control areas (or control gangs) set aside within the city because of the following: (1) The aim was to do something about serious youth violence wherever it presented itself in the city, (2) the target of the intervention was defined as the self-sustaining cycle of violence in which all gangs were caught up and to which all gangs contributed, and (3) the communications strategy was explicitly intended to affect the behavior of gangs and individuals not directly subjected to enforcement attention (Kennedy et al., 1996). Therefore, it was not possible to compare areas and groups affected by the strategy to similar areas and groups not affected. Our analysis of impacts within Boston associated with the Ceasefire intervention follows a basic one-group time-series design (Campbell and Stanley 1966; Cook and Campbell 1979); we also use a nonrandomized quasi-experiment to compare youth homicide trends in Boston to youth homicide trends in other large U.S. cities (Cook and Campbell 1979; Rossi and Freeman 1993).

Within-Boston Outcome Measures: Homicide and Gun Violence

The key outcome variable in our assessment of the impact of the Ceasefire intervention was the monthly number of homicide victims ages 24 and younger. The Ceasefire intervention mostly targets violence arising from gang dynamics; our earlier research suggests that most gang members in Boston are ages 24 and younger (Kennedy et al. 1996; Kennedy et al. 1997). Therefore, our impact evaluation focuses on the number of youthful homicide victims in this age group. The homicide data used in these analyses were provided by the Boston Police Department's Office of Research and Analysis. The youth homicide impact evaluation examined the monthly counts of youth homicides in Boston between January 1, 1991, and May 31, 1998; the preintervention period included the relatively stable but still historically high postepidemic years of 1991 to 1995 (see Figure 1).

Beyond preventing youth homicides, the Ceasefire intervention was also designed to reduce other forms of nonfatal serious violence. As such, our evaluation also examines monthly counts of citywide shots-fired citizen calls for service data and citywide official gun assault incident report data. These data are available for a slightly shorter time period than our homicide data set due to lags in the Boston Police Department's data collection and preparation procedures. These data are examined for the January 1, 1991, through December 31, 1997, time period. The computerized Boston Police Department incident data have what is, for our purposes, an important shortcoming—the records do not capture the age of the victim (this is, of course, also true for shots-fired calls for service). To assess the effects of the intervention on gun assaults in specific age groups, we collected information on the age of the victim from hard copies of gun assault incident reports for the study time period. Because the collection and coding of this information was a time-consuming task, we chose to collect these data for one high-activity police district. District B-2 covers most of Boston's Roxbury neighborhood and has a very dense concentration of gangs; 29 of 61 identified gangs (47.5 percent) had turf in B-2 (Kennedy et al. 1997). Furthermore, there were 217 homicide victims ages 24 and younger in Boston between 1991 and 1995; a third of these victims were killed in B-2 (71 of 217, 32.7 percent).

Simple Pre/Post Comparisons

In these analyses, we selected May 15, 1996, the date of the first direct communications with Boston gangs, as the date Ceasefire was implemented because all elements of the strategy—the focus on gun trafficking, a special interagency response to gang violence, and the communications campaign with gangs—were in place as of that date. No other rival programs were implemented in Boston even roughly close to this time period (Piehl, Kennedy, and Braga 2000). The well-known large reduction in yearly Boston youth homicide numbers certainly suggests that something noteworthy happened after Operation Ceasefire was implemented in mid-1996. As discussed earlier, Boston averaged 44 youth homicides per year between 1991 and 1995. In 1996, the number of Boston youth homicides decreased to 26 and then further decreased to 15 youth homicides in 1997. It is noteworthy that the yearly total of youth homicides in 1997—the first full calendar year of data after the implementation of Operation Ceasefire—represents the smallest number of youth homicides in Boston since 1976. This suggests that it was unlikely that the youth homicide reduction was due to a regression to the mean number of yearly youth homicides of the pre-youth homicide epidemic years. Figure 2 presents the monthly counts of youth homicides in Boston during the study time period. The time series shows a 63 percent reduction in the mean monthly number of youth homicide victims from a pretest mean of 3.5 youth homicides per month to a posttest mean of 1.3 youth homicides per month. This simple analysis suggests that Operation Ceasefire was associated with a large reduction in youth homicides in Boston (see also Piehl et al. 2000).

Generalized Linear Models

Generalized linear models were used in our deeper analysis of impacts associated with the Ceasefire intervention to analyze the time-series data (Dobson 1990; McCullagh and Nelder 1989). Generalized linear models are an extension of traditional linear models that allow "the mean of a population to depend on a *linear predictor* through a nonlinear *link function* and allows the response probability distribution to be any member of an exponential family of distributions" (SAS Institute 1993:4). This allows the technique to be applied to a wider range of problems. Generalized linear models are constructed by selecting the appropriate link function and response probability distribution. Because the underlying data were counts, a Poisson regression in a log-linear model was selected to model the monthly counts. The SAS Institute's GENMOD procedure was used to calculate the maximum likelihood estimate of the Ceasefire intervention effect parameters on the outcome

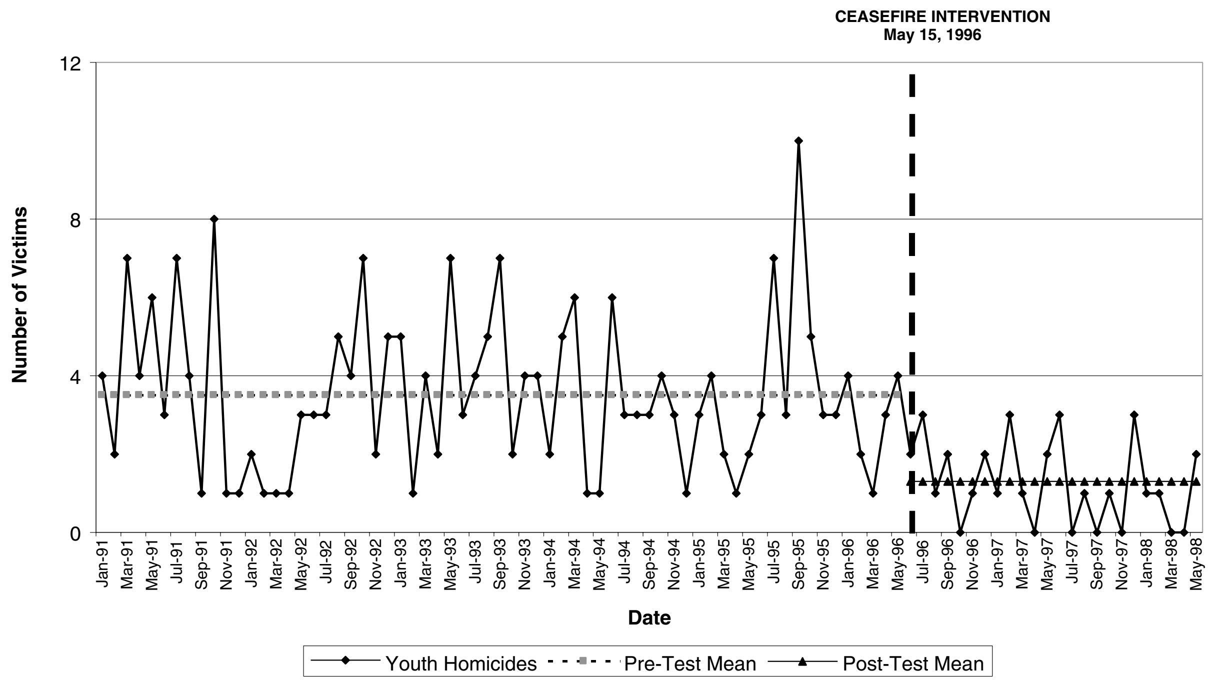

Figure 2: Monthly Counts of Youth Homicides in Boston

measures and to compute the associated probability values. The parameters for the intervention effects were also expressed as incidence rate ratios (i.e., exponentiated coefficients). Incidence rate ratios are interpreted as the rate at which things occur; for example, an incident rate ratio of .40 would indicate that, controlling for other independent variables, the Ceasefire intervention was associated with a 60 percent reduction in the number of youth homicides. Following social science convention, the two-tailed .05 level of significance was selected as the benchmark to reject the null hypothesis of "no difference." Likelihood ratio tests were used to determine whether adding the intervention variable provided statistically significant improvement of the model fit to the data (Aldrich and Nelson 1984). Finally, we present the results of the deviance statistic divided by the degrees of freedom; this measure examines the dispersion of the dependent variable and indicates whether these data are appropriate for the Poisson regression model (SAS Institute 1997:285).

As noted, we considered the Ceasefire intervention to be fully in place as of May 15, 1996. For convenience, we therefore begin the "post" period on June 1, 1996. Beyond our qualitative observations on the implementation of the program, we also examined the youth homicide time series for exogenous structural breaks; these analyses suggested that the maximal break in the series occurred in June 1996 (Piehl et al. 1999:13). The preintervention time series was composed of the monthly counts between January 1991 and May 1996; the intervention time series was composed of monthly counts between June 1996 and May 1998 for homicide measures and between June 1996 and December 1997 for nonfatal serious violence indicators.

In any time series, there are three sources of noise that could obscure intervention effects: *trend*, meaning the series could drift upward or downward; *seasonality*, meaning the series could spike at different times (e.g., homicide increases in summer months); and *random error*, meaning that even if the series was detrended and deseasonalized, observations would fluctuate randomly around some mean level (McDowall et al. 1980:14). If a time-series model does not account for these sources of error, the intervention analysis will be confounded. The general class of auto regressive integrated moving average (ARIMA) models can be used to good effect in detecting these three sources of noise in a time series (McDowall et al. 1980). We used ARIMA models to unravel the error structure of the preintervention time series for each outcome measure to guide us in accounting for these sources of noise in our generalized linear models.[4] The important findings of this exercise are discussed here, and the details are available on request from the *Journal of Research in Crime and Delinquency.* None of the outcome measures exhibited statistically significant serial autocorrelation. However, all outcome measure time series exhibited varying seasonal effects; that is, all time series had either seasonal moving averages (a shock that is felt once each season and then disappears), seasonal autocorrelation (e.g., August 1991 figures correlated with August 1992, August 1993, and so on), or both. To account for these seasonal effects in our models, we included dummy variables for each month. None of the time series data showed significant nonseasonal autocorrelation (i.e., monthly counts serially correlated); therefore, we did not estimate a nonseasonal autoregressive component in our models.

The preintervention time series varied in whether a trend was present. Youth homicides and youth gun assault incidents in B-2 were relatively stable during the preintervention time series, whereas citywide shots-fired calls and citywide gun assault incidents in B-2 exhibited simple linear downward trends. To account for trends in the series, we included a simple linear trend variable in the model.[5] Finally, months do not have an equal number of days. Therefore, the probability that a violent event could occur in a given month increases or decreases. For example, January has 3 more days (31 days) than February (28 days) to experience a youth homicide. To account for these differences in monthly interval lengths, we allowed the interval length in the GENMOD to vary according to the number of days per month.[6] Inserting youth homicides as the dependent variable, the basic model was as follows:

Monthly Youth Homicide Count = Intercept + Intervention
+ Trend + Month Dummy Variables + Error.

Table 1 presents the results of the Poisson regressions controlling for trend and seasonal effects. The Ceasefire intervention was associated with a statistically significant decrease in the monthly number of youth homicides; according to the incidence rate ratio, the Ceasefire intervention was associated with a 63 percent decrease in the monthly number of youth homicides. The Ceasefire intervention was also associated with statistically significant decreases in the monthly numbers of citywide gun assault incidents, citywide shots-fired calls for service, and youth gun assault incidents in district B-2. According to the incidence rate ratios, the Ceasefire intervention was also associated with a 25 percent decrease in the monthly number of citywide gun assault incidents, a 32 percent decrease in the monthly number of citywide shots-fired calls for service, and a 44 percent decrease in the monthly number of youth gun assaults in district B-2. The likelihood ratio test result was also significant, confirming that the intervention variables significantly improved the fit of the models to the data. The deviance divided by degrees of freedom results were only slightly higher than 1.0; this suggests that the Poisson distribution was appropriate for the youth homicide, city gun assault incidents, and B-2 youth gun assault incidents models (see SAS Institute 1997:285). The results for the shots-fired calls for service model, however, suggested that

TABLE 1: Results of the Poisson Regressions Controlling for Trend and Seasonal Effects

	Youth Homicides	*Gun Assaults*	*Shots Fired*	*B-2 Youth Gun Assaults*
Incidence rate ratio	0.37	0.75	0.68	0.56
Parameter estimate	–0.9948	–0.2886	–0.3854	–0.5814
Standard error	0.2501	0.0514	0.0271	0.1339
Chi-square	15.8217	31.5819	202.6158	18.8439
Probability > chi-square	0.0001*	0.0001*	0.0001*	0.0001*
Likelihood ratio test chi-square	16.6259	31.9418	206.8892	19.6072
Probability > likelihood ratio test chi-square	0.0001*	0.0001*	0.0001*	0.0001*
Trend	–0.0014	–0.0093*	–0.0119*	–0.0093*
January	–0.0213	–0.0108	–0.0008	–0.0442
February	2.8335*	2.8736*	2.8356*	3.1343*
March	–0.0185	0.0508	–0.0111	0.0382
April	0.3767	1.0479*	1.0473*	0.9969*
May	0.1890	0.1048	0.3272*	0.1842
June	1.1827*	1.1471*	1.5021*	1.1553*
July	0.3444	0.2728*	0.4407*	0.4252*
August	0.1410	0.3388*	0.4416*	0.1975
September	1.3472*	1.1825*	1.2423*	1.2634*
October	0.3486	0.1141	0.2507*	0.1807
November	0.6932*	0.9248*	1.0636*	0.7601*
Log likelihood	47.5647	19680.27	111620.60	1535.6891
Deviance/*df*	1.12	1.65	9.18	1.47

NOTE: December was the reference category for the month dummy variables.
*$p < .05$.

these data were overdispersed. The significant reduction in shots-fired calls for service associated with the Ceasefire intervention remained after the model was run with a correction for overdispersion.[7]

The youth homicide and gun violence reductions associated with the Ceasefire intervention could have been caused or meaningfully influenced by other causal factors (see Piehl et al. 2000). We therefore controlled for changes in Boston's employment rate as measured by the Massachusetts Department of Employment and Training, changes in Boston's youth population ages 5 to 24 as measured by the U.S. Bureau of the Census, changes in citywide trends in violence as measured by the robbery data reported in the Federal Bureau of Investigation Uniform Crime Reports, changes in homicide victimization among older victims (ages 25 and older), and changes in youth involvement in street-level drug market activity as measured by Boston Police Department arrest data. Admittedly, these controls are far from ideal. For example, measuring changes in Boston's citywide youth population does not directly measure population changes among our target audience—gang-involved youth offenders. However, these variables represent the best available information on these alternate endogenous explanations for Boston's decrease in youth homicide. When these control variables were added to our models, our findings did not substantively change. The significant reductions in youth homicide, shots-fired calls for service, gun assault incidents, and youth gun assault incidents in B-2 associated with Operation Ceasefire remained when the control variables were added to our Poisson regression models (see Table 2).

Youth Homicide Trends in Boston Relative to Youth Homicide Trends in Other Cities

Although the within-Boston analyses support that a large reduction in youth homicide and gun violence was associated with the Ceasefire intervention, it is necessary to distinguish youth homicide trends in Boston from national trends in youth homicide. Many major cities in the United States have enjoyed noteworthy reductions in homicide and nonfatal serious violence (see, e.g., Blumstein and Rosenfeld 1998); the reductions in other cities could be associated with a number of complex and tightly interwoven endogenous or exogenous factors such as positive changes in the national economy, shifts in the age distribution of offending populations, or the stabilization of urban drug markets. Moreover, many cities, most notably New York (Kelling and Bratton 1998), have implemented crime prevention interventions that have been credited with substantial reductions in violence. The following analyses provide insight on whether Boston's reduction in youth homicide was part of national youth homicide trends and whether the program impact associated with the Ceasefire intervention was distinct in magnitude from other youth homicide reductions occurring at the same time as the Ceasefire intervention. Furthermore, because other cities were also taking intervention action to reduce youth homicide, these analyses will suggest whether any program impact in Boston was larger than, or distinct from, any other deliberate interventions implemented during the same time period. A priori, we predicted that Boston would experience a significant reduction in monthly youth homicide counts associated with the timing of the Ceasefire intervention.

To compare youth homicide trends in Boston to national youth homicide trends, we analyzed youth homicide data for the largest cities in the United States. By rank ordering U.S. Census population data in 1990 and 1996, we selected 41 of the most populous cities in the US.[8] Boston was ranked 20th in population size among these cities in both 1990 and 1996 with an average population of about 565,000. We then obtained monthly counts of the number of homicide victims ages 24 and younger for the 41 comparison cities

TABLE 2: Results of the Poisson Regressions Controlling for Rival Causal Factors, Trend, and Seasonal Effects

	Youth Homicides	*Gun Assaults*	*Shots Fired*	*B-2 Youth Gun Assaults*
Incidence rate ratio	0.28	0.81	0.72	0.58
Parameter estimate	–1.2578	–0.2081	–0.3234	–0.5378
Standard error	0.3500	0.0684	0.0353	0.0018
Chi-square	12.92	9.25	84.00	9.75
Probability > chi-square	0.0003*	0.0024*	0.0001*	0.0018*
Likelihood ratio test chi-square	14.11	9.33	84.99	10.04
Probability > likelihood ratio test chi-square	0.0002*	0.0023*	0.0001*	0.0015*
Population age 5 to 17	0.0001	–0.0001	0.0001*	0.0001
Population age 18 to 24	–0.0001	–0.0001	–0.0001*	–0.0001*
Employment rate	14.1371	1.0363	9.1317*	9.6738
Robbery index crimes	–0.0002	–0.0001	–0.0001	–0.0002
Youth drug arrests	–0.0048*	0.0001	0.0005*	0.0015
Adult homicide	0.0163	0.0078	0.0027	0.0290
Trend	–0.0306	–0.0287*	–0.0543*	–0.0746*
January	0.1706	–0.1961	–0.3295*	–0.6819*
February	2.8743*	2.7125*	2.5749*	2.6565*
March	0.1093	–0.1007	–0.2715*	–0.4667
April	0.5568	0.9265*	0.8413*	0.6060*
May	0.3411	0.0050	0.1526*	–0.1287
June	1.1215*	1.0326*	1.2701*	0.7736*
July	0.2483	0.1820*	0.2035*	0.0954
August	0.1776	0.2666*	0.2418*	–0.0966
September	2.0265*	1.1420*	1.2052*	1.0413*
October	0.6295	0.0789	0.1756*	–0.0030
November	0.8130*	0.9127*	1.0206*	0.6868*
Log likelihood	52.7188	19684.46	111677.47	1542.29
Deviance/*df*	1.07	1.67	8.27	1.40

NOTE: December was the reference category for the month dummy variables.
*$p < .05$.

from Supplementary Homicide Report (SHR) data for the time period of January 1991 through December 1997. After a close examination of these data, 2 cities (Washington, D.C. and New Orleans) were excluded due to extensive missing data. This left us with 39 major U.S. cities in the comparison group.

Recognizing that youth homicide trends can vary greatly across 39 major U.S. cities, we built a model that would maximize our ability to control for the various sources of error in the time series of each city. After a number of analyses,[9] we decided on the following model:

Monthly Count of Youth Homicide = Intercept + Trend + Trend Squared + Month Dummy Variables + Intervention + Autoregressive (1) Component + Error,

where trend controls for simple linear trends within each time series, trend squared controls for nonlinear trends within each time series, month dummy variables control for monthly seasonal effects within each time series, intervention estimates the effect of the intervention within each time series, and autoregressive (1) component estimates an overall AR(1) serial lag-one correlation components for each time series.

The SAS GENMOD procedure does not allow the estimation of an autoregressive component in generalized linear models. However, the SAS GLIMMIX macro allows autoregressive components to be estimated in generalized linear mixed models (see Littell et al. 1996). Mixed models are generally used by statisticians to estimate random effects in statistical models. However, they can also be used to estimate a variance component that is different from that assumed by generalized linear models. In our fixed-effects model, the GLIMMIX macro simply allows us to estimate a variance component that includes an AR(1) coefficient in a generalized linear Poisson regression model. GLIMMIX also automatically corrects for overdispersion in the distribution of the dependent variable by estimating an overdispersion coefficient (see Littell et al. 1996). Finally, we also accounted for the varying number of days per month.

Table 3 presents the results of the Poisson regressions for the 39 comparison cities plus Boston.[10] Four cities—Boston, Jacksonville, Dallas, and Virginia Beach—had differences in youth homicides at the time of the intervention that were statistically significant at the .05 level; Boston had the largest estimated effect. Because our cross-city analysis involved 40 statistical tests, the expected number of effects significant at the .05 level is two. Thus, we need to be especially sensitive to the possibility of Type II error in our results. We would expect that 14 percent of the time we would find four or more statistically significant effects by chance alone. However, these cities would be a randomly selected set; we made an a priori prediction that Boston would have a significant reduction. The probability of finding four or more successes one of which is Boston (or any specific city) by chance alone is .0155. The inter-city results, therefore, fit what would be expected if Boston had a change that was not due to chance alone. However, the statistical analysis cannot provide a basis for determining whether this was the case. Nonetheless, on the basis of these results, we can conclude that there was no national trend that explains the change in youth homicide that occurred in Boston at the time of the Operation Ceasefire intervention.

Examination of the trends in youth homicides in the other cities with significant intervention coefficients also supports the distinctiveness of the

TABLE 3: Results of the Poisson Regressions for 39 Comparison Cities Plus Boston

City	*Estimate*	*Standard Error*	df	t	*Prob (t)*	*AR(1)*
Albuquerque, New Mexico	–0.1195	0.5783	24.18	–0.21	0.8380	0.1751
Atlanta, Georgia	–0.0336	0.3635	22.42	–0.09	0.9273	0.1973
Austin, Texas	–0.5207	0.4801	28.89	–1.08	0.2870	–0.0780
Baltimore, Maryland	0.2505	0.1974	26.03	1.27	0.2155	0.0134
Boston, Massachusetts	–1.1351	0.3771	25.98	–3.01	0.0057*	–0.0009
Charlotte, North Carolina	0.2948	0.4321	27.30	0.68	0.5009	0.0197
Chicago, Illinois	0.1764	0.1421	24.03	1.24	0.2264	0.2671
Cleveland, Ohio	0.2811	0.3947	21.94	0.71	0.4839	0.0877
Columbus, Ohio	0.3246	0.3478	21.83	0.93	0.3610	0.0470
Dallas, Texas	–0.5254	0.1786	28.79	–2.94	0.0064*	–0.1270
Denver, Colorado	–0.6698	0.4514	24.55	–1.48	0.1505	0.0576
Detroit, Michigan	0.2675	0.1873	21.57	1.43	0.1677	0.2207
El Paso, Texas	–0.1672	0.6274	28.03	–0.27	0.7918	–0.0866
Fort Worth, Texas	0.1385	0.4273	24.45	0.32	0.7485	0.1756
Fresno, California	0.0347	0.4260	25.14	0.08	0.9357	0.1952
Honolulu, Hawaii	–0.0443	0.6515	27.94	–0.07	0.9463	–0.0447
Houston, Texas	–0.3069	0.1972	24.97	–1.56	0.1322	–0.0108
Indianapolis, Indiana	–0.0577	0.3267	27.65	–0.18	0.8611	–0.0313
Jacksonville, Florida	–0.5670	0.2693	29.28	–2.11	0.0439*	–0.1637
Kansas City, Missouri	–0.5239	0.3483	24.75	–1.50	0.1452	0.0106
Los Angeles, California	–0.2324	0.1421	26.09	–1.64	0.1140	–0.0156
Long Beach, California	–0.3046	0.4892	24.59	–0.62	0.5393	0.1625
Memphis, Tennessee	–0.0328	0.3147	23.78	–0.10	0.9178	0.1029
Milwaukee, Wisconsin	–0.3408	0.2659	28.52	–1.28	0.2102	–0.1194
Nashville, Tennessee	0.1387	0.2936	31.11	0.47	0.6400	–0.1854
New York, New York	0.1583	0.1442	23.63	1.10	0.2833	0.1144
Oakland, California	–0.1766	0.3877	23.11	–0.46	0.6530	0.1336
Oklahoma City, Oklahoma	0.2657	0.6092	28.94	0.44	0.6659	–0.0299
Philadelphia, Pennsylvania	0.3227	0.1659	25.19	1.95	0.0629	0.0177
Phoenix, Arizona	–0.4195	0.2500	26.31	–1.68	0.1053	0.0207
Portland, Oregon	–0.3787	0.5133	30.06	–0.74	0.4663	0.0107
San Antonio, Texas	–0.2199	0.2907	30.09	–0.76	0.4553	–0.1754
San Diego, California	0.2118	0.5302	22.54	0.40	0.6933	0.1404
San Francisco, California	0.1256	0.4518	27.36	0.28	0.7831	0.0357
San Jose, California	–0.2445	0.6483	24.19	–0.38	0.7094	0.2625
Seattle, Washington	0.4182	0.6829	22.71	0.61	0.5463	0.1630
St. Louis, Missouri	–0.5068	0.2925	24.22	–1.73	0.0959	0.0772
Tucson, Arizona	–0.1741	0.4770	25.63	–0.37	0.7180	0.0143
Tulsa, Oklahoma	0.0213	0.6573	28.78	0.03	0.9744	0.1115
Virginia Beach, Virginia	1.2287	0.5968	29.43	2.06	0.0485*	–0.1935

NOTE: Deviance = 3613.23; dispersion parameter = 0.8616.
*$p < .05$.

Boston case (Figure 3). Virginia Beach, for example, shows a significant increase in youth homicides occurring in June 1996, although the yearly counts of youth homicides were stable between 1995 and 1997.[11] The declines in Dallas and Jacksonville both began months earlier than that in Boston. We are unaware of any known connection between youth homicides in these four cities. Although based on exploratory analysis, the presence of these differences undermines the argument that the changes in Boston reflect trends in other major U.S. cities.

Of course, other cities may have experienced a sudden significant decrease in youth homicide either before or after Boston experienced its significant decrease in youth homicide, and these might be missed by the single-time-period analysis presented in Table 3. Therefore, we conducted an exploratory analysis to identify abrupt significant youth homicide reductions in the comparison cities occurring in other months during the time series. We performed our main analysis of youth homicides in 39 major U.S. cities with a varying intervention point from month 12 to month 72 in the time series.[12] Five out of 39 cities experienced a sudden significant youth homicide reduction at some point in the time series.[13] These cities were Philadelphia; Tucson, Arizona; Dallas, Texas; Los Angeles; and New York City. A sharp and sustained break will lead to significant before and after differences for several time periods around the intervention. This is because the analyses are, in essence, comparisons of two means adjusted for other factors (Piehl et al. 1999). For this reason, significant break points in Boston are found in months 65 through 67 rather than just in month 66 (the June 1996 start date). Results in the 5 cities with significant breaks indicate that each had a series of successive significant breaks.

Although five cities experienced large reductions in youth homicide at some point within the time series, it is difficult to make a direct link between youth homicide trends in the five cities and Boston, as the yearly trends across cities look different. Philadelphia experienced significant reductions in monthly counts of youth homicides in months 36 (December 1993) through 38 (February 1994), 30 months before the implementation of Operation Ceasefire (Figure 4). This was followed by a steady increase in youth homicide between 1994 and 1997 (Figure 4). Tucson experienced significant decreases in monthly youth homicide counts between month 59 (November 1995) and month 60 (December 1995). This sudden decrease was followed by an increase in Tucson youth homicides in 1997 (Figure 4). Dallas experienced a significant decrease in the monthly count of youth homicides between month 63 (March 1996) and month 65 (May 1996). Although this significant reduction coincides with the implementation of Operation

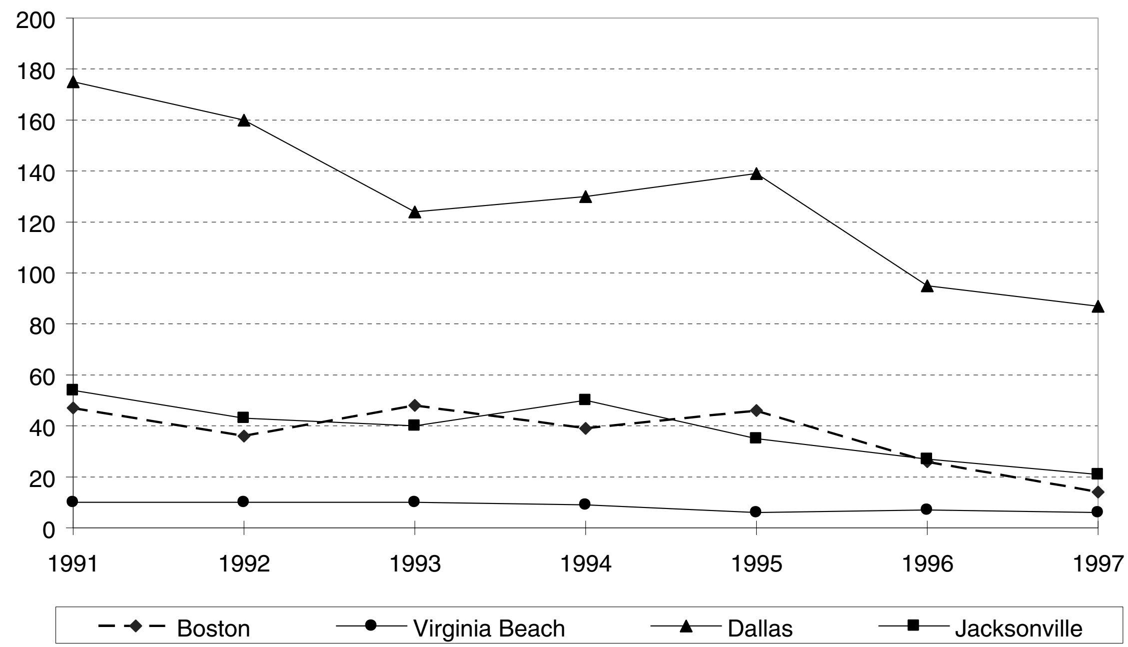

Figure 3: Youth Homicide in Boston, Dallas, Jacksonville, and Virginia Beach, 1991 to 1997 (annual)

Ceasefire, youth homicide in Dallas declined almost linearly between 1991 and 1997 (Figure 4).

Los Angeles experienced a significant reduction in monthly counts of youth homicides during months 30 and 31 (June and July 1993). New York City experienced sudden significant reductions in monthly counts of youth homicides during months 39 and 40 (March and April 1994) and also during months 44 and 45 (August and September 1994). As in Dallas, youth homicide trends in Los Angeles and New York show steep declines during the mid 1990s (see Figure 5). Superficially, the steady declines in New York, Los Angeles, and Dallas seem different from the trajectory of youth homicide in Boston. Overall, the results from this analysis do not support the idea that changes in Boston either followed or trailed national changes or changes in other major cities.

We also used this technique to examine whether Boston's youth homicide reduction could have been influenced by decreases in regional youth homicide trends. We obtained monthly counts of the number of homicide victims ages 24 and younger for 29 large New England cities[14] from SHR data for the time period of January 1991 through December 1997. The majority of the New England cities experienced very small numbers of youth homicides and did not exhibit any discernable trends. The youth homicide time series of 11 (37.9 percent of 29) New England cities were analyzed statistically.[15] When the main analyses were run with the varying intervention point, none experienced a significant reduction in the monthly count of youth homicides.

Careful within-city studies are necessary to unravel youth homicide trends in these cities. Without the benefit of a detailed analysis, it is difficult to know whether there is some broad link between the youth homicide trajectories in such diverse cities. Although some cities may have experienced a similar decrease, these analyses suggest that Boston's significant youth homicide reduction associated with Operation Ceasefire was distinct when compared to youth homicide trends in most major U.S. and New England cities.

The Role of Preventing Illegal Firearms Trafficking

Finally, there is the question of what degree, if any, of violence reduction in Boston should be attributed to the prevention of illegal firearms trafficking. Trafficking was, of course, one of the principal original foci of the Gun Project and attention to trafficking one of Operation Ceasefire's two fundamental planks. Evaluating the particular contribution of supply-side interventions in Boston is, we believe, essentially impossible. Antitrafficking efforts were

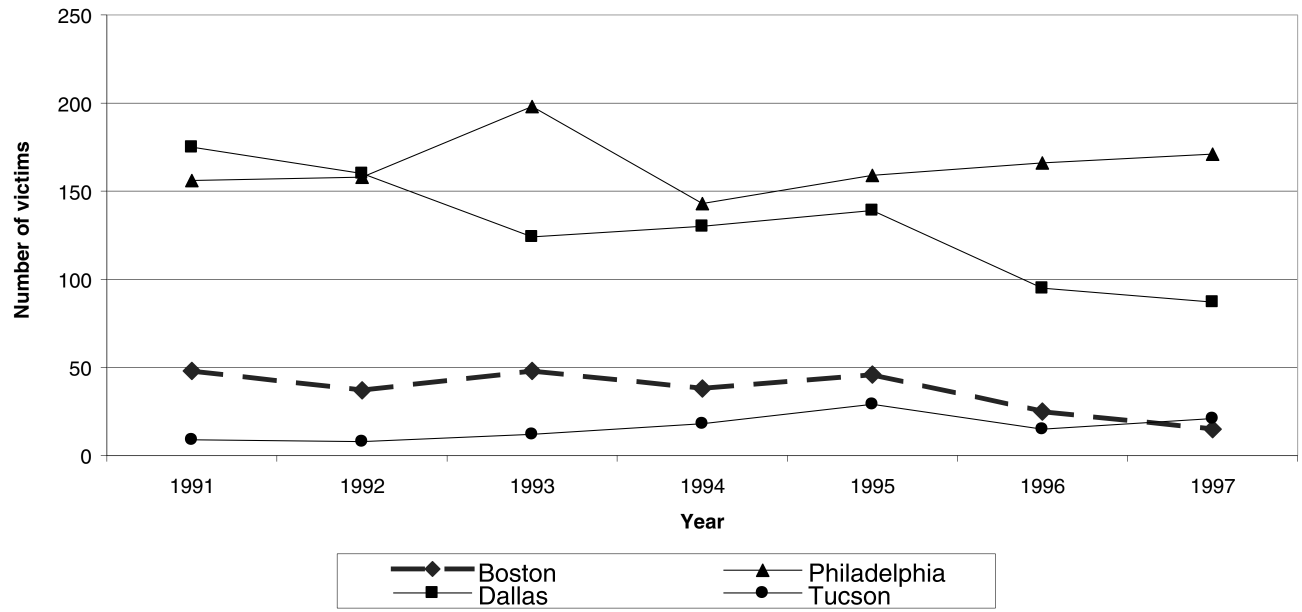

Figure 4: Youth Homicide in Boston, Dallas, Tucson, and Philadelphia, 1991 to 1997 (annual)

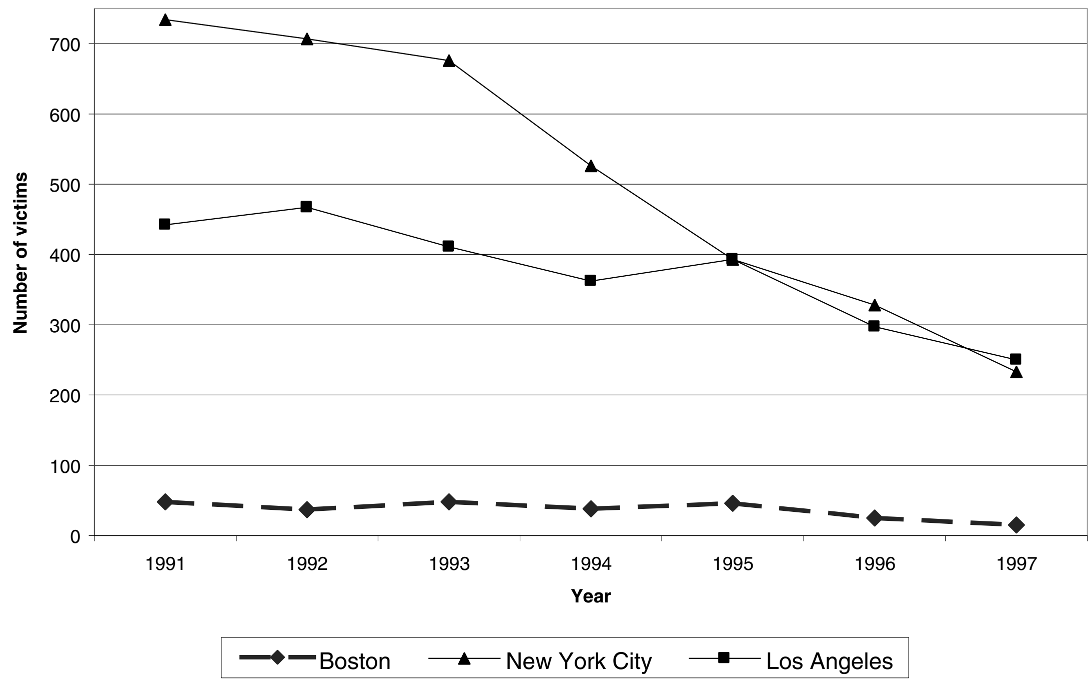

Figure 5: Youth Homicide in Boston, Los Angeles, and New York City, 1991 to 1997 (annual)

implemented at the same time as violence deterrence efforts, and both might be expected to influence, for example, gun carrying, gun use, and the mix of illegal guns found on the street. A stand-alone trafficking prevention intervention would not face these difficulties and could lead to definitive answers on the impact of supply-side interventions. Operation Ceasefire, however, was not a stand-alone trafficking prevention intervention.

Here, as well, the distinctive characteristics of the decline in homicide and shootings in Boston offer the best insight into what might have happened. Two things are certain. First, supply-side efforts cannot be responsible for the abrupt reductions in gun-related violence over the summer of 1996. Boston trafficking cases follow that reduction rather than anticipate it. Second, antitrafficking efforts in Boston did nothing to reduce the existing stockpile of illegally acquired and possessed firearms in Boston. Those guns held by gang members in Boston in May of 1996 were, for the most part, still held by them several months later when the violence reached its new, lower equilibrium. The change that had occurred was not in the extent of gun *ownership* but in gun *use*. The principal impact therefore was nearly certainly a demand-side, deterrence-based effect rather than a supply-side effect. It may well be that antitrafficking efforts strengthened and prolonged that impact. Whether any such effects were large or small cannot be independently established in this case.

CONCLUSION

The Boston Gun Project was an attempt to bring problem-oriented policing to bear on one important problem, youth violence, in one city, Boston. The project assembled a working group with members from a wide variety of agencies and representing a wide variety of law enforcement, social service, and other operational capacities (Kennedy et al. 1996). It went through a variety of shifts typical of problem-solving operations: shifts in the problem definition, in the shape of the intervention, and in the management and membership of the core operational partnership. Its core operational intervention, Operation Ceasefire, was designed to operate anywhere in the city where youth violence was a serious problem and was intended to interrupt the self-sustaining cycle the Gun Project hypothesized to be driving youth violence in the city (Kennedy et al. 1996). The pulling-levers deterrence strategy at the heart of Operation Ceasefire was designed to influence the behavior, and the environment, of the core population of chronic-offender gang-involved youth Gun Project research found to be at the heart of the city's youth violence problem (Kennedy 1997).

As we have noted, these interests and diagnoses—the desire to operate wherever youth violence presented itself and the belief that there was essentially one dynamic, which had to be addressed, driving violent behavior by various groups in various places within the city—made a classic experimental evaluation design impossible. It was appropriate neither from the viewpoint of participating agencies nor from the perspective of the forces believed to be driving youth violence to set aside particular areas, groups, or individuals as controls. There are thus irreducible limits to attributing any violence reduction in Boston to any particular operational intervention.

This article makes a weaker but still meaningful case: that there was an innovative intervention implemented, there were subsequent substantial reductions in youth violence in Boston, the timing of those reductions is consistent with the intervention having impact, those reductions were robust relative to proxy measures of rival causal factors in the city, the reductions in Boston were significantly larger than those in most other American cities at the time, and the large and abrupt changes that characterized the reduction in Boston differed from those of other American cities. There seems, then, to be reason to believe that something distinct happened in Boston and that its impact was both larger and of a different character than either secular trends or deliberate interventions then operating in other cities.

The results of the impact evaluation support the growing body of research that asserts that problem-oriented policing can be used to good effect in controlling crime and disorder problems (Braga, Piehl, et al. 1999; Clarke 1992; Eck and Spelman 1987; Goldstein 1990). In particular, the Ceasefire intervention suggests a new approach to controlling violent offenders from a more focused application of deterrence principles. In contrast to broad-based "zero tolerance" policing initiatives that attempt to prevent serious offending by indiscriminately cracking down on minor crimes committed by all offenders, the pulling-levers deterrence strategy controlled violence by focusing on particular groups that were behaving violently, subjecting them to a range of discretionary criminal justice system action, and directly communicating cause and effect to a very specific audience. Unfortunately, we were not able to collect the necessary pretest and posttest data to shed light on any shifts in street-level dynamics that could be associated with the pulling-levers deterrence strategy. Our research efforts during the pretest phase were focused on problem analysis and program development. A priori, we did not know what form the intervention would take and who our target audience would be. In this regard, our assessment is very much a "black box" evaluation. Additional research on the deterrence mechanisms of the pulling-levers approach to controlling offenders is necessary.

We believe that the research presented here shows that the Boston Gun Project was a meaningful problem-oriented policing effort, bringing practitioners and researchers together in new ways, leading to a fresh assessment of the youth violence problem in Boston, and leading to operational activities that were a substantial departure from previous practice. The principal intervention, Operation Ceasefire, was likely responsible for a substantial reduction in youth homicide and youth gun violence in the city. At first blush, the effectiveness of the Operation Ceasefire intervention in preventing violence may seem unique to Boston. Operation Ceasefire was constructed largely from the assets and capacities available in Boston at the time and deliberately tailored to the city's particular violence problem. Operational capacities of criminal justice agencies in other cities will be different, and youth violence problems in other cities will have important distinguishing characteristics. However, we believe that the working-group problem-solving process and the pulling-levers approach to deterring chronic offenders are transferable to other jurisdictions. A number of cities have begun to experiment with these frameworks and have experienced some encouraging preliminary results (see, e.g., Coleman et al. 1999; Kennedy and Braga 1998). These cities include Minneapolis, Minnesota; Baltimore; Indianapolis, Indiana; Stockton, California; Lowell, Massachusetts; Los Angeles; Bronx, New York; High Point, North Carolina; Winston-Salem, North Carolina; Memphis, Tennessee; New Haven, Connecticut; and Portland, Oregon.

The Boston Gun Project applied the basic principles of problem-oriented policing to a substantial public safety problem. Addressing this problem required the involvement of multiple agencies and the community as well as substantial investments in analysis, coordination, and implementation. The experience of the Gun Project suggests that deploying criminal justice capacities to prevent crime can yield substantial benefits. The problem-solving orientation of the project means that the problem definition, the core participants, and the particulars of the intervention evolved over the course of the collaboration. Operation Ceasefire itself was highly customized to the goals of the collaboration, the particular nature of the youth violence problem in Boston, and the particular capacities available in Boston for incorporation into a strategic intervention. Therefore, Operation Ceasefire as such is unlikely to be a highly specifiable, transportable "technology." However, certain process elements of the Boston Gun Project, such as the central role of the line-level working group and the use of both qualitative and quantitative research to "unpack" chosen problems, should be generally applicable to other problem-solving efforts. Using the working-group problem-solving approach, criminal justice practitioners in other jurisdictions will develop a set of intervention strategies that fits both the nuances of their youth violence problem and their operational capacities. Although the resulting package of interventions may not closely resemble the tactics used in Operation Ceasefire, the frameworks will be similar.

NOTES

1. We expanded our study to include youth ages 24 and younger when Boston Gun Project research revealed that street gangs were an important driver in youth gun violence. Most Boston gang members were between the ages of 14 and 24 (see Kennedy, Braga, and Piehl 1997).

2. During the problem analysis phase of the project, the authors did not provide or press a definition of *gang* on the members of the working group. Defining gang is a core problem in analyzing and understanding gang- and group-related youth crime and violence (Ball and Curry 1995). The character of criminal and disorderly juvenile gangs and groups varies widely both within and across cities (Curry, Ball, and Fox 1994). The members of the working group used a definition that could be reduced to self-identified group of kids who act corporately (at least sometimes) and violently (at least sometimes) (see Kennedy et al. 1997).

3. There were, in fact, only two major Ceasefire crackdowns. In May 1996, the Vamp Hill Kings were subjected to a multiagency operation that included street drug enforcement and drug market suppression, warrant service, stepped-up street enforcement by the Boston Police Department (10 arrests), Operation Night Light probation visits to suspected gang members (38 home visits, 10 probation surrenders), parole visits, 4 Department of Youth Services surrenders, seizure of pit bull dogs by animal control, special bail conditions established for cases presented to Massachusetts district courts, and 4 cases accepted for prosecution by the U.S. Attorney (3 pled guilty, 1 was deported). In August 1996, the Intervale Street Posse was subjected to a similar multiagency operation that included 15 federal arrests on drugs and homicide conspiracy charges (those federally charged were held out of state on pretrial detention) and 8 state drug arrests prosecuted by Suffolk County District Attorney.

4. We pursued these analyses to ensure that we were accounting for possible sources of error in our generalized linear models and did not use auto regressive integrated moving average (ARIMA) models to measure intervention effects. Identifying appropriate ARIMA models for evaluation purposes can be a very subjective exercise. As Gary Kleck (1995) suggests,

> Experts in ARIMA modeling also commonly point out difficulties that even experienced practitioners have in specifying time series models. Specification is very much an art rather than a science, so that different researchers, using the same body of data, can make substantially different, even arbitrary, and, as a result, obtain sharply different results. (P. 354)

5. The trend variable was simply the month number from the start to the end of the time series (i.e., for the January 1991 through December 1997 series, the trend variable ranged from 1 to 84). We also ran the model (Dependent Variable = Intercept + Month Dummies + Trend + Error) on the preintervention series with trend squared. Trend squared did not improve the fit of the model to any of the preintervention time series. The trend variable improved the fit of the model to all of the preintervention time series with the exception of youth homicides. This was not surprising because the youth homicides were very stable during the preintervention times series.

6. The OFFSET option sets the interval length in the GENMOD procedure. We created a series of if-then statements that assigned the appropriate interval lengths per month in the offset specification.

7. We ran separate models with the PSCALE and DSCALE options in the SAS GENMOD procedure. The PSCALE option uses the Pearson's chi-square divided by the degrees of freedom as the dispersion parameter, and the DSCALE option uses the deviance statistic divided by the degrees of freedom as the dispersion parameter. Neither option altered the significant reductions in shots-fired calls for service associated with the Ceasefire intervention.

8. We ranked the top 40 cities according to U.S. Census population estimates in 1990 and 1996. In this procedure, we observed that Fresno, California, and Tulsa, Oklahoma, were not in the top 40 in 1990 but were in the top 40 in 1996. St. Louis, Missouri, and Oakland, California, were in the top 40 in 1990 but not in the top 40 in 1996. Rather than exclude either pair of cities, we decided to keep both pairs in the sample. After Boston was removed from this group of populous cities, we were left with 41 cities.

9. Relative to other models, this model fit the data best, and, by allowing trend, trend squared, month, and autocorrelation effects to vary within each city, we believed that it best allowed the model to reflect the reality of heterogeneous error structures across the city time series. We compared the goodness of fit of the various models by examining the deviance statistics; a smaller deviance indicates a better fit of the model to the data. The deviance of the reported model was 3,613.23. Other models included a model predicting monthly counts of youth homicides as a function of month effects within each city, intervention effects within each city, and an autoregressive component within each city (deviance = 4,130.26); a model predicting monthly counts of youth homicides as a function of simple linear and nonlinear trends within each city, intervention effects within each city, and an autoregressive component within each city (deviance = 6,403.99); a model predicting monthly counts of youth homicides as a function of simple linear and nonlinear trends within each city, month effects within each city, and an autoregressive component within each city (3,680.77); and a model predicting youth homicides as a function of simple linear and nonlinear trends within each city, month effects within each city, intervention effects within each city, and a simple variance component within each city (3,611.80). Note that including an autoregressive component within each city time series does not supply a significantly better fit when compared to a model with a simple variance component within each city time series. Although there is almost no autocorrelation in the Boston time series, there are a number of cities that have strong autocorrelations in their respective time series (see Albuquerque, Atlanta, Chicago, Detroit, Fort Worth, Fresno, San Antonio, San Jose, and Virginia Beach in Table 3). Therefore, we felt that it was important to include an autoregressive component in our model. Finally, also note that the addition of the intervention variable does not significantly improve the fit of the overall model to the data. A priori, this is what we expected. A significant improvement in fit would indicate a strong nationwide effect coinciding with the implementation of Operation Ceasefire. This would suggest that there was nothing unique about Operation Ceasefire's effect on youth homicides in Boston.

10. Earlier drafts of this article contained somewhat different results than reported here. Specifically, in the intercity analysis in this article, several additional cities are found to have statistically significant results. Prior analyses were based on the SAS GLIMMIX macro available for version 6.12 of SAS. That macro calls the SAS procedure Proc Mixed. Due to an error in that version of Proc Mixed, the degrees of freedom are estimated incorrectly when the Satterthwaite method is used (SAS Institute 1998). This, in turn, led to the incorrect calculation of p values associated with the t tests performed on the parameter estimates. This article presents the correct p values. This error, which is limited to models with specific variance structures, including AR(1), has been resolved in later versions of SAS.

11. The Supplementary Homicide Report data reported the following yearly counts for youth homicides in Virginia Beach: six in 1995, seven in 1996, and six in 1997. The June 1996 significant break was due to a period of four months without youth homicides followed by a period of six months with one youth homicide each.

12. The intervention point could vary from month 12 to month 72 only rather than the full time period of month 1 to month 84. Twelve months were excluded at either end of the time series to ensure enough data to identify trends and autocorrelation in the time series.

13. For this analysis, we used the standard of a p value of .01 or less to define a significant break. We chose this level to decrease the risk of Type II error in an analysis that involved 50 tests

in each of 40 cities (expected to yield 100 breaks if a .05 level were used). However, we chose not to move to an even more stringent alpha level both because the temporal nature of the data made it likely that the within-city tests were not independent and because exploratory analyses should not be overly restrictive. Although using a .05 level would identify a number of additional significant breaks, these are in the form of one- or two-month spikes rather than sustained change.

14. We selected all New England cities with populations of more than 60,000. These 29 cities were Bridgeport, Danbury, Hartford, New Britain, New Haven, Norwalk, Stamford, and Waterbury in Connecticut; Brockton, Cambridge, Fall River, Framingham, Lawrence, Lowell, Lynn, New Bedford, Newton, Quincy, Somerville, Springfield, and Worcester in Massachusetts; Portland, Maine; Nashua and Manchester in New Hampshire; and Cranston, Pawtucket, Warwick, and Providence in Rhode Island. Although it has only 50,000 residents, we included Burlington in this pool because it was the only major "city" in Vermont.

15. These cities included Bridgeport, Hartford, New Haven, Stamford, and Waterbury in Connecticut; Providence, Rhode Island; and Lynn, Lowell, Springfield, and Worcester in Massachusetts.

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