DID *CEASEFIRE*, *COMPSTAT*, AND *EXILE* REDUCE HOMICIDE?*

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Research Summary:
Police officials across the United States often claimed credit for crime reductions during the 1990s. In this article, we examine homicide trends in three cities that mounted widely publicized policing interventions during the 1990s: Boston's Operation Ceasefire, New York's Compstat, and Richmond, Virginia's Project Exile. Applying growth-curve analysis to data from the 95 largest U.S. cities and controlling for conditions known to be associated with violent crime rates, we find that New York's homicide trend during the 1990s did not differ significantly from those of other large cities. We find some indication of a sharper homicide drop in Boston than elsewhere, but the small number of incidents precludes strong conclusions. By contrast, Richmond's homicide reduction was significantly greater than the decline in other large cities after the implementation of Project Exile, which is consistent with claims of an intervention effect, although the effect may have been small.

Policy Implications:
Criminologists gave police and other public officials something of a free ride as they claimed credit for the 1990s crime drop. We propose that researchers employ comparable data and methods to evaluate such claims-making, with the current analysis intended as a departure point for ongoing research. The use of common evaluation criteria is especially urgent for assessing the effects of the multiple interventions to reduce violent crime launched under the nation's primary domestic crime-control initiative, Project Safe Neighborhoods.

KEYWORDS: Homicide Trends, Police Interventions, Compstat, Operation Ceasefire, Project Exile.

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As crime rates in the United States plummeted in the 1990s, it was inevitable that police and other public officials would claim credit for the decline. How could they resist? Rates of violent crime, especially in the large cities, were falling to levels not seen since the 1960s (Rosenfeld, 2004). Falling crime rates are good news for politicians, and for good reason: "The potential political payoff is huge," a columnist wrote during New York's crime decline. "If the crime numbers continue their precipitous fall, no one will be able to beat Mr. Guliani when he comes up for reelection. . ." (Herbert, 1995). In his 1996 State of the Union address, President Clinton embraced the nation's crime drop and declared: "At last, we have begun to find a way to reduce crime" (Krauss, 1996). A few months later, Clinton greeted the release of FBI crime figures with the claim: "Because of our tough and smart decisions to put more cops on the street and get kids, guns and drugs off the street, we are now beginning to reverse the trend in violent crime" (Butterfield, 1996:A1).

President Clinton joined local officials around the country during the 1990s in what one journalist called "a chorus of self-congratulation" (Krauss, 1996). In city after city, officials attributed the falling crime rates, in whole or part, to a new program or policy they had instituted. Austin's mayor linked crime reductions to community and problem-oriented policing. Buffalo's police commissioner attributed the decrease to more aggressive enforcement, targeting career criminals, and a youth curfew. In Chicago, the addition of new officers and community policing were credited (the claims for Austin, Buffalo, and Chicago are described in *Law Enforcement News,* 1997). Detroit's police chief highlighted his efforts to "improve the image of the police with the community and get as many people involved as possible" (Butterfield, 1995:10). The Mayor of East St. Louis attributed the crime drop in his blighted city to more officers, new equipment, and antidrug and antigang-targeted patrols (Gillerman, 1996). Houston's chief cited computerized crime analysis, more officers, storefront stations in crime hotspots, and an antigang taskforce (Butterfield, 1995:10). In Los Angeles, officials credited hot-spot and community policing, but they also cited neighborhood crime prevention, a gang truce, and an earthquake as reasons for the crime drop (Feldman, 1994).[1]

Criminologists generally greeted such claims-making with skepticism and countered with alternative explanations for the crime drop, most featuring some combination of withered drug markets, the booming economy, mass incarceration, or the "little brother syndrome" (younger

1. Not all officials were willing to claim credit for the drop in crime. LA Deputy Police Chief Mark Kroeker told a reporter that, although the news was encouraging, "the last thing we should all do is say, "Oh, it's because of certain strategies we're talking about in police work. If you start taking credit for crime decreases, you better start getting ready to take the blame for crime increases as well" (Feldman, 1994:B2).

adolescents were turned off by the drug use and violence of older adolescents) (Lardner, 1997; see also Butterfield, 1996, 1998; Lacayo, 1996). Some criminologists acknowledged they had no idea why crime rates were decreasing and limited their public commentary to airy proclamations such as "I think we can now say a trend has been established" and "Something broad is happening" (Butterfield, 1996).[2] Overviews of the crime drop published as it was coming to an end contained elaborate statistical descriptions of the trends but remarkably little in the way of explanatory research on either the crime-reduction claims advanced by public officials or the counterclaims of the "experts" (Blumstein and Wallman, 2000; Cook and Laub, 2002; Rosenfeld, 2002, 2004). "The academic world," one critic observed, "has not distinguished itself in this field of inquiry" (Lardner, 1997:54).

Evaluating the impact of policy initiatives on crime trends is challenging under the best of circumstances; it is even more difficult to isolate whatever additional difference policy may have made when crime rates are already on the decline. Furthermore, most crime-control policies are not designed for valid evaluation, and those with built-in evaluation designs usually are intended to prevent the criminal behavior of individuals rather than reduce the crime rates of populations. An individualistic bias pervades criminology. In the midst of the 1990s crime decline, the significant advances in criminology were in the study of individual developmental patterns of delinquency and other problem behaviors (Farrington, 2003); analysis of aggregate crime trends remained on the periphery of the field (LaFree, 1998).

In this article, we seek to advance the study of policy impacts on crime trends through an assessment of homicide trends in three cities with widely publicized local law enforcement initiatives: Boston's *Operation Ceasefire*, New York's *Compstat*, and Richmond Virginia's *Project Exile*. Each program has been credited with reducing rates of serious violent crime, including homicide, and each has been subject to some form of outcome evaluation. However, to our knowledge, the crime-reduction effects of the three programs have never been evaluated with comparable data and methods, and the existing evaluations either lack systematic comparisons with crime trends in other cities or fail to control for other influences on crime trends, both of which are critical for separating the change in crime trends associated with policy interventions from changes caused by other factors. We use piecewise linear growth models to evaluate homicide trends in the three cities during, before, and in the case of Boston, after the intervention periods. Our models control for a broad range of other

2. And some criminologists, an anonymous reviewer reminds us, joined practitioners in touting the crime-reduction effects of local law enforcement interventions.

influences on homicide trends and are fit to data from all U.S. cities with a 1990 population greater than 175,000 (N = 95).

Let us be very clear about our objectives. We do not claim to evaluate the three interventions *per se*; we have neither the data nor the methods for doing so. Rather, we evaluate homicide trends in the three cities in and around the respective intervention periods and draw inferences from that evaluation about the accuracy of claims that the interventions reduced homicide. Those inferences cannot be as strong as many program proponents or critics would like, but they should be preferred over the baseless success claims and counterclaims and handful of disparate empirical investigations that have characterized evaluations of the three policing initiatives to date.

THE INITIATIVES

Ceasefire, *Compstat*, and *Exile* arguably are the most important local law enforcement initiatives intended to reduce serious criminal violence launched during the 1990s. Certainly, they are the best known. In this section, we describe each of the programs, their intended outcomes, claims asserting their success, and prior research on their crime-reduction effects.

BACKGROUND AND PROGRAM LOGIC

Ceasefire, *Compstat*, and *Exile* were launched against a backdrop of sharply escalating violent crime rates during the late 1980s and early 1990s. Homicides in Boston among youth aged 24 years and under more than tripled between 1987 and 1990 and remained above 1980s levels through the mid-1990s (Kennedy et al., 2001:1). New York registered a record 2,245 homicides in 1990, well above the previous record of 1,826 set in 1981, prompting one seasoned journalist to warn: "If there are 2,000 murders this year, get ready for 4,000. New York is dying" (quoted in Karmen, 2000:7). Richmond consistently ranked among the top ten U.S. cities in homicides per capita during the late 1980s and early 1990s; in 1994, it ranked number two, behind New Orleans, with a homicide rate of 80 per 100,000 residents (authors' calculations from the FBI's *Uniform Crime Reports*).

Although the three initiatives were similarly situated in a context of rising violent crime, they differed in problem diagnosis, design, and strategy. From 1996 to 1999,[3] Boston's *Operation Ceasefire* focused on firearm violence involving youth gangs. *Ceasefire's* central objective was to deter

3. Although the Boston police continue to employ certain tactics related to *Ceasefire*, in its original formulation the intervention effectively ended in 2000 (Braga, 2004). In our assessment of Boston homicide trends, therefore, we define the intervention period as 1996–1999.

youth firearm violence through direct communication to gang youth that firearm possession and use would not be tolerated, and all available levers would be pulled to ensure swift and tough punishment of violators.[4] The dual strategy of "retail deterrence" and "pulling levers" was implemented in a series of meetings with gang members involving police, youth workers, probation and parole officials, the U.S. attorney, and the local district attorney. "We're here because of the shooting," they would say. "We're not going to leave until it stops. And until it does, nobody is going to so much as jaywalk, nor make any money, nor have any fun" (Kennedy et al., 2001:27–28). The gang members were told to spread the word on the street. Youth workers also took the message directly to other gang youth. "Stop the Violence" posters were put up throughout areas with high levels of gang violence. Other posters described the sanctions, including federal charges and sentences, applied to recalcitrant gang members: "They were warned," the posters said. "They didn't listen (Kennedy et al., 2001:28–41; see also Kennedy, 1998).

A different kind of "retail deterrence" through aggressive order maintenance policing is at the heart of New York's *Compstat*. Begun in 1994 with the installation of William Bratton as police commissioner and continuing to the present, *Compstat* sought to restore order on the streets and accountability for crime in the police department. The police were no longer to tolerate minor offenses; they were to make arrests for vagrancy, vandalism, littering, minor drug possession, prostitution, public drunkenness and urination, aggressive panhandling, and harassment by "squeegee pests." The logic of the approach is taken from the so-called broken windows theory of crime causation: Minor crimes and disorder invite more serious offending by signaling that the police and community have lost control of the streets (Kelling and Bratton, 1998; Kelling and Coles, 1996; Wilson and Kelling, 1982). Aggressively restoring order reverses the message, thereby deterring more serious crime. In addition, frequent arrests for minor offenses on occasion net bigger fish, such as persons wanted on warrant for serious crimes or who are illegally carrying firearms or large quantities of drugs for sale. Other enforcement changes were introduced under Bratton, including "buy and bust" offensives against drug sellers and anti-gun patrols, but for Bratton the "linchpin strategy" was aggressive order maintenance policing (Bratton and Knobler, 1998:228).

The other component of the New York strategy is to hold commanders

4. A second objective of the Boston Gun Project, of which *Ceasefire* was a part, was to trace and interdict crime guns flowing to gang members (Kennedy et al., 1996, 2001:18–20). An alternative explanation of Boston's youth homicide decline in the mid-1990s emphasizes the role of a coalition of clergy members in improving relations between the police and minority residents and offering legitimacy to the Boston Gun Project (see Fagan, 2002; Winship and Berrien, 1999).

accountable for the crime rates in their precincts. At regular meetings in the "war room" at police headquarters, top administrators grill commanders on their knowledge of crime patterns and on their crime-reduction activities and plans (Karmen, 2000:92–94). A deterrence logic underlies this approach as well, now directed at police officials rather than at offenders: You are directly responsible for what happens under your command. If you fail, there will be consequences.

In practice it is nearly impossible to separate the two components of *Compstat* for evaluation purposes. Even in principle disagreement exists concerning which one should be credited for New York's crime decline during the 1990s. Bratton believes order maintenance policing was the "key" to falling crime rates. Mayor Giuliani, on the other hand, maintained that police reorganization and greater accountability were primarily responsible (Karmen, 2000:94–96, 120). Critics contend that order maintenance policing in New York departed markedly from the "broken windows" model, and that whatever effect it may have had on crime was because of aggressive stop-and-frisk tactics aimed primarily at racial and ethnic minorities (Fagan and Davies, 2001). In any event, our analysis is limited to the question of whether New York homicide trends differ significantly from those of other cities after *Compstat* was initiated. We cannot determine which aspects of *Compstat* (or *Ceasefire* or *Exile*) may have been responsible for observed differences in the trends.

A more traditional deterrence logic underlies Richmond, Virginia's *Project Exile*, which was formally initiated in February of 1997. *Exile* entails sentence enhancements through federal prosecution for violent or drug crimes involving firearms.[5] Federal sentences for such crimes generally are longer than those in state courts, bail is denied more often, and sentences are served in federal prisons likely to be located out of state (Raphael and Ludwig, 2003:254). By increasing the expected penalty for firearm-related offenses, the program is intended to deter firearm carrying and criminal use. By sentencing more violent offenders to longer prison sentences, the program also is intended to reduce crime by incapacitating violent felons. It is unclear whether deterrence or incapacitation is the primary objective of *Exile's* sentence enhancements. However, the broad "outreach" campaign accompanying the program is intended explicitly to deter criminal use of firearms. *Exile* is advertised extensively in print and electronic media and on city buses and business cards carrying the blunt and bleak warning that "An illegal gun will get you five years in federal prison," the

5. Specifically, the program covers felon-in-possession-of-a-firearm cases and drug and domestic violence cases involving firearms (Raphael and Ludwig, 2003:253–254). See the U.S. Attorney's description of *Exile* at http://www.vahv.org/Exile.

mandatory minimum sentence for federal "felon-in-possession" cases (http://www.vahv.org/Exile). There is, then, something of a "retail" component to *Project Exile's* practice of deterrence, if not as precisely targeted to particular groups and neighborhoods as was the case in Boston's *Operation Ceasefire*.

PRIOR OUTCOME EVALUATIONS

The most important obstacle in the way of rigorous outcome assessments of the three programs is that each of them was implemented citywide, leaving thereby no like situated areas without the intervention that might serve as within-city controls, A second obstacle to reliable evaluation is that the different aspects of each program were implemented more-or-less at the same time, so that even if a "program effect" was found, it would not be obvious which program component should be credited (sentence length? advertising? change in enforcement? change in command accountability? which levers?). Not surprisingly, then, those few outcome evaluations that have been conducted take the form of single-case time-series investigations with limited comparisons with crime trends in other cities.

New York. The single exception is Kelling and Sousa's (2001) study of violent crime trends during the 1990s in New York's 76 police precincts. Controlling for changes in borough-level unemployment, age composition, and a measure of drug involvement, they find an association between decreases in violent crime and increases in "broken windows" policing, as measured by misdemeanor arrests. Kelling and Sousa (2001) interpret this result as evidence for a key plank of Bratton's policing reforms and against so-called root cause explanations of New York's crime drop and crime in general. Although the coincidence of rising misdemeanor arrest rates and falling violent crime rates across New York City precincts is compelling, Kelling and Sousa (2001) do not control for several additional factors commonly associated with violent crime (e.g., poverty, family structure, immigration trends) and fail to consider the possibility of reciprocal causation between their policing measure and violent crime: Decreases in violent felonies may enable the police to devote greater attention to less serious offenses, which results in increases in misdemeanor arrests.

Other assessments of *Compstat* are less sanguine but also less conclusive about its crime-reducing consequences. Several investigators have pointed out that New York's sharp decline in homicide and other violent crimes during the 1990s was not unique; San Diego, San Antonio, Houston, San Francisco, Los Angeles, and other large cities that had not implemented similar policing reforms also registered very sizable declines (Harcourt, 2001:90–94; Joanes, 2000). In an early assessment, Fagan et al. (1998) observed that nongun homicides had begun to fall in New York well before the *Compstat* reforms were implemented in 1994. They also find, however, that the drop in firearm homicides is more consistent with the timing of *Compstat* and conclude that the policing changes may have contributed to that decline. Even a harsh critic of broken windows policing concedes that it "contributed in some degree to the decline in crime in New York City" (Harcourt, 2001:103). Finally, the most comprehensive assessment of the sources of New York's crime drop concludes that changes in policing likely accounted for some decrease, along with a booming economy, escalation in imprisonment, shrinking drug markets, favorable demographic trends, and possible changes in the values of adolescents and young adults. However, the author could draw no conclusions regarding the size of the policing effect (Karmen, 2000:262–266; see, also, Conklin, 2003).

In summary, most independent assessments of the policing changes under Bratton conclude that they probably reduced levels of violent crime in New York over and above the effects of other factors. The most definitive of the studies (Kelling and Sousa, 2001) does not compare New York crime trends with those in other cities. The others place New York trends in the context of those elsewhere, but they do not employ comprehensive samples of comparison cities or control for other determinants of violent crime.

Boston. The Harvard researchers who participated in the development of *Operation Ceasefire* compared the timing and magnitude of violent crime trends in Boston with those in other New England cities and a national sample of large cities (Braga et al., 2001; Kennedy et al., 2001; Piehl et al., 2003). In a two-part investigation, the researchers first performed a time-series analysis of monthly youth firearm homicides, gun assaults, and shots fired calls in Boston before and during the intervention period, controlling for changes in unemployment, size of the youth population, adult homicide victimization trends, index crime trends, and youth drug activity as measured by arrests. The results indicate a reduction in firearm violence, net of the controls, coinciding with the implementation of *Ceasefire*.

In the second part of the study, the researchers compared Boston monthly youth homicide trends with those in 29 other New England cities and 39 large U.S. cities, which they adjusted for linear and nonlinear trends in each series, monthly effects, an autoregressive component, and an intervention component in each series. They found three cities in addition to Boston with significant values on the intervention component, but either the direction or precise timing of those effects differ from Boston's.[6]

6. In a separate investigation, Piehl et al. (2003) found that the maximum "break"

The Harvard team concluded that *Ceasefire* was associated with a significant reduction in youth firearm violence. Their conclusion would be more persuasive had they included additional controls in both the within- and between-cities analyses they conducted. Nonetheless, their findings are consistent with claims of a program effect.

Richmond. Richmond's *Project Exile* has been subject to a single published outcome evaluation. Raphael and Ludwig (2003) evaluate claims that *Exile* significantly reduced firearm homicides in Richmond by comparing Richmond gun homicide trends during the 1980s and 1990s, Richmond trends with those in other cities, and adult and juvenile homicide arrest rates in Richmond. The first comparison shows that Richmond gun homicide rates had increased markedly during the 1980s, peaking in the early 1990s. Raphael and Ludwig (2003) conclude that Richmond's gun homicide decline during the 1990s was to be expected as part of a general regression to the mean common across U.S. cities with high homicide rates. They conclude from their comparison of Richmond's gun homicide trends with those of other cities that the proportional drop in Richmond's rates through the 1990s was not unusual. What was unusual was Richmond's gun homicide rate in 1997, the year *Exile* began. Raphael and Ludwig (2003) suggest that 1997 was subject to "transitory" influences because it broke a two-year "trend" that began in 1995 and resumed in 1998. Therefore, they dropped 1997 from their trend analyses. "Using this unusual year as the base for calculating the change," they maintain, "is bound to inflate the apparent impact of the program" (Raphael and Ludwig, 2003:258).

To control for unmeasured influences on Richmond homicide trends, Raphael and Ludwig (2003) compare trends in juvenile and adult homicide arrest rates. The logic of the comparison is that juveniles are not subject to *Exile* provisions (or not to the same extent as adult criminals) but are affected by other factors driving homicide rates. They find that juvenile homicide arrests increased slightly from 1995/1996 to 1998 (again, 1997 is omitted), but adult arrests increased even more over the same period. In other large urban counties, adult homicide arrests declined at a greater rate than did juvenile homicide arrests. Neither finding is consistent with the idea that *Exile* reduced homicide among Richmond adults.

In summary, Raphael and Ludwig (2003) find little evidence to support claims that Richmond's *Project Exile* reduced firearm or overall homicide rates. We regard their evaluation as suggestive but far from the final word on *Exile's* effects. We reserve our specific criticisms until we present the findings from the current analysis.

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in the Boston youth firearm homicide series occurred during the summer of 1996, just after *Ceasefire* began.

The results of the outcome evaluations of *Compstat*, *Ceasefire*, and *Exile* are mixed. Evaluations of *Compstat* and *Ceasefire* support claims that the interventions resulted in reductions in homicide. The single published study of *Project Exile* concludes that it had little apparent effect on Richmond firearm homicide rates. It is difficult to compare findings across the evaluations of the three interventions, because they targeted differing population groups and homicide types, and because they are based on distinct sources of data and methods of analysis.

We use the same data sources and methods to evaluate all three interventions. The logic of our approach assumes that an effective intervention will produce a reduction in homicide that (1) differs significantly from corresponding changes in comparison cities; (2) occurs during or after but not before the intervention period; (3) occurs in the specific type of homicide targeted by the intervention; and (4) is independent of other influences. By the same logic, an ineffective intervention will produce a reduction in homicide that does not differ significantly from changes in comparison sites or occurs before the intervention or does not occur in the homicides targeted by the intervention or is brought about by conditions other than the intervention. To be deemed "effective" (or not ineffective), an intervention must meet all four conditions. To be found ineffective, it must fail to meet only one of them.

ANALYTICAL STRATEGY

To assess the effects of the three interventions, we apply growth-curve models to homicide trends in Boston, New York, and Richmond. We estimate each city's homicide trend as a function of a baseline model of covariates fit to data for the 95 largest U.S. cities. An intervention's effectiveness is indicated by a reduction in homicide during the intervention period that is significantly greater than the average reduction for the sample. Our strategy cannot conclusively rule out other possible explanations for observed differences in homicide trends between cities with and without the interventions. However, the *absence* of such differences would place a particularly strong onus on program defenders to demonstrate that an observed homicide reduction resulted from a particular intervention.

DATA

The homicide data for the analysis were obtained from the Supplementary Homicide Reports (SHR) for the 95 U.S. cities with a 1990 population of 175,000 or more over the period 1992–2001. The data correspond to the distinct type of homicide targeted by each intervention. Boston's *Ceasefire* was designed to reduce firearm homicides among the "gang age" population of adolescents and young adults. The policing strategies in New York were intended to reduce homicides (and other crimes) of all types, regardless of weapon or age of the offender. Richmond's *Exile* specifically targeted firearm homicides. Accordingly, the analysis of *Ceasefire* was carried out with SHR data on victims of firearm homicide ages 15–24, the *Compstat* analysis with data on total homicide victimizations, and the *Exile* analysis with data on all firearm homicide victimizations. The SHR data were missing for 62 of the 950 city-years in the analysis. Estimated values for the missing data were obtained from within-city regressions of observed values on a period variable (1992 = 0).[7]

To obtain unbiased estimates of the effects of the interventions on homicide rates, we created baseline models incorporating well-established covariates of homicide (Land et al., 1990; Messner and Rosenfeld, 1998). Measures of social and economic disadvantage and population density were obtained from the 1990 Census and the 2000 Census. The density measure was logged to reduce skewness, and all measures were interpolated between census years and extrapolated to 2001. The disadvantage measure was derived from a principal components analysis of several highly intercorrelated variables: percent of families with children under age 18 headed by a female, percent black, median family income (logged), the male unemployment rate, the poverty rate, and the Gini coefficient of income inequality. This measure of economic and social disadvantage corresponds closely to that in Land et al. (1990).

Prior research has found that homicide trends are negatively related to trends in incarceration and police density (Levitt, 2002; Marvell and Moody, 1997; Spelman, 2000). We incorporated in our analysis the yearly state incarceration rate corresponding to each of the 95 cities, obtained from the Bureau of Justice Statistics, and an annual indicator of police per 100,000 city residents, computed from the number of sworn full-time officers reported in the Law Enforcement Management and Administration Statistics (LEMAS) surveys.

Finally, several studies have shown a connection between homicide trends and crack cocaine markets in U.S. cities (Baumer et al., 1998; Blumstein, 1995; Cork, 1999; Ousey and Lee, 2002). We developed a city-level

7. Data for the five Florida cities in the sample were missing for the years 1997–2001. Because they fall at the end of the series, our imputation procedure did not yield plausible values for these years, and they were left missing. Our analytic methods are robust to missing data (Raudenbush and Bryk, 2000:199–200).

An alternative homicide data source to the SHR is the homicide series compiled by the National Center for Health Statistics (NCHS). However, the NCHS data are available at the county level but not the city level, and the boundaries of several cities in our sample are not coextensive with county lines. In addition, the NCHS homicides are coded according to the victim's residence rather than location of the incident as in the SHR.

proxy measure of the level of cocaine use among arrestees from the 20 cities in our sample for which yearly Drug Use Forecasting (DUF) and Arrestee Drug Abuse Monitoring (ADAM) data were available between 1990 and 2000. The proportion of arrestees testing positive for cocaine reported by DUF-ADAM was regressed on a composite measure of socioeconomic disadvantage,[8] the percentage of city residents living in the same house for five or more years, and the percentage of the population residing in owner-occupied housing. The fit statistics indicated that the model could serve as a suitable proxy for city-level variation in cocaine involvement. The predicted values from the equation were used to estimate the extent of cocaine use in cities for which the DUF-ADAM data were unavailable.[9]

METHOD

The primary research question is whether the three intervention sites experienced greater declines in homicide than other cities during the intervention period after adjusting for between-city differences in other determinants. The analytic strategy must therefore provide estimates of both the within-city trajectories in homicide and between-city differences in those trajectories. We use growth-curve analysis to obtain these estimates. Growth-curve analysis estimates the homicide trends for each city and compares the intercept and slope parameters across cities. Traditionally, such trends are estimated as a polynomial function of time, with single parameters for the linear trend and change in the growth rate. Because our interest lies with the trends during the intervention periods, we estimate the linear components as a series of piecewise trends. The piecewise strategy estimates a linear trend component for successive subperiods of the series, in effect allowing the slope to "bend" at predetermined points. Two linear components are specified in the piecewise models for New York and Richmond, corresponding to the pre-intervention and intervention periods. Three linear components are specified for Boston, corresponding to the pre-intervention, intervention, and post-intervention periods.

The hierarchical generalized linear model (HGLM) has become a common approach to modeling change within- and across-individuals, schools, neighborhoods, and cities. This approach is an attractive alternative to multivariate repeated-measures (MRM) models, which can be difficult to

8. Socioeconomic disadvantage is a principal components factor score of percent black, percent female-headed families with children under 18, the male unemployment rate, median family income, the poverty rate, and poverty concentration (the proportion of the population residing in census tracts with a poverty rate of 40% or higher).

9. Details concerning the development and validation of the proxy measure are available from the authors.

specify when the number of cross-sectional units is relatively large and when there are multiple time periods under investigation, as is the case in our analysis (Greene, 2000). Because the plausible values for homicide rates are constrained to non-negative numbers, the dependent variable in our analysis has a truncated distribution, and a Poisson sampling model is used with city population as the exposure variable.

Given our interest in modeling homicide trends both within and between cities, we estimate two-level models. The annual homicide counts are treated as repeated measures, nested within cities in the level-1 equation. Time-varying explanatory variables that may be related to change in homicide within cities are included at level-1. In this analysis, three time-varying covariates are included in the level-1 model: police per capita, state incarceration rate, and estimated level of cocaine use. In a multilevel model, the parameter estimates from level-1 become the outcomes, and variables hypothesized to explain differences in those estimates across units are incorporated in the level-2 model. In our analysis of homicide trends in each of the three intervention sites, the key level-2 covariate is a dummy variable that distinguishes the specified intervention site from other cities in the sample. We also incorporate at level-2 the indicators of resource deprivation and population density. We allow the level-1 intercepts and slope coefficients to vary randomly across cities. A more detailed description of our methods is presented in Appendix I.[10]

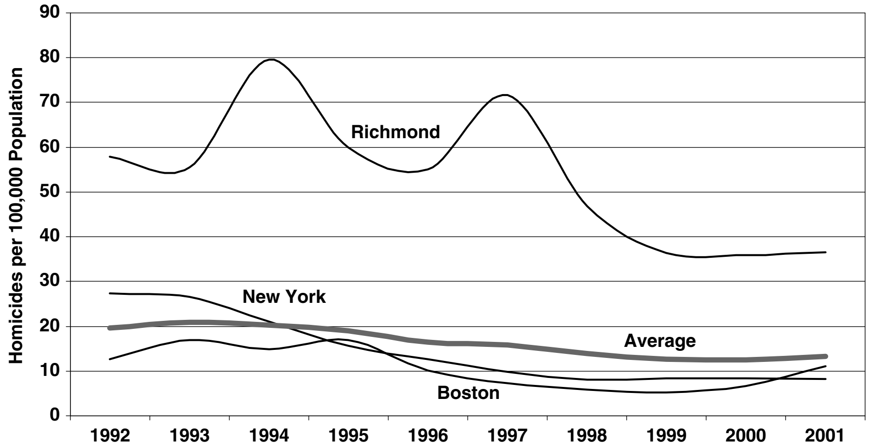
RESULTS

Figure 1 displays the average homicide trends for the 95 cities in our sample as well as trends in the three intervention sites: Boston, New York, and Richmond. As the figure shows, homicide rates declined in the largest U.S. cities during the 1990s to about 13 per 100,000 population at the end of the decade from 20 per 100,000 at the beginning. Homicide rates in Boston fell from roughly 15 per 100,000 during the early 1990s to about 6 per 100,000 at the end of the decade, only to rise again to 11 per 100,000 in 2001. In New York, they plunged to about 8 per 100,000 from 27 per 100,000 over the same period. Richmond homicide rates roller-coastered through 1997, reaching a peak of 80 per 100,000 in 1994, before falling to about 36 per 100,000 at the end of the decade. Figure 1 confirms that homicide rates dropped substantially in the three cities during their respective intervention periods, but it remains to be seen whether these declines were significantly greater than those observed elsewhere after adjusting for relevant correlates of crime trends.

To examine whether the observed homicide declines in Boston, New

10. HLM 5.04 statistical software was used for all analyses (Raudenbush et al., 2001).

FIGURE 1. HOMICIDE TRENDS IN US CITIES WITH 175,000 OR MORE RESIDENTS, 1992-2001 (N = 95)



York, and Richmond were significantly greater than those observed in other large cities, net of other factors, we applied piecewise linear growth-curve models to the trend in the type of homicide targeted by each intervention. The results of our analysis are summarized in Table 1 and presented in greater detail in Appendix II. Table 1 summarizes the results of the level-1 and level-2 estimations of "unconditional" and "conditional" models of the homicide trends before the intervention, during the intervention period, and for Boston after the intervention. The results from the unconditional models are unadjusted for the influence of the three time-varying (incarceration rate, police density, drug use) and two time-invariant covariates (resource deprivation and logged population density) included in the analysis.[11] The results from the conditional models are adjusted for the effects of these covariates. In both cases, the deviation of the trend for each of the three intervention sites from the sample average or "base" trend is presented.

11. In preliminary analyses, we evaluated the effects of other time-varying influences on homicide trends, including a measure of firearm availability (percent suicides with a firearm), concentrated poverty (percent population residing in census tracts with a poverty rate of 40% or higher), age structure (percent population between the ages of 15 and 24), and male unemployment rate (percent males age 16 and older unemployed). None of these measures yielded significant results or improved model fit and, with the exception of the male unemployment rate, were dropped from the analyses shown. The male unemployment rate was added as a time-invariant covariate to the disadvantage measure.

TABLE 1. HGLM SUMMARY RESULTS FOR CITY HOMICIDE RATES, 1992–2001 (N = 95)

A. Boston	**Unconditional Model**	**Conditional Model**
Initial (1992) Youth Firearm Homicide Rate, π_{0i}		
Base, β_{00}	1.435***	.339
	(.104)	(.782)
Boston, β_{01}	.212	–.146
	(.992)	(.632)
Pre-Intervention Change (1992-1994), π_{1i}		
Base, β_{10}	–.015	.188
	(.017)	(.148)
Boston, β_{11}	.006	–.008
	(.149)	(.161)
Intervention Change (1995–1999), π_{2i}		
Base, β_{20}	–.151***	–.173
	(.014)	(.135)
Boston, β_{21}	–.134	–.186
	(.123)	(.113)
Post-Intervention Change (2000–2001), π_{3i}		
Base, β_{30}	.012	–.043
	(.027)	(.269)
Boston, β_{31}	.372	.305
	(.238)	(.204)
B. New York		
Initial (1992) Homicide Rate, π_{0i}		
Base, β_{00}	2.697***	2.048***
	(.084)	(.598)
New York City, β_{01}	.612	–.521
	(.800)	(.493)
Pre-Intervention Change (1992–1993), π_{1i}		
Base, β_{10}	.066***	.452**
	(.023)	(.200)
New York City, β_{11}	–.209	.046
	(.159)	(.089)
Intervention Change (1994–2001), π_{2i}		
Base, β_{20}	–.079***	.040
	(.006)	(.059)
New York City, β_{2i}	–.090*	.027
	(.048)	(.041)
C. Richmond		
Initial (1992) Firearm Homicide Rate, π_{0i}		
Base, β_{00}	2.358***	1.658**
	(.093)	(.646)
Richmond, β_{01}	1.593*	.700
	(.891)	(.549)
Pre-Intervention Change (1992–1996), π_{1i}		
Base, β_{10}	–.064***	.175
	(.012)	(.133)
Richmond, β_{11}	.083	.113
	(.107)	(.111)
Intervention Change (1997–2001), π_{2i}		
Base, β_{20}	–.103***	–.108
	(.009)	(.098)
Richmond, β_{21}	–.069	–.144**
	(.075)	(.067)

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

BOSTON

Panel A of Table 1 presents the results for the trend in youth firearm homicide rates targeted by Boston's *Ceasefire*. The pertinent results are the coefficients for the sample average (minus Boston) and Boston's deviation from the average during the pre-intervention (1992–1995), intervention (1996–1999), and post-intervention (2000–2001) periods. The average youth firearm homicide rate for the sample exhibits no significant change during the pre-intervention period ($\beta = -0.015$). It dropped significantly during the intervention period ($\beta = -0.151$, $p < 0.01$), but that change becomes nonsignificant once the covariates are introduced in the conditional model. Boston's youth firearm homicide rate exhibits a greater decrease than the sample average during the intervention period, in both the unconditional and conditional specifications. Although neither difference is statistically significant, the difference between Boston and the sample average in the conditional model just misses the most permissive significance threshold ($p = 0.101$). Finally, the Boston trend did not deviate significantly from the sample average during the post-intervention period in either model.

By exponentiating and subtracting 1, the coefficients displayed in Table 1 can be converted to proportional increases or decreases in the outcomes. Doing so indicates that Boston's youth firearm homicide rate dropped an estimated 25% per year during the 1996–1999 intervention period, compared with a decline of 14% in the average rate during the same period. After adjusting the changes for the effects of the covariates, Boston's youth firearm homicide rate fell an estimated 30% per year during the intervention period, and the average rate dropped 16%.[12] Although the estimated decline in Boston's rate was nearly double that of the sample average in the conditional model, the difference is not statistically significant, even by a permissive standard of $p < 0.10$. The lack of statistical significance reflects Boston's low youth firearm homicide counts during the intervention period (ranging from 21 in 1996 to 10 in 1999). Relatively small year-to-year changes in the number of youth firearm homicides in

12. The percentage changes are derived as follows: The coefficients for the sample average in the unconditional and conditional models during the intervention period are –0.151 and –0.173, respectively. Exponentiating, subtracting 1, and multiplying the result by 100 yields respective yearly percentage changes of –14% and –15.9%. Boston's rate fell an additional –0.134 and –0.186 in the unconditional and conditional models, respectively. Summing these values with those for the average changes yields total changes in the Boston rates of –0.285 (–0.151 + –0.134) and –0.359 (–0.173 + –0.186). Exponentiating, subtracting 1, and multiplying the result by 100 yields yearly percentage changes in Boston's youth homicide rate of –25% in the unconditional model and –30.2% in the conditional model.

Boston resulted in large percentage changes. Therefore, the prudent conclusion regarding *Ceasefire's* impact is that it is statistically indeterminate given the small number of youth firearm homicides in Boston.

It is possible that the age range used to define the youth firearm homicide rate in our analysis is too narrow. In supplementary analysis, we reestimated the Boston models using a broadened victim age range of 11–24.[13] Widening the age range of victims adds only three homicides in Boston during the entire 1992–2001 period. Not surprisingly, the supplementary estimation results differ only slightly from the original results, with a marginally significant p-value for the "Boston effect" in the conditional model of 0.092. This result does not alter the substantive conclusion that *Ceasefire's* impact on youth firearm homicides in Boston is difficult to discern given the small number of incidents.

NEW YORK

Panel B of Table 1 shows the results for New York. The New York homicide trend did not deviate significantly from the sample average during the 1992–1993 pre-intervention period. The New York rate fell more sharply than the sample average during the intervention period, although the difference is only marginally significant at $p < .10$. However, when the covariates are introduced in the conditional model, the difference between the New York and the average homicide trend becomes nonsignificant. Thus, after adjusting for measured differences between New York and other large cities, the magnitude of the decline in homicide in New York between 1994 and 2001 seems not to have been atypical.

It is possible that *Compstat* had a greater effect on New York's firearm homicide rate than the total homicide rate, as Fagan et al. (1998) have suggested. However, substituting the firearm homicide rate for the total rate in the conditional model yields no significant deviation of New York's firearm homicide trend from the sample average during the intervention period (results not shown).

Consistent with prior research (Marvell and Moody, 1997), the growth in incarceration during the 1990s is consistently related to the homicide trends examined in this study. We cannot rule out entirely the possibility that *Compstat*, and perhaps *Ceasefire* and *Exile*, had an indirect effect on homicide through incarceration. Conceivably, the programs increased state prison sentences that, in turn, reduced homicide rates.

More research is necessary to determine how much of the growth in

13. Previous studies of *Ceasefire* broaden the age range even further to include all firearm homicide victims under the age of 25 (Braga et al., 2001; Piehl et al., 2003). However, we question whether *Ceasefire* legitimately can be expected to have reduced the number of infant killings or homicides involving very young children.

New York's state prison population was caused by the policing changes under *Compstat*. New York's imprisonment rate grew steadily through the 1980s and 1990s, with no evident acceleration after 1994 (Karmen, 2000:147). Moreover, the proportion of New York City felony arrests resulting in a prison sentence peaked in 1993, the year before *Compstat* was introduced, and declined thereafter (Karmen, 2000:155–156). A growing felony arrest rate could still have produced an increase in the size of the prison population, even after the imprisonment-to-arrest ratio began to decline. However, *Compstat* could not have contributed to the growth in New York's prison population that occurred before 1994, and it is not likely that it was responsible for all of the growth after 1994.

RICHMOND

The Richmond story differs from those in Boston and New York. The unconditional model shows that Richmond's firearm homicide rate fell by nearly 16% per year after *Exile* was introduced in 1997, but that decrease is not significantly greater than the almost 10% average reduction in firearm homicide for the sample. However, after controlling for other factors, Richmond's firearm homicide rate exhibits a 22% yearly decline, whereas the average reduction for the sample remains about 10% per year. That difference is statistically significant ($p < 0.05$).

This finding differs from those obtained by Raphael and Ludwig (2003) in their evaluation of *Exile*, which concluded that the intervention had little effect on Richmond's firearm homicide rate. The discrepancy may be from the use of a longer firearm homicide series in the current study. Raphael and Ludwig (2003) analyzed firearm homicide rates through 1999, whereas the series used in this study extends the intervention period two additional years to 2001. Raphael and Ludwig (2003) also omitted the year 1997 from their analysis, on the grounds that the unusually high rate of firearm homicide in Richmond that year constitutes an unreliable base on which to gauge the effectiveness of *Exile*. We question this analytical decision. It is not evident why 1997 should be dropped from the Richmond data and not, for example, 1994, when the homicide rate spiked to 80 per 100,000 (see Figure 1; see, also, Raphael and Ludwig, 2003:262, Figure 7-3B). Compared with other large cities, Richmond's high level of homicide is "unusual" in general and not just in 1997.

Nonetheless, it is true that assessments that include 1997, the year *Exile* was introduced, are more apt to find a program effect than those from which 1997 is excluded. We therefore omitted 1997 from our analysis and replaced it with the 1996–1998 average firearm homicide count. Reestimating the conditional model on the revised series reveals a marginally significant downward departure of Richmond's firearm homicide trend from the sample average during the intervention period ($p < 0.10$; results not shown). We also respecified the conditional model by including the 1992 and 1997 firearm homicide rates as additional covariates in the pre-intervention and intervention estimations, respectively. Controlling for between-city differences in initial levels of firearm homicide helps to account for possible selection bias associated with the location of crime control interventions in high-crime cities and minimizes omitted variable bias by capturing unmeasured influences on homicide. No substantively meaningful differences emerge from the reanalysis; in fact, the "Richmond effect" becomes somewhat stronger with the initial firearm homicide rates in the model (results available on request).

It would have been instructive had Raphael and Ludwig (2003) also compared their results with and without *Exile's* first year in the analysis. However, the primary reason for the difference between the current results and those from the Raphael and Ludwig (2003) study probably is the inclusion of other determinants of homicide trends in our analysis. We find a significant divergence between Richmond's firearm homicide trend and that for other large cities only after introducing the covariates in the conditional model. Raphael and Ludwig (2003) acknowledge the importance of controlling for other influences on firearm homicide when examining the impact of *Exile* (p. 270):

> The most important concern with our analysis is whether we are able to distinguish the effects of Project Exile from those of other unmeasured factors that drive crime trends over time at the local level. Our comparison of Richmond homicide trends to those of other cities is intended to address this concern. However, such comparisons may be invalid because of unobserved differences among cities in policing, age structure, and other factors likely to influence homicide rates.

Their comparison of juvenile and adult homicide arrest trends is intended to identify the effects of unmeasured factors on Richmond's trend in firearm homicide rates, on the assumption that juveniles are not subject to the threat of federal imprisonment for using a gun in crime, but are affected by other influences on firearm violence. But juveniles are not threatened by state imprisonment either, and we find a significant effect of state incarceration rates on firearm homicide trends (see Appendix II). Raphael and Ludwig's (2003) use of juveniles as a "control group" does not effectively eliminate the effect of incarceration and perhaps other influences on Richmond's trend in firearm homicide.

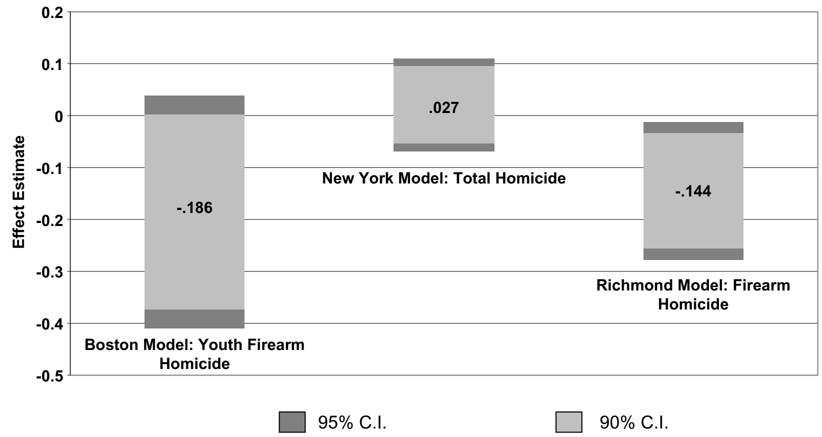
In summary, we find evidence consistent with an intervention effect on homicide trends for Richmond's *Project Exile*. Richmond's firearm homicide rate fell more rapidly than the average firearm homicide rate among large U.S. cities, with other influences controlled. We cannot rule out the possibility that unmeasured factors are responsible for Richmond's drop in firearm homicides after *Exile* was introduced in 1997. However, they apparently do not include changes over time in police size, drug involvement, or incarceration rates, all prime candidates for explaining the decline in big-city homicide rates during the 1990s (Rosenfeld, 2004). Nor do they include between-city differences in resource deprivation, population density, or unmeasured factors correlated with the initial (1997) level of firearm homicide. Furthermore, unmeasured factors that may be responsible for Richmond's homicide reduction must have come into play after and not before *Exile* was initiated, because Richmond's firearm homicide trend before 1997 did not deviate significantly from the sample average. These results, although not definitive, amount to a fairly strong circumstantial case for *Exile's* impact, and shift the burden of proof back to critics to identify factors not captured in our models that may explain Richmond's sizeable drop in firearm homicide after 1997.

CONFIDENCE IN THE RESULTS

We do not find evidence supportive of a program impact on homicide trends for Boston's *Operation Ceasefire* or New York's *Compstat*. In both instances, the homicide trends during the intervention did not diverge significantly from the sample average, although the Boston results are marginally significant when the age range of the target group is expanded to encompass 11–24 year-olds. Our methods may lack sufficient power to detect *Ceasefire's* effect on youth firearm homicide, although in the nature of the case, it is difficult to identify with high confidence program effects on low base-rate events. This point is illustrated nicely by comparing the confidence intervals around the point estimates of the divergence of the Boston, New York, and Richmond homicide trends from those of other large cities during the respective intervention periods.[14] As shown in Figure 2, despite the relatively large point estimate, we cannot confidently conclude that a nonzero difference exists between Boston's youth firearm homicide trend and those of other large cities. The Richmond results inspire somewhat greater confidence in the *existence* of a difference between Richmond's firearm homicide trend and the average trend for the sample, although the difference may have been quite small. We are more confident still in the results for New York, which are in line with earlier criticisms that factors other than *Compstat* were responsible for New York's homicide drop during the 1990s (Eck and Maguire, 2000; Karmen, 2000).

14. We thank Christopher Winship for this suggestion.

FIGURE 2. CONFIDENCE INTERVALS AROUND CITY FIXED EFFECTS IN CONDITIONAL MODELS



DISCUSSION

The decade of the 1990s was a period of marked innovation in policing practices in American cities. As new policies and procedures were introduced, crime rates began to fall across the country, which resulted in claims that the enforcement changes were responsible for the crime drop. Criminologists generally met such claims with skepticism, but they could not point to a substantial body of research of their own that refuted them. Their criticisms often came off as harping or nit-picking, which prompted some practitioners to boast that they would defeat not just crime but criminology. A reporter quipped in 1995 that New York's Police Commissioner William Bratton would not be satisfied "until every last criminologist surrenders" (quoted in Karmen, 2000:75).

Researchers failed to provide the public or policy makers with clearly articulated standards for judging the credibility of local officials' success claims. The standards are easy enough to identify; they are found in every textbook on social research methods. Before an intervention can be deemed effective in reducing crime, the observed reductions must be plausibly linked to the characteristics of the intervention. Second, the observed reductions must have occurred where and when the intervention was present and not where and when the intervention was absent. Third, the observed reductions must be shown to have exceeded the expected rate of crime decline, the change that would have taken place absent the intervention, what the textbooks term "history." These criteria constitute the minimum threshold, the prima facie case, for granting credence to claims that an intervention has reduced crime. Insisting that they are met is, or should be, the research community's first response to official claims-making and the basis on which the public is advised whether to take the claims seriously.

Criminologists also must impose these minimum methodological standards on themselves. It is not the research community's responsibility to make the prima facie case for a program's effectiveness; that is the burden of program proponents. The researcher's job is to subject the case to systematic empirical evaluation. What is remarkable about the criminological research community's response to the success stories told by local officials during the 1990s crime drop is how little research went into it. Even widely publicized innovations such as *Ceasefire*, *Compstat*, and *Exile* attracted no more than a handful of outcome evaluations. And the evaluations that were conducted are not easily compared with one another, because they employed different empirical methods and data.

These considerations prompted this investigation. Our intention has been to empirically assess claims made regarding reductions in homicide associated with *Ceasefire*, *Compstat*, and *Exile* and to offer a model for how such investigations might be undertaken in the future. We do not claim to have presented the last word on either the specific interventions or a general analytic strategy. On the contrary, given the paucity of existing research, our effort must be viewed as a point of departure for ongoing evaluation of the impact of local law enforcement interventions on crime rates.

Few commonly accepted standards exist for undertaking statistical evaluations of crime-control interventions using observational data and econometric methods, even though these are the data and methods we are stuck with for evaluating large-scale initiatives such as those addressed in this study. We propose that such evaluations meet the same requirements we would impose on program officials: The evaluation strategy must systematically compare observed crime reductions with those that would have occurred without the intervention. That in turn requires comparisons with other places and controls for other influences based on nationally representative samples, especially when evaluating widely known programs such as *Ceasefire*, *Compstat*, and *Exile*. Statistical models should be applied that efficiently estimate differences across places in crime trends before, during, and when possible after the intervention. We have chosen piecewise growth models and hierarchical methods for these purposes, but other approaches are defensible. What no longer can be defended are isolated evaluations of significant crime-control initiatives that cannot be compared with one another and do not even begin to rule out competing explanations.

This is no mere academic requirement. Current federal crime control policy in the form of *Project Safe Neighborhoods* (PSN) was inspired by the presumed success of Richmond's *Exile* and has been implemented in every federal judicial district in the country. PSN's goal is "to reduce the violent crime rate in our communities," and it promises "accountability" by "measuring success based on 'outcome' rather than 'output' " (http://www.projectsafeneighborhoods.gov). PSN's impact on violent crime remains to be seen. But it establishes a rationale and national platform for greater cooperation among program planners to ensure coordinated interventions and among researchers to devise common evaluation criteria. We should expect to see greater uniformity and generalizability in the resulting evaluations than have characterized the research on the great policing initiatives of the 1990s.

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APPENDIX I

HGLM is a multilevel modeling strategy available for growth-curve analysis. The HGLM for count data uses an overdispersed Poisson sampling model at level-1 and a log link function to equate the transformed count to a linear structural model. In this analysis, the model assumes an expected homicide count $E(Y_{it} | \lambda_{it}) = m_{it}\lambda_{it}$, where λ_{it} is the homicide rate of city i at time t and m_{it} is the exposure—the city population expressed in 100,000s. The expected homicide rate for a city is transformed through a natural logarithm, such that $\eta_{it} = \log(\lambda_{it})$. The log-event rate, η_{it}, becomes the dependent variable in level-1 of the model. The linear structural model thus becomes

$$\eta_{ti} = \beta_{0i} + \sum_{p=1}^{q} \beta_{pi}(T_{ti}) + \sum_{r=1}^{s} \beta_{ri}(X_{ti} - \bar{X}_{\bullet i})$$

in level-1. Here,

$$\sum_{p=1}^{q} \beta_{pi}\ (T_{ti})$$

is a series of individual linear growth estimates during the pre-intervention, intervention, and post-intervention periods, and

$$\sum_{r=1}^{s} \beta_{ri}(X_{ti} - \bar{X}_{\bullet i})$$

is a series of time varying covariate estimates that are group-centered to measure within-city change (cf. Horney et al., 1995).

When the outcome for an observation is modeled as a function of time, the indicators of temporal variation for that unit are included in the level-1 equation. Variables hypothesized to explain differences in trend parameters across units are then entered into the model at level-2. From the within-city change model at level-1, variations in the parameter estimates of the intercept and time components between cities are modeled at level-2. The level-2 intercept model is

$$\beta_{0i} = \pi_{00} + \sum_{p=1}^{q} \pi_{0p}(W_i) + \sum_{r=1}^{s} \pi_{0r}(\bar{X}_i) + \pi_{03}(D_i) + u_{0i},$$

where

$$\sum_{p=1}^{q} \pi_{0p}(W_i)$$

includes the effects of resource deprivation and ln(population density),

$$\sum_{r=1}^{s} \pi_{0r}(\bar{X}_i)$$

includes the within-city average for the time varying covariates, and $\pi_{03}(D_i)$ is the parameter estimate for a dummy variable representing the city of interest. Between-city differences in the level-1 time trends are modeled at level-2 as

$$\beta_{pi} = \pi_{p0} + \sum_{q=1}^{r} \pi_{pq}(W_i) + \pi_{p2}(D_i) + u_{1i},$$

where W_i and D_i remain as above. Finally, the between-city variation in parameter estimates for time varying covariates is estimated as a function of the grand mean effect: $\beta_{ri} = \pi_{r0} + u_{ri}$. Each level-2 model is specified with a city-level random effect.

APPENDIX II. HGLM RESULTS FOR BOSTON, NEW YORK, AND RICHMOND, 1992–2001 (N = 95)[15]

A. Boston **Fixed Effects**	**Unconditional**	**Conditional**
Initial (1992) Youth Firearm Homicide Rate, π_{0i}		
Base, β_{00}	1.435***	.339
	(.104)	(.782)
Boston, β_{01}	.212	–.146
	(.992)	(.632)
Resource Deprivation, β_{02}		.509***
		(.096)
ln Population Density, β_{03}		.015
		(.108)
Mean Police Size, β_{04}		.002***
		(.001)
Mean Incarceration Rates, β_{05}		.001**
		(.000)
Mean Cocaine Prevalence, β_{06}		.004
		(.012)
Pre-Intervention Change (1992–1995), π_{1i}		
Base, β_{10}	–.015	.188
	(.017)	(.148)
Boston, β_{11}	.006	–.008
	(.149)	(.161)
Resource Deprivation, β_{12}		–.003
		(.017)
ln Population Density, β_{13}		–.024
		(.018)
Intervention Change (1996-1999), π_{2i}		
Base, β_{20}	–.151***	–.173
	(.014)	(.135)
Boston, β_{21}	–.134	–.186
	(.123)	(.113)
Resource Deprivation, β_{22}		.007
		(.012)
ln Population Density, β_{23}		.006
		(.016)
Post-Intervention Change (2000–2001), π_{3i}		
Base, β_{30}	.012	–.043
	(.027)	(.269)
Boston, β_{31}	.372	.305
	(.238)	(.204)
Resource Deprivation, β_{32}		.059**
		(.025)
ln Population Density, β_{33}		.007

15. Conventional model fit statistics, such as the deviance statistic, are not available for Poisson count models in HGLM (Raudenbush et al., 2001:ch. 5.1). However, some sense of model fit can be gained by comparing the variance parameters from the unconditional and conditional specifications of the intercept model. By this measure, the models reported here would seem to fit the data reasonably well. For example, the residual variance component from the unconditional intercept model for New York is 0.6326; the residual variance from the conditional intercept model is 0.2634. The percentage reduction in error between the unconditional and conditional specifications is, therefore, (0.6326/(0.2634 – 0.6326)) * 100 = –58.4%. The corresponding reductions in error for Boston and Richmond are –57.7% and –53.4%, respectively.

		(.032)
Police Size, π_{4i}		–.001
		(.002)
Incarceration Rates, π_{5i}		–.001***
		(.000)
Cocaine Prevalence, π_{6i}		.000
		(.004)
Random Effects	**Variance Estimate**	**Variance Estimate**
Initial Homicide Rates, r_{0i}	.9511***	.4022***
Pre-Intervention Change, r_{1i}	.0163***	.0209
Intervention Change, r_{2i}	.0101***	.0128**
Post-Intervention Change, r_{3i}	.0315***	.0259
Police Size, r_{4i}		.0001
Incarceration Rates, r_{5i}		.0000*
Cocaine Prevalence, r_{6i}		.0002**
B. New York **Fixed Effects**	**Unconditional**	**Conditional**
Initial (1992) Homicide Rate, π_{0i}		
Base, β_{00}	2.697***	2.048***
	(.084)	(.598)
New York City, β_{01}	.612	–.521
	(.800)	(.493)
Resource Deprivation, β_{02}		.363***
		(.077)
ln Population Density, β_{03}		–.039
		(.082)
Mean Police Size, β_{04}		.003***
		(.001)
Mean Incarceration Rates, β_{05}		.0001
		(.000)
Mean Cocaine Prevalence, β_{06}		.004
		(.010)
Pre-Intervention Change (1992–1993), π_{1i}		
Base, β_{10}	.066***	.452**
	(.023)	(.200)
New York City, β_{11}	–.209	.046
	(.159)	(.089)
Resource Deprivation, β_{12}		.013
		(.018)
ln Population Density, β_{13}		–.042*
		(.024)
Intervention Change (1994–2001), π_{2i}		
Base, β_{20}	–.079***	.040
	(.006)	(.059)
New York City, β_{21}	–.090*	.027
	(.048)	(.041)
Resource Deprivation, β_{22}		.006
		(.005)
ln Population Density, β_{23}		–.013*
		(.007)
Police Size, π_{3i}		–.002
		(.001)
Incarceration Rates, π_{4i}		–.001**
		(.000)
Cocaine Prevalence, π_{5i}		–.005
		(.004)
Random Effects	**Variance Estimate**	**Variance Estimate**
Initial Homicide Rates, r_{0i}	.6326***	.2634***
Pre-Intervention Change, r_{1i}	.0241***	.0052***

Post-Intervention Change, r_{2i}	.0023***	.0046***
Police Size, r_{3i}		.0001***
Incarceration Rates, r_{4i}		.0000***
Cocaine Prevalence, r_{5i}		.0012***

C. Richmond **Fixed Effects**	**Unconditional**	**Conditional**
Initial (1992) Firearm Homicide Rate, π_{0i}		
Base, β_{00}	2.358***	1.658**
	(.093)	(.646)
Richmond, β_{01}	1.593*	.700
	(.891)	(.549)
Resource Deprivation, β_{02}		.420***
		(.075)
ln Population Density, β_{03}		–.039
		(.090)
Mean Police Size, β_{04}		.003***
		(.001)
Mean Incarceration Rates, β_{05}		.001**
		(.000)
Mean Cocaine Prevalence, β_{06}		–.002
		(.010)
Pre-Intervention Change (1992–1996), π_{1i}		
Base, β_{10}	–.064***	.175
	(.012)	(.133)
Richmond, β_{11}	.083	.113
	(.107)	(.111)
Resource Deprivation, β_{12}		–.003
		(.013)
ln Population Density, β_{13}		–.026
		(.016)
Intervention Change (1997–2001), π_{2i}		
Base, β_{20}	–.103***	–.108
	(.009)	(.098)
Richmond, β_{21}	–.069	–.144**
	(.075)	(.067)
Resource Deprivation, β_{22}		.025***
		(.009)
ln Population Density, β_{23}		.003
		(.012)
Police Size, π_{3i}		–.004**
		(.002)
Incarceration Rates, π_{4i}		–.001*
		(.0006)
Cocaine Prevalence, π_{5i}		–.004
		(.004)

Random Effects	**Variance Estimate**	**Variance Estimate**
Initial Homicide Rates, r_{0i}	.7806***	.3637***
Pre-Intervention Change, r_{1i}	.0106***	.0219***
Post-Intervention Change, r_{2i}	.0049***	.0041***
Police Size, r_{3i}		.0001***
Incarceration Rates, r_{4i}		.0000***
Cocaine Prevalence, r_{5i}		.0005***

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.