J Quant Criminol (2010) 26:165–190
DOI 10.1007/s10940-010-9091-9

ORIGINAL PAPER

Project Safe Neighborhoods and Violent Crime Trends in US Cities: Assessing Violent Crime Impact

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Published online: 29 January 2010

Abstract Since the mid-1990s, a number of initiatives intended to address gang, gun and drug-related violence have arisen and demonstrated promise in reducing levels of violent crime. These initiatives have employed some combination of focused deterrence and problem-solving processes. These strategies formed the basis for Project Safe Neighborhoods (PSN), a national program implemented by the Department of Justice and coordinated by US Attorneys' Offices. This paper is an initial attempt to assess the potential impact of the nationally implemented PSN initiative through an analysis of violent crime trends in all US cities with a population of 100,000 or above. While a number of site specific studies exist examining the potential impact of locally implemented PSN programs, to date no national-level study has examined whether PSN may have had an impact on violent crime trends. Cities included in the current study are distinguished on the basis of whether they were considered a treatment city by the PSN task force and by the level of implementation dosage of the PSN program. This allowed a comparison of 82 treatment cities and 170 non-treatment cities as well as a variable of dosage level. Hierarchical Generalized Linear Models (HGLM) were developed that controlled for other factors that may have affected the level of violent crime across the sample of cities. The results suggested that PSN treatment cities in higher dosage contexts experienced statistically significant, though modest, declines in violent crime whereas non-target cities and low dosage contexts experienced no significant changes in violent crime during the same period. The limitations of this initial analysis are noted but the evidence seems to suggest that the multi-agency, focused deterrence, problem solving approach holds promise for reducing levels of violent crime. At a minimum, these findings call for continued programmatic experimentation with data-driven, highly focused, deterrence-based violence reduction strategies.

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Keywords Violent crime · Deterrence · Hierarchical generalized linear models (HGLM) · Project safe neighborhoods

Although the US experiences lower levels of certain types of crime than many other western democracies, rates of gun related homicides, robberies, and assaults are much higher in the US (Farrington et al. 2004; Langan and Farrington 1998; Zimring and Hawkins 1999). Homicide is the second leading cause of death for US citizens ages 14–30 and the leading cause of death for African-American males ages 14–30 (Center for Disease Control 2008a). The costs associated with firearms violence are staggering with estimates ranging from $63 to $158 billion per year (Miller and Cohen 1995; Center for Disease Control 2008b) and $1 million per gunshot injury (Cook and Ludwig 2000). The emotional and economic toll incurred by families of victims and offenders, neighborhood residents, police and criminal justice actors, and communities are similarly substantial.

The good news is that the country has experienced an unexpected and substantial decline in homicide and gun violence since the early 1990s (Bureau of Justice Statistics 2008). In 1991, the US homicide rate was 9.8 per 100,000 population. By 2000 it had declined to 5.5 per 100,000 and has had a relatively stable range rate between 5.5 to 5.7 per 100,000 between 2000 and 2007 (Uniform Crime Reports 2008). This represents a drop from over 24,700 homicides in 1991 to just less than 17,000 in 2007. The total violent crime victimization rate also fell from 51.2 per 1,000 in 1994 to 21.0 per 1,000 in 2005 (Bureau of Justice Statistics 2009). Similarly, non-fatal firearms victimization declined from 6.0 per 1,000 in 1994 to 2.0 per 1,000 in 2005. This translated into impact in the nation's trauma centers as the number of gunshot wounds treated in emergency rooms declined from over 64,000 in 1993 to under 40,000 in 1997 (Bureau of Justice Statistics 2008).

The question of why the nation experienced such a significant decline in homicide and violent crime has been the subject of significant scholarly debate (Blumstein and Wallman 2000; Blumstein 2006; Symposium 1998). A variety of factors including improved economic conditions, increased rates of incarceration, the decline of the crack cocaine epidemic, and improved policing have been suggested as potential causes, yet there is no consensus within the academic community.

The suggestion that improved policing practices may have been a factor is based on a series of studies that emerged in the last two decades that found some police interventions were related to significant reductions in some types of crime (Weisburd and Braga 2006). These studies were striking because they seemed to contradict widely held beliefs that crime was driven by a variety of macro-level historical, cultural, economic, and social structural factors and was largely impervious to planned police intervention (e.g., Bayley 1994).

In 2001 the US Department of Justice developed a program to address gun related violence that attempted to build on some of these promising police practices that had emerged during the 1990s. The program, known as Project Safe Neighborhoods (PSN), represents a major initiative that has received an estimated $1.5 billion in federal funding (Project Safe Neighborhoods 2008). This paper represents an initial attempt to assess the question of whether PSN had any demonstrable impact on violent crime across US cities. The paper first reviews the studies that served as a foundation for PSN. From this basis, an analysis of trends in violent crime in all US cities with a population of 100,000 or above is used to provide this initial assessment of the impact of PSN on violent crime.

Foundations of Project Safe Neighborhoods

Although there are likely many programs and factors (e.g., weed and seed, community policing, community prosecution) that influenced Department of Justice (DOJ) officials as they constructed the anti-gun crime initiative that came to be known as PSN, several policing interventions with varying degrees of empirical research support appeared to be influential. These included Project Exile that was developed by the US Attorney's Office in Richmond, Virginia. Indeed, the US Attorney from Richmond had moved to the Attorney General's Office at DOJ and was one of the principal architects of PSN. Similarly, the Boston Gun Project, which was then extended through the Department of Justice's Strategic Approaches to Community Safety Initiative (SACSI), appeared influential in shaping PSN.[1]

As will be discussed, these programs had varying components but had a common theme of trying to focus police and criminal justice resources on some combination of those individuals, social networks, locations, and contexts thought to be driving the violent crime problem. This is consistent with, and several of these studies influenced, the findings of a National Research Council report that found police strategies that are most focused on specific problems have the most impact on crime (National Research Council 2004; Piquero 2005).

Project Exile

In the late 1990s, then Managing Assistant US Attorney James Comey (Eastern District of Virginia) decided that the long experienced high levels of homicide and aggravated assault were unacceptable and that the full force of federal prosecution would be brought to bear against prior felons possessing or using firearms. Along with increasing federal prosecutions for gun crimes, the US Attorney's Office worked with a coalition of local law enforcement, local government, and businesses to launch a high profile media campaign to communicate a message that the illegal possession or illegal use of a gun would result in severe federal sanctions. Following the implementation of Exile, homicide levels in Richmond declined significantly from their peak levels (Rosenfeld et al. 2005).

Boston Gun Project

Boston's Gun Project, also referred to as Operation Ceasefire, was another major strategic problem solving initiative, though here the specific intent was to reduce the high level of youth gun violence in the city driven by a small number of chronic offenders involved in networks of known offenders. Ceasefire was initiated by a multi-agency working group involving the US Attorney's Office, local prosecutors, the Boston Police Department, probation, youth service workers, and a team of researchers from Harvard's Kennedy School of Government. The strategy that emerged was a deterrence-based model, referred to as "pulling levers," whereby the threat of federal prosecution was directly communicated to these groups of known offenders. Youth violence declined dramatically, as indeed, Boston went two and one-half years without a youth homicide and youth gun violence declined by approximately 60% (Braga et al. 2001a, b; Kennedy et al. 2001).[2]

[1] An additional set of deterrence-based interventions were the series of directed police patrol studies in Kansas City, Indianapolis, and Pittsburgh (Sherman and Rogan 1995; Sherman et al. 1995; McGarrell et al. 2001; Cohen and Ludwig 2003).

[2] Rosenfeld et al. (2005) used growth curve models and controlled for factors that affect city-level violent crime rates and report less robust impact of the Boston gun project, though it should be noted that Berk

Strategic Approaches to Community Safety Initiative (SACSI)

The apparent success of the Boston Gun Project resulted in DOJ funding a 10-city crime reduction initiative based on the strategic problem solving approach implemented in Boston. One of the cities that most closely implemented and evaluated the SACSI intervention was Indianapolis. Systematic analysis of gun violence patterns indicated that both homicide victims and suspects were typically young men with extensive involvement in the justice system, in concentrated geographic locations, and often involved in gangs or neighborhood crews or cliques. Officials implemented a very similar strategy: crackdowns on known violent groups, direct communication of a deterrence message, and social support intervention. Like the situation in Boston, Indianapolis experienced a significant reduction in homicide and gun crime with a particular impact on gang homicide (McGarrell et al. 2006; Corsaro and McGarrell 2009). Other SACSI sites also experienced crime reductions (Roehl et al. 2008), and additional cities later implemented pulling levers strategies that were evaluated from quasi-experimental research designs demonstrating impact on gun-related crime (see Braga 2006; Braga et al. 2006; Papachristos et al. 2007; Tita et al. 2003).

Common and Distinctive Themes

The Project Exile, Boston Gun Project, and SACSI interventions shared several features and had some distinctive qualities. After some degree of problem analysis, all moved from responding to crime generally to having a very specific and proactive focus on gun crime. The Boston and Indianapolis projects included careful and systematic analysis of gun crime patterns as well as a combination of police crime data analyses and systematic incident reviews of gun crimes (see Klofas et al. 2006). It is not clear how much analysis was conducted prior to implementation of Project Exile but the project did operate on the premise that many gun crime victims and offenders have extensive prior criminal histories and often prior felony convictions.

Project Exile and the Boston and Indianapolis and related SACSI projects involved communication of deterrence messages through media campaigns including billboards, posters on buses, and posters in the jail. The Boston and Indianapolis projects took this a step further through direct communication with groups of offenders who were called into face-to-face meetings with criminal justice officials, service providers, and neighborhood leaders. These two programs attempted to complement the deterrence message with social support and linkage to opportunities. Finally, Project Exile was driven by the US Attorney's Office and the Boston and Indianapolis projects involved multi-agency collaborations, research partners, and the leadership of the US Attorney's Office.

Project Safe Neighborhoods

As noted, PSN built on these foundations. US Attorneys' Offices were asked to create task forces involving local, state and federal law enforcement, local prosecutors, probation and parole, and often including local government, service providers, neighborhood leaders, and

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Footnote 2 continued
(2005) raised a number of methodological limitations with the Rosenfeld et al. critique. In addition, Wellford et al. (2005) found the evaluation of and the reduction in youth homicide in the Boston project compelling despite some of their own methodological concerns.

the faith community. Deterrence and incapacitation was emphasized through the threat of federal prosecution for illegal gun possession and violent, gang, and drug related offenses involving a firearm. Federal funding was provided for development of a communication strategy.

Additional funding was provided for a research partner who was to analyze the local gun crime problem so that the task force could tailor the national strategy to the local context. The research partner's role also included providing ongoing assessment of impact as well as a longer-term evaluation. Supplemental funds were provided for training as well as for funding local anti-gun crime prevention and enforcement interventions. As will be described subsequently, these programmatic components were utilized to develop measures of PSN treatment and dosage utilized in the analyses conducted herein.

Analytic Framework

Aggregate-level studies of policy interventions tend to suffer from a number of methodological flaws (Berk 2005; Bushway and McDowall 2006). As Berk (2005) and others have noted, the lack of an experimental design subjects most of these studies to threats of internal and external validity. This was clearly the case for assessing the impact of PSN, a national program reflecting a "full-coverage" program. The issue arises, to what do we compare shifts in violent crime rates?

Rosenfeld et al.'s (2005) examination of the intervention effects of Boston's Operation Ceasefire (Braga et al. 2001a, b; Kennedy et al. 2001; Piehl et al. 2003), New York's Compstat program (Kelling and Sousa 2001; Fagan et al. 1998; Harcourt 2001; Moore and Braga 2003), and Richmond's Project Exile (Raphael and Ludwig 2003) provided an extremely useful analytical model for studying PSN impact. Rosenfeld and colleagues compared the relative decline in the target cities to the mean change in homicide rates in the largest 95 US cities. Additionally, they argued that an intervention effect should be observed while controlling for other influences of homicide as evidenced in prior research (see Land et al. 1990; Levitt 2002; Marvell and Moody 1997; Spelman 2000). Subsequently, Berk (2005) asserted that the Rosenfeld et al. analysis was hampered by a number of limitations, which (among other methodological considerations) could be addressed and improved by including estimates of "treatment effects" (Berk 2005: 460–461). Consequently, we chose an approach that borrowed from the Rosenfeld et al. analytic strategy (i.e., using similar hierarchical generalized regression models and including comparable theoretically important covariates of violent crime) while also attempting to minimize some of the concerns raised by Berk by including appropriate measures of PSN treatment and dosage to better model estimates of potential PSN treatment effects.

The strategy employed in the current study was designed to examine violent crime trends from 2000 to 2006 in all cities with a population of 100,000 or larger. A series of comparisons were then constructed to first compare PSN "treatment" cities with "non-treatment" cities, and then to examine the potential changes in violent crime related to cities' level of PSN dosage, while controlling for theoretically relevant measures that have been shown to influence violent crime rates. At the basic level, the logic behind this evaluation strategy was to examine the following hypothesis: if PSN had an impact on violent crime, greater declines should be apparent when comparing violent crime trends between treatment cities and non-treatment cities and also by comparing higher dosage cities with lower dosage cities.

Data

The data used in the subsequent analyses were culled from multiple sources. We relied on data from the Federal Bureau of Investigation (FBI) Uniform Crime Reports (UCR) that captured Type I offenses for the years 2000–2006 to create violent crime outcome measures for each city. Data from the 2000 US Census and 2000 Bureau of Labor Statistics were used to create static structural indicators measuring the demographic and population profiles for each city. Data were obtained from FBI UCR Police Employee records for the period 2000–2006 to create an annual city-level police density measure.[3] Data were also collected from Bureau of Justice Statistics (BJS) to create a yearly measure of state incarceration rates. Finally in order to obtain measures of PSN implementation, including indicators of treatment and dosage, we used data collected by a team of researchers at the Michigan State University (MSU) School of Criminal Justice. MSU researchers partnered with the Department of Justice and were responsible for bi-annual data collection detailing the process and outcome measures that were reported directly from the PSN project coordinators and research partners across the country, which was a stipulation of the PSN program. Taken collectively, these data sources were used to create the measures in the subsequent statistical models.

Dependent Variable

In order to conduct an initial national assessment of PSN and its potential impact on crime, we chose to examine a dependent variable of aggregated violent crime offenses that met the following criteria: (1) was comprised of crimes that were most *likely* to involve consistent and stable firearm usage, as indicated in national violent crime statistics, (2) included homicide, but also additional offenses that case studies indicated were a major focus of site specific PSN initiatives, and (3) included offense data that were consistently and readily available for all PSN intervention sites as a well as a large number of comparison sites that were not a direct focus of PSN implementation.

These criteria were guided by the following considerations. First, in terms of offenses that were likely to involve firearm related incidents, UCR data indicate that homicides, robberies, and aggravated assaults had very stable ranges in terms of overall percentage of offenses that involved the use of a firearm during this past decade. More specifically, between 2000 and 2007 offenses that were firearm related ranged from 67.9 to 71.1% for homicides, 41.1 to 42.8% for robberies, and 18.3 to 21.9% for aggravated assaults.[4] Second, a number of PSN case studies showed potential program impact across a variety of offenses (McGarrell et al. 2009). As an illustration, in St. Louis (MO) homicides, aggravated assaults, and robberies all experienced somewhat similar reductions across the entire city (Decker et al. 2007). However, particularly in smaller cities with lower base rates of homicides such as Lowell (MA) and Montgomery (AL), the major emphasis of their programs and substantive impact was seen in the reduction of aggravated gun assaults

[3] Rosenfeld et al. (2005) relied on the Law Enforcement Management and Administration Statistics (LEMAS) surveys for their police density measure. However, their study had a significant lag between their period of interest and their analyses, and thus LEMAS data were available for the period of time they examined (i.e., the 1990's). Complete LEMAS data through 2006 were not available at the time of our study and we substituted with the use of UCR employee data.

[4] These offense percentages were obtained from the Uniform Crime Reports (FBI 2000–2007).

(McDevitt et al. 2007; McGarrell et al. 2007).[5] Ultimately, these PSN case studies indicated the need to examine an aggregated violent crime outcome measure that captured the variety of outcomes targeted across multiple districts. Finally, firearm-related offenses are not available as part of the Uniform Crime Reporting System, beyond the National Incident-Based Reporting System (NIBRS) data, which has extremely limited coverage (<20% of law enforcement agencies across the country report using NIBRS).[6]

Consequently, we contend that a composite violent crime count variable comprised as an aggregate measure of murders, robberies, and aggravated assaults between 2000 and 2006 for all US cities that had a population of 100,000 or greater was the most robust outcome to model in terms of satisfying these requirements.[7] Given the limitations of including violent offenses that do not involve firearms, however, the following analysis is supplemented by additional models that exclude aggravated assaults given the much higher proportion of non-firearm related incidents for this specific offense type.[8]

Within-city regressions were used to impute values for missing violent crime data prior to aggregation since the outcome measure is a composite variable.[9] However, where missing data existed for two or more within-city offenses, we simply left the data as missing since the statistical strategy we employ is flexible in handling missing data (Raudenbush and Bryk 2002: 199–200). Missing data were an issue in less than 1.6% of the homicide counts over the 7 year period.[10] A similar strategy was employed with 1.9% of robbery counts and 1.8% of assault counts.[11] In all, this strategy yielded complete violent crime measures for 98.5% of the violent crime cases (1,739 of 1,764 cases) and complete violent crime data for 95.6% of cities (241/252 cities), noting that seven cities accounted for most of the missing data (22/25 cases).[12]

[5] Unfortunately national UCR reports do not distinguish aggravated assaults with a gun from total aggravated assaults and thus were not available for the present study.

[6] In future analyses we plan to use Supplemental Homicide Reports (SHR) in regression models to focus more extensively on gun homicides. The limitation of the SHR data in this preliminary assessment is that many of the cities have small numbers of homicides and thus the population of cities becomes further restricted due to data power issues. As noted subsequently we do provide a basic analysis of SHR firearms homicides as a supplemental analysis.

[7] The selection of cities with a population of 100,000 or greater was based on maximizing the sample size while also providing sufficient base rates of violent offenses to support the analyses.

[8] Results are available upon request.

[9] We were concerned with cases where at least one of the measures (homicide, robbery, or assault) had missing data, but the other offenses had complete data. If aggregation occurred under this circumstance there would be a bias in the measure. Thus, we imputed missing data values prior to aggregation.

[10] Missing homicide data were an issue in 29/1,764 cases, 1.6%. In 16 of these cases, we were able to supplement the missing annual homicide count with the Supplementary Homicides Reports (SHR) data, given that both data sources were initially housed by the FBI reporting system and are created from incident information. For 10 of the remaining 16 cases, we used within-city regressions to impute a missing value for missing homicide data. In the remaining 6 cases, we simply left the homicide count as missing due to the ‘multiple-missing’ data issue.

[11] Missing robbery data were an issue in 34/1,764 cases, or 1.9% of the cases. Missing assault data were an issue in 33/1,764 cases, or 1.8%.

[12] None of the ‘chronic missing data’ cities (Westminster, Co; Olathe, KS; Overland Park, KS; Warren, MI; Akron, Oh; Alexandria, VA; and Chesapeake, VA) were designated as PSN treatment sites.

Independent Variables

Several independent variables were utilized in the subsequent regression models. We employed two structural measures that were treated as static (i.e., time invariant) characteristics of each city, population density and concentrated disadvantage, which are established macro-level correlates of homicide specifically (Land et al. 1990; Messner and Rosenfeld 1998; Rosenfeld et al. 2005) as well as violence and crime in general (Blau and Blau 1982; Chamlin and Cochran 1997; Kane 2006; LaFree 1999; Liska and Bellair 1995; Miethe et al. 1991; Messner and Golden 1991; Sampson and Raudenbush 1999). Population density was operationalized as the number of people per square mile and was logged in order to reduce skewness. Concentrated disadvantage was a composite variable obtained from a principal components factor analysis that included the following highly intercorrelated measures: percent of families with children under 18 headed by a female, percent of persons below poverty, median family income, male unemployment rate (i.e. males 16 years old and older who are unemployed), and percent African American. The factor loadings for this measure (i.e. concentrated disadvantage) were all moderately strong (>.60) and 72.7% of the inter-correlation between these items was captured in this measure. Thus, the concentrated disadvantage measure used here is comparable to disadvantage measures that have been used in similar research (Krivo and Peterson 1996; Land et al. 1990; Reisig and Parks 2004; Rosenfeld et al. 2005; Sampson and Raudenbush 1999).

In terms of dynamic or time variant independent variables, prior research indicates that trends in incarceration and police density are related to homicide (Marvell and Moody 1997; Spelman 2000) and violent crime (Kuziemko and Levitt 2004). Thus, we incorporated the annual state incarceration rate into our analyses for each city included in our research methodology. Similarly, an indicator of police per 100,000 residents was used, which was measured as an annual city-level measure created from the UCR Police Employee data (i.e., the number of law enforcement officers in each city per year/population). We also included a series of annual dummy variables designed to control for random variability in violent crime trends in each year.

PSN Indicators: Treatment and Dosage

As noted above, the most significant challenge for the evaluation of PSN stemmed from the fact that at one level it is a full coverage program. The threat of federal prosecution for illegal gun possession and use was theoretically available in every community of the US and the media component of PSN was a national campaign.[13] When the entire US receives the treatment, clear evaluation effects are difficult to obtain. To overcome this challenge, two strategies were employed. First, PSN task forces identified target jurisdictions that were the focus of the task force's efforts. We expected that if PSN had an impact, it should be observed in target sites in comparison to non-target sites. Second, we constructed measures of PSN dosage. As explained earlier, PSN was not implemented evenly across all 93 PSN task forces. We hypothesized that if PSN had an impact on violent crime, it should result in greater reductions in high dosage jurisdictions in comparison to low dosage jurisdictions.

[13] The coverage of the media campaign is impossible to measure in a fashion that would allow measurement of variation across jurisdictions. It included a national campaign that involved television and radio public service announcements (PSAs) and each district included its own campaign that also included PSAs as well as billboards, posters, and other creative mediums.

The base measure of PSN treatment was incorporated into the statistical models as a static measure (0 = non-PSN treatment city, 1 = PSN treatment city). The operationalization of the PSN treatment indicator is similar to 'level' of implementation (Berk 2005: 452; see also Papachristos et al. 2007). In terms of delineating between PSN treatment cities and non-PSN treatment cities, we relied on a systematic approach drawing from district level data and district reports that the MSU PSN research team received from 2000 to 2006. In all, 68 US federal districts had large cities (i.e., population over 100,000) that were the focus of a PSN intervention strategy. In addition, 20.5% of these districts had multiple large cities that were the focus of a PSN intervention, which led to an N of 82 treatment cities and 170 non-treatment cities to provide comparison estimates.[14] Only those cities that were specifically designated as a PSN treatment city either by the district project coordinator, research partner, or both were considered treatment cities in this evaluation.

When an entire county was the specific focus of PSN implementation, any city with a population over 100,000 that was housed within the county was designated a PSN treatment city. This was an issue in four (5.8%) of the districts. When documentation indicated the entire federal district was the focus of the intervention, we contacted the site coordinator for clarification regarding specific target cities in the district. Finally, given the *district nature* of the intervention and the fact that all included cities were part of a federal district, we have confidence that non-treatment cities were indeed absent a focus of PSN strategies due to the fact that district coordinators and researchers were in a position to clarify this issue. We must note that it was impossible to rule out a 'trickle down' or adoption effect that may have occurred in non-treatment cities. In order to control for this issue, we also relied on a more precise measure of PSN 'dosage' (Berk 2005: 452).

Dosage is an overall composite variable designed to capture the specific policy adoption of the outlined PSN strategy. PSN dosage was an aggregate measure comprised from standardized scores measuring the following three specific policy elements: (1) *collaborative implementation*, (2) *research integration into strategic planning*, and (3) *enhanced federal prosecution* (for a more detailed review, see Zimmermann 2006). Data for this item were culled from the formal semi-annual reports from the United States Attorneys' Offices (USAO) between 2000 and 2005, as well as a research partner survey and additional data submitted to the MSU PSN national research team. A more itemized breakdown, display, and description of the specific indicators and reliability measures of these three distinct components can be found in Appendix A.

Collaborative implementation was designed to measure the extent to which the US Attorney's Office worked with other law enforcement, criminal justice, and community groups and developed intervention strategies that worked across agencies. The measure focused on a reported task force emphasis on enhanced local and state prosecution, enhanced federal prosecution, law enforcement implementation including directed patrol (McGarrell et al. 2001) or street-level enforcement (Braga and Pierce 2005), parole and probation integration through the notification meeting or offender home visits (Braga et al. 2001a, b; McGarrell et al. 2005), community programs (i.e., reported more than the modal value), supply-side intervention (Koper 2005; Ludwig 2005), and gang focus (Braga 2008; Braga et al. 2006). This component of PSN dosage was constructed from the formal reports submitted by the local USAO where the United States Attorney was responsible for indicating which, if any, of seven major strategies were implemented as part of PSN. These

[14] Fourteen of 68 districts included in the analyses had multiple large cities that were the focus of PSN intervention.

seven strategies were: enhanced federal prosecution, enhanced state and local prosecution, law enforcement (either directed patrol or street-enforcement), parole and probation integration, community programs (i.e., districts that scored above the average response for active engagement of community members), supply-side intervention strategies, and gang/criminal organization interventions. The scores from these specific items comprised an index variable and were assigned an ordinal value that fell into three categories from 1 (low collaborative implementation, bottom one-third of districts on this measure), 2 (medium collaborative implementation, middle one-third of districts on this measure), or 3 (high collaborative implementation, top one-third of districts on this measure).

The second component of PSN dosage was designed to measure variation in the *level of research integration in task force analysis and planning*. Research integration measured whether PSN created an environment where data analysis drove decision making as well as the quality of the data submitted to the MSU PSN team. The measures were obtained from three sources: A survey item with the PSN coordinator (US Attorney), and also a survey item included in the PSN research partner survey (both taking place in 2005) were designed to capture the degree to which data were translated into decision-making, the degree to which data were used to inform policy and strategies, the helpfulness of data used as part of PSN, and the degree of the overall data and research usefulness in the strategy. Finally, the MSU PSN research team assessed the annual data submitted as part of PSN requirements, which fell into one of five categories from none, very poor, poor, good, or very good data. Again, an average over these 6 years was created for each district. These specific items were included into a composite standardized z-score and were assigned an ordinal value that fell into three categories from 1 (low research integration, bottom one-third of districts on this measure), 2 (medium research integration, middle one-third of districts on this measure), or 3 (high research integration, top one-third of districts on this measure).

The third component of PSN dosage was a measure of the level and trend in *federal prosecution* for gun crime charges. Specifically, a factor score measured changes in federal prosecution (Papachristos et al. 2007), which relied on both numeric changes as well as changes in the per capita prosecution rate. The numeric changes and change in the per capita prosecution rate for the districts were reduced into an overall weighted factor score, via principal components analysis, that explained 82.9% of the variance in this item. Similar to the creation of the two prior PSN dosage indicators, districts were assigned an ordinal value into one of three categories from 1 (low federal prosecution, bottom one-third of districts on the factor score), 2 (medium federal prosecution, middle one-third of districts on the factor score), or 3 (high federal prosecution, top one-third of districts on the factor score).

Adding the three ordinal scores together created a PSN dosage variable that had a range from low (3) to high (9) for PSN treatment cities while non-treatment cities received a zero for this measure. The three constructs were combined into a single dosage indicator because, although PSN was not a unitary policy and flexibility was encouraged given the diverse nature of district-level problems, DOJ explicitly promoted the use of multi-agency partnerships, strategic planning, training, media outreach, and program accountability, coupled with increased federal prosecution for gun crime, as central to successful PSN program implementation (Office of Justice Programs 2009).[15] Thus, successful

[15] Zimmermann (2006) notes that additional elements framed by DOJ including media outreach strategies and formal training exhibited extremely low variability across districts and were considered constants and were thus dropped in the aggregation of the overall policy adoption, or dosage variable.

implementation defined by DOJ required a combination of these strategies rather than focusing more heavily on one of the three items. In summary, the examination of dosage was based on collaborative implementation of multiple strategies, research integration, and prosecution. Dosage thus reflected key ingredients of the type of model developed in Boston, applied in Indianapolis and SACSI cities, and reflected in PSN case studies conducted in the Middle District of North Carolina, Lowell, Omaha, St. Louis, and in the initiatives in Chicago (Papachristos et al. 2007) and Stockton (Braga 2008).[16]

Although PSN officially was launched in 2001, interviews with PSN officials and review of various data indicated that PSN was not implemented at the local level until 2002. Thus, for purposes of the evaluation, 2002 was considered the treatment date.[17] In terms of operationalization, dosage was treated as a time-variant measure. Non-treatment cities received a zero for the dosage indicator between 2000 and 2006 since there was no indication of implementation of this three-stage approach in the control cities. Intervention sites also received a zero for dosage in years 2000 and 2001 because we chose 2002 as the beginning of PSN implementation. In 2002 through 2006 PSN target sites received a fixed score that reflected the amount of PSN dosage (ranging from 3 to 9) that was implemented in each unique target city.[18]

Analytic Strategy

To assess the relationship between PSN implementation and potential changes in violent crime, we applied growth curve regression models to violent crime trends using data from all US cities that had a population greater than 100,000. In total, we examined changes in violent crime trends in 252 of the largest US cities. We relied upon Hierarchical Generalized Linear Models (HGLM) to assess within- and between- city changes in violent crime between 2000 and 2006, using a Poisson sampling model with a correction for over-dispersion, and the city population as the exposure variable. In this case, the annual violent crime counts were treated as repeated measures nested within cities at level 1. Incorporating the exposure variable allowed interpretation of the left-hand side of the level 1 equation as the log violent crime rate per 100,000 population (Browning et al. 2006; Raudenbush and Bryk 2002). The inclusion of the population as the exposure variable was based on the notion that the expected violent crime count of a city (i) in a given year (t) is contingent upon both the criminal propensity of offenders in a given city as well as the number of people living in that city. More specifically, larger cities have a greater opportunity to house more offenders and subsequently will have more offenses. In this case lambda (λ) is interpreted as a violent crime rate outcome.

The data contain multiple observations for the same cities over time, meaning each of the observations are not statistically independent. Thus, we estimated random effects

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16 These case studies provided seven "tests" of PSN impact. In all seven cases violent gun crime declined. In two of these sites, the decline was either not statistically significant (Durham, NC) or was observed in comparison sites as well (St. Louis). See McGarrell et al. 2009.

17 The reality is that for many districts, it was not until 2003 or later that the task force was truly operational and various enforcement, intervention, and prevention components were actually implemented. Thus, the reliance of a common 2002 treatment date results in a conservative test of PSN's impact as it may discount impact observed in late adopter jurisdictions. The 2002 date is justifiable based on federal prosecution trends. This makes sense in that it is a strategy under the control of the US Attorney's Office and thus was often the earliest indicator of PSN implementation.

18 The limitation of this approach is discussed and addressed throughout the results and discussion sections.

estimates to capture all the unobserved and stable city-specific characteristics that generate differences between cities in violent crime and also a random error term at different observation occasions (see Brame et al. 1999; Rosenfeld et al. 2005; Horney et al. 1995; Xie and McDowall 2008). In addition, all level 1 measures were group-centered in order to create a unique intercept and slope estimate for each city in the analysis, while each level 2 measure was grand mean centered in order to provide unique between city estimates. Group centering a time-varying covariate (X) at level 1 provides an unbiased estimate of the effect of change between the independent variable X and violent crime *within* a city as the outcome, which is the focus of the current study. Our analyses relied on the use of HLM computer software (version 6.02a; Raudenbush et al. 2004) throughout.

Time-varying (i.e., dynamic) explanatory variables including changes in city level police density, the state prison population, and dosage effect of PSN were included in the conditional level 1 HGLM model. In addition, a series of dummy variables were included at level 1 in order to control for the annual random within-city changes in violent crime, using the year 2000 as the reference category. We included two theoretically relevant and static social indicators at level 2: concentrated disadvantage and population density. We used a two-level model that predicts *within*-city trajectories in violent crime at level 1 and *between*-city violent crime variation at level 2 using the predicted level 1 intercepts and slopes as outcomes (Hox 2002). Ultimately, we used this multi-level approach to assess whether there was an observed relationship between PSN implementation (i.e., both PSN dosage (dynamic) and PSN treatment (static) in different models) and violent crime, controlling for theoretically relevant indicators within- and between cities.[19]

Results—Treatment and Multiple Component Dosage

Prior to conducting the HGLM analyses, basic trends were reviewed in the treatment and non-treatment cities as well as across cities with varying levels of PSN dosage. The comparison of treatment and non-treatment cities revealed that treatment cities experienced a 4.1% reduction in violent crime comparing 2002–2006 to 2000–2001. This equated to roughly 45 fewer violent crimes per 100,000 residents between pre- and post- national PSN implementation. Comparatively, non-treatment cities experienced a 0.9% decline, which equated to roughly five fewer violent crimes per 100,000 population over this period. Similarly, comparisons by level of dosage indicated that medium and high dosage cities experienced declines in violent crime whereas non-treatment and low dosage cities had experienced increases in violent crime by the end of the study period (2006).

Fig. 1 displays the average violent crime trends for both treatment and non-treatment cities by disaggregating treatment sites into three categories related to PSN dosage: Low, Medium, and High. Low dosage cities ($N = 14$) were those treatment sites that were consistently low on measures related to research integration, collaborative implementation, and enhanced federal prosecution. Interestingly, low PSN dosage sites and non-treatment sites ($N = 170$) experienced very similar violent crime trends between 2000 and 2006. Medium dosage cities ($N = 33$) experienced a far more gradual and sustained decline in

[19] We did not include PSN treatment as a level 2 measure and PSN dosage as a level 1 measure within the *same* model due to the high inter-relationship between these two measures. Specifically, dosage only increased in PSN target cities. However, when examining a model that included both treatment and dosage at different levels, the results were virtually identical to those presented here-in (i.e., the dosage-violent crime relationship remained the same at level 1 with or without the treatment estimate at level 2).

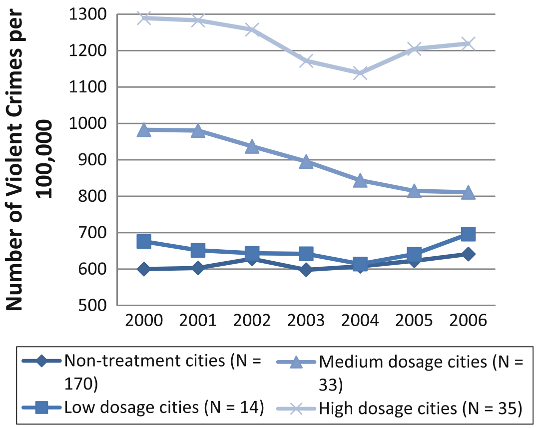

Fig. 1 Violent crime trends in US cities by dosage designation

violent crime after 2001 than all other city distinctions. Medium dosage sites tended to be moderate to very strong in at least two of the three measures that comprise the PSN dosage measure. High dosage cities ($N = 35$) scored very high in all of the indicators related to PSN dosage, and they also tended to be cities that had a much higher base-line violent crime rate prior to PSN implementation. Also interestingly, high dosage sites had a more abrupt reduction between 2002 and 2003, but also experienced a marginal rebound in violent crime around 2005. Given this preliminary suggestive evidence, we proceeded to conduct the HGLM analyses that provided controls for other factors likely to influence city-level crime trends.

HGLM Growth Curve Models—Treatment and Multiple Component Dosage

Table 1 provides the descriptive statistics for the variables included in the conditional growth curve models that utilized all the independent variables available here. One note is that 11 of the 252 cities were excluded from analysis due to incomplete and inadequate data. None of these eleven cities were PSN-target cities.[20] In terms of the univariate distribution of the variables, the average violent crime count was 2,712 with a standard deviation of 6,098 violent crimes, which means the outcome measure was highly skewed (s^2 violent crime $> \bar{X}$ violent crime) requiring an additional model parameter to correct for over-dispersion (Raudenbush and Bryk 2002: 334).

The results of the HGLM growth curve estimates were used to determine whether PSN treatment cities experienced a change in violent crime rates, net of other relevant correlates related to violent crime. The unconditional random effects baseline model indicated significant variation and reduction in violent crime trends within cities in the current study ($\pi_{0i} = -5.17$, $P < .01$). In addition, the χ^2 statistic accompanying the estimated variance component in the unconditional model indicated significant variation in violent crime among cities (s^2 component $= .432$, df $= 247$, $\chi^2 = 46{,}715$, $P < .01$).

[20] A series of Independent Samples T-Tests comparing measures from the eleven cities excluded from the mixed-regression models were performed. None of the tests were statistically significant ($P > .05$) where measures existed (including violent crime rates in a given year, level 1 covariates in a given year, and level 2 covariates that were treated as time-invariant), indicating that the non-treatment cities excluded from the analyses were not significantly different than non-treatment cities that were included in the regressions presented here-in.

Table 1 Descriptive statistics

Variable	Mean	SD	Minimum	Maximum
Level 1 measures ($N = 1660$)				
Violent crime count	2712.4	6098.1	72	74115
Dosage	1.5	2.75	0	9
Prison rate	472.9	145.4	126	872
Police density	273.5	111.8	108	927
Year 2000	.14	.35	0	1
Year 2001	.14	.35	0	1
Year 2002	.14	.35	0	1
Year 2003	.14	.35	0	1
Year 2004	.14	.35	0	1
Year 2005	.14	.35	0	1
Year 2006	.14	.35	0	1
Level 2 measures ($N = 241$)				
Disadvantage[a]	.04	.99	−2.58	2.74
(Ln) Population density	3.52	.31	2.18	4.42
Treatment Site	.34	.47	0	1

[a] We re-estimated our models adding a constant (3.0) to the disadvantage measure to eliminate the negative values and the estimates did not change in any meaningful way

Table 2 summarizes the results of the conditional model that includes all level 1 and level 2 measures included in the current study (see Appendix B for more specific details and equations). Consistent with prior research, the strongest indicator of violent crime rates across cities was concentrated disadvantage. Cities with more concentrated disadvantage experienced significantly more violent crime. Every unit increase in the standardized measure of concentrated disadvantage was accompanied by a 66% increase in violent crime across cities. Similarly, population density had a significant relationship with violent crime across cities but in a negative direction. In terms of within city estimates, state prison incarceration rates had a marginally significant ($P < .10$) and positive relationship with a violent crime increase over time, meaning cities that experienced a rise in violent crime were also very likely to have a rise in state prison incarceration rates.[21] Most important to the present examination, PSN dosage exerted a negative and statistically significant relationship ($P < .05$) with changing violent crime rates, net of dynamic and static indicators.

Consistent across analyses, cities that experienced an increase in PSN dosage were significantly more likely to experience a significant decline in violent crime rates. For every unit increase in the multiple component PSN dosage measure, within-city violent crime rates declined by roughly 5.7%. Although included as statistical controls, the dummy variables designed to capture annual random variability in violent crime also revealed a pattern that was consistent with the line graphs seen in Fig. 1. The 'peak' decline year for

[21] We felt it necessary to address the concern that PSN implementation could have led to an increase in law enforcement agents at the city level and an increase in incarceration rates at the state level. In this case, increases in police density and incarceration could actually constitute an indirect reflection of PSN implementation. To address this concern, we estimated a growth curve model where annual police density (the outcome at level 1) was a function of a city being designated a PSN site at level 2 (0 = non-treatment site, 1 = treatment site). The estimated effect was actually negative and statistically significant, indicating that PSN sites actually had a larger *decline* in police per 100,000 residents than did non-PSN sites. The same was true when state incarceration changes were modeled as the outcome variable at level 1. Thus, we find contradictory evidence to the concern that PSN sites actually led to a significant increase in state incarceration rates as well as increases in law enforcement density These relationships were actually negative.

Table 2 Conditional random effects poisson model of violent crime rates in large US cities between 2000 and 2006 (examining PSN dosage)

Fixed effects	Coefficient	Standard error	Odds ratio
Level 1 (within-city effects)			
Intercept	−5.176	.0296	.006
PSN dosage	−.0592*	.0301	.943
Incarceration rate	.0005^{+}	.0003	1.001
Police density	.0002	.0003	1.000
Year 2001	−.0180^{+}	.0094	.982
Year 2002	.0195	.0129	1.019
Year 2003	−.0225^{+}	.0132	.977
Year 2004	−.0449**	.0135	.956
Year 2005	−.0261^{+}	.0140	.978
Year 2006	−.0125	.0145	.987
Level 2 (between-city effects)			
Disadvantage	.5030**	.0316	1.65
(Ln) Population density	−.2918**	.0999	.747

Random effects	Variance component	χ^2	*P* value
Intercept, r_{0i}	.2066	27078.1	<.01
PSN dosage, r_{1i}	.0002	173.1	<.01
Incarceration rate, r_{2i}	.0000	235.3	<.01
Police density, r_{3i}	.0000	196.8	<.01
Level 1 error, e_{ti}	22.95	–	–

** $P < .01$, * $P < .05$, $^{+}$ $P < .10$

violent crime trends was 2004 both in terms of coefficient size and statistical significance, while in 2005 the coefficient became both empirically smaller and reduced from statistical significance ($P < .05$) to marginal significance ($P < .10$), while in 2006 it became statistically insignificant ($P = .338$). Thus, net of other factors, there appeared to be a ‘rebound’ effect in violent crime after 2004, net of the PSN dosage reduction. Implications of these findings are discussed in the summary section.

In terms of model improvement from the unconditional model that did not include any covariates to the conditional model, there are no conventional model fit statistics, such as the deviance statistic, for Poisson count models in HGLM (Raudenbush et al. 2004: ch. 5.1). However, model improvement can be compared by examining the change in the residual variance component between the unconditional model and conditional model (Raudenbush and Bryk 2002: 309; Rosenfeld et al. 2005: 447). In the unconditional model, the residual variance was .4320, while in the conditional model the residual variance was .2066. Thus, the percentage reduction in the error variance between the unconditional model and the conditional mixed-model was 52.1%,[22] suggesting a substantial improvement in model fit.

Cross-level interaction effects (i.e., slopes as outcomes) capturing the relationship between structural indicators and PSN dosage yielded statistically significant and negative interaction estimates, though in both models the *direct* effects of PSN dosage were no

[22] The reduction of 52.1% of the residual variance from the unconditional to the conditional model was computed as a percentage change: (.2066−.4320)/.4320 = −.521 or −52.1%.

longer statistically significant.[23] Most importantly, the PSN dosage effect retained statistical significance in alternative cross-level interaction models indicating a robust relationship between dosage and violent crime reduction. We also substituted the dichotomous measure designating treatment cities from non-treatment cities (0 = non-PSN treatment city, 1 = PSN treatment city) in place of the PSN dosage measure at level 1 following the same pre/post intervention guidelines used in the dosage measure. Those results were very consistent with the estimates presented in Table 2 in terms of direction and statistical significance.

Alternative Model

One of the greatest threats to the validity of the above findings indicating a relationship between increased PSN dosage and a reduction in violent crime is the internal measurement validity of the PSN dosage measure used in the previous models. More specifically, in terms of the operationalization of the PSN dosage measure used at level 1, the analysis is based on the assumption that the three-phase component to PSN (i.e., collaborative implementation, research into strategic planning, and enhanced federal prosecution) started in 2002 and maintained its dosage intensity over time in all treatment cities through 2006. In order to include treatment as a level 1 measure, the research team had to choose the best overall discrete time point to designate as the national PSN start date. While some districts were able to provide specific start and end dates for their specific PSN implementation that differed from the current time measure, we did not want to selectively choose to use such information until a more detailed analytic approach could be used for all districts.

Currently, there are no systematic measures available for a majority of PSN treatment cities concerning their *specific* start and end dates, and how dosage varied over the period examined here. We acknowledge this is a somewhat limited approach with a very challengeable assumption. In order to address this issue, an additional growth curve model is presented here that does not require a designated start and end date by treating PSN treatment as a static factor at level 2, though this approach is not without its own set of limitations in the current context. This analytic approach was adopted from a similar and very rigorous PSN research evaluation that also relied on growth curve models to assess the intervention effect in Chicago (Papachristos et al. 2007). Similar to their strategy, we substituted a single measure of time (0–7 for the number of years of data available here) in place of all annual dummy variables used in the prior mixed-model.

Table 3 displays the results of the growth curve model where PSN treatment (0 = non-PSN treatment city, 1 = PSN treatment city) was designated as a static measure at level 2.[24] The results were very similar to the prior model displayed above. Specifically across cities, PSN treatment sites had significantly more violent crime (roughly 37%) than non-treatment sites. Similarly, there was significantly more violent crime in cities where disadvantage was higher and lower violent crime in cities that had higher population density. In terms of the cross-level interaction effect where the slope of time was estimated as a function of PSN treatment, there was a significant decline in violent crime ($P < .01$) in PSN treatment cities compared to non-treatment cities, net of within- and between city

[23] Results available upon request.

[24] Refer to Appendix C for the equation for this model.

Table 3 Conditional random effects poisson model of violent crime rates in large US cities between 2000 and 2006 (examining PSN treatment*time)

Fixed effects	Coefficient	Standard error	Odds ratio
Level 1 (within-city effects)			
Intercept	−5.17**	.0283	.0056
Incarceration rate	.0008*	.0003	1.000
Police density	.0002	.0003	1.000
Time	−.0045*	.0021	.9954
Level 2 (between-city effects)			
Disadvantage	.4538**	.0320	1.574
(Ln) Population density	−.1959*	.0969	.8220
PSN treatment city	.3133**	.0626	1.367
Cross-level interaction effects			
PSN treatment city*time	−.0252**	.0038	.9731

Random effects	Variance component	χ^2	P value
Intercept, r_{0i}	.1878	31858.4	<.01
Incarceration rate, r_{1i}	.0000	633.9	<.01
Police density, r_{2i}	.0000	616.1	<.01
Level 1 error, e_{ti}	25.16	–	–

** $P < .01$, * $P < .05$

correlates of violent crime. Thus, as time passed (i.e. increased), PSN treatment cities experienced a modest 2.4% reduction in violent crime compared to non-treatment cities.[25]

While the model presented in Table 3 did not require a designated start and end date, which was a requirement when including PSN dosage as a level 1 time variant measure, it assumes PSN treatment was static between 2000 and 2006 in all treatment cities.[26] Finally, in order to examine the apparent quadratic function (i.e., the fall and rise) seen in violent crime rates in treatment cites, both time ($t1$) and time squared (t^2) variables were included in another model (not displayed), and both estimates were statistically significant ($P < .01$) and in opposite direction (negative and positive respectively). Thus, there was a quadratic change in violent crime rates over time within cities.[27] The cross-level interaction estimates between level 2 treatment with both time and time squared at level 1 yielded statistically significant results ($P < .05$), again in the opposite direction for each interaction effect. Importantly, the odds of the event rate for the decline (.949) were greater than the rebound (1.003) in violent crime which means that the 'fall' in violent crime was greater than the 'rise' and thus there was a greater reduction than rebound as time increased.

25 [exp(−.0252)] = −.024 or −2.4%.

26 This was a minimal concern in the research done in Chicago by Papachristos et al. (2007) because they included additional relevant time-variant measures such as prosecution changes and sentences associated with federal prosecution in their linear growth models. Thus, they included both static and dynamic PSN treatment measures in one overall model.

27 Results available upon request.

Supplementary Trend Analysis: Firearm Related Homicide Incidents[28]

As noted above, the selection of the violent crime dependent variable (i.e., the total number of homicides, robberies, and aggravated assaults) for this initial PSN impact assessment is limited. Specifically, given that PSN was intended to focus on gun crime, the preferred outcome measure would be the number of firearm-related violent crime offenses in the target or non-target jurisdictions. The only source of gun-related crime across all of these cities is the Supplementary Homicide Reports (SHR). The limitations of an exclusive focus on SHR data in this assessment are that: (1) many of the cities included as either treatment or control sites had relatively low homicide incidents and thus the population of cities becomes further restricted with consequent data power issues,[29] and (2) many of the target sites focused their efforts on additional forms of firearm related incidents given the infrequency of homicides.[30]

Despite these limitations, it was important to cross-validate the current study's findings by including a general bivariate trend analysis of SHR annual homicides that were firearm related for high dosage, medium dosage, low dosage, and non-target sites. Due to data limitations posed by the SHR, some of the sites could not be included. Specifically, five of the original 82 treatment sites were excluded due to their lack of reporting SHR data over this period. In addition, only those non-treatment cities with adequate base rates of firearms homicides were included ($n = 38$). Many of the original 170 non-treatment areas had very few homicides; thus, for comparison purposes, only those non-treatment cities in the highest quartile of homicides were included in the SHR analysis.

The SHR analysis indicated that high dosage sites experienced the greatest reduction in firearm related homicides (−10.5%) between pre-intervention (2000–2001) and post-intervention (2002–2006). Comparatively, medium dosage sites experienced no appreciable change over this same period (i.e., a less than .1% change), while low dosage and non-treatment sites experienced firearm homicide count increases of 14.0 and 11.1%. Thus, although not identical this basic trend analysis of SHR firearm related homicides is consistent with the earlier findings relying on UCR violent crime incidents and supports the basic finding that high dosage PSN sites experienced the greatest reduction in targeted offenses.

Analytic Issues and Study Limitations

We conducted a number of diagnostic tests on the above statistical models (i.e., Q–Q plots of residuals where no significant outliers were identified, a comparison of Empirical Bayes and Least Squares residuals that conformed to the assumption of a normal distribution, and

[28] The above models were also analyzed excluding aggravated assaults from the dependent variable (i.e., based on homicides and robberies). The results were consistent with those presented above and are available upon request.

[29] Twenty-five percent of all US cities with a population of 100,000 or more averaged 3.85 homicides per year, or .32 homicides per month. Fifty percent averaged 11.28 homicides per year, or .94 per month. Seventy-five percent of all large US cities averaged 30 homicides per year, or 2.5 homicides per month.

[30] It should also be noted that as part of the requirements for PSN funding, many districts submitted firearm related offense data to the MSU PSN research team. However, a focused preliminary analysis of these data would have two major flaws: First, only those districts that reported data would be included in the analysis, which would have a tautological relationship with our PSN dosage measure since one of the components of dosage was the use and quality of district crime data. Second, we would not be able to compare target site offense data to non-target sites.

zero-order correlations of independent variables where the highest co-variance was .320) and we are confident that none of the estimates presented here were heavily influenced by extreme scores. Nor was there any evidence that the inferential statistics in this paper were the result of biased standard errors. This does not mean that the current study is not without its own set of methodological and theoretical limitations. We acknowledge a number of these limitations below which we believe should serve as a guide for future research attempting to examine the impact of the nationally implemented PSN program with violent and gun-related crime.

First, the limitation of an ‘off/on’ PSN dosage effect requiring a designated beginning and end point has already been discussed. The fact that the PSN treatment estimates were robust across the different models lends support that the observed co-variation between treatment and violent crime reduction occurred at the aggregate level. A more specific analytic approach should attempt to discern unique (rather than general) start and end dates, and rely on the use of interrupted time series models (see Bushway and McDowall 2006) to assess changes in violent crime *within* PSN target cities, particularly once unique and specific onset and duration measures are captured.

Second, the methodology employed here also assumes equal treatment *within* PSN target cities. Many of the PSN task forces focused on intensive hot spot areas within cities, rather than a broad focus across entire cities. For example, Papachristos et al. (2007) compared PSN treatment neighborhoods against non-treatment areas within Chicago. This would suggest the Chicago-based intervention was not a city-wide approach but was more specific to discrete neighborhoods within the city. It is certainly reasonable to suspect that a number of PSN treatment cities used a similar strategy, which is not captured in the current analytic approach. A more detailed methodology should attempt to discern overall target city approaches from a more discrete neighborhood focus.

Third, PSN was enacted in all federal districts, though only 68 US districts had cities with a population over 100,000 that received PSN treatment. This equates to coverage of roughly 75% of all US districts, which conversely means roughly one quarter of all districts were excluded from the current methodology. One of the issues in a large number of the remaining districts is that many of these sites relied on a more general and rural focus across a large range of locations rather than specific cities. For example, some rural sites focused on a state reduction in domestic violence. Given that these rural areas were likely to have a series of unique strategies over vast areas, a subsequent approach that utilizes county-level crime trend data (e.g., Lee 2008) will be required to perform a systematic evaluation of these sites.

Fourth, PSN was an initiative designed to reduce violence, and in particular gun violence and gun related crime (Wellford et al. 2005). The outcome used in the regression analyses presented here relied upon UCR Type I violent crimes including robbery, aggravated assault, and homicide.[31] The supplementary analysis of SHR firearms homicides represented an extension of the analysis to a more refined focus on gun crime. Future systematic firearm related offense analysis is vital to a more thorough understanding of the utility of PSN strategies and will guide future research in this area.

Finally, there is a limitation that is obvious in the results presented here and in PSN implementation as a whole. PSN treatment sites and correspondingly higher dosage sites were incorporated in cities that had the largest violent crime rates. Ultimately, PSN implementation was selectively targeted and calibrated with areas in most need of an intervention, and thus were places most likely to experience some variation in crime over the period of assessment. A plausible rival explanation for the observed trends is that some

[31] As noted previously, analyses based on SHR reports are being conducted in subsequent stages.

of the high violent crime cities experienced a regression to the mean. This type of selection bias often plagues criminal justice and more generally social science research when trying to assess program impact, which is particularly true given the nation-wide scope of this intervention. However, we propose that the use of a standardized violent crime rate outcome measure comprised of offenses that were consistently firearm related over this past decade, the subsequent reliance on a vast number of comparison sites, specific and detailed measures of level and dosage concerning PSN implementation, and the use of control measures of structural social processes that have been established as correlates of changes in violent crime in large US cities is a very good first step in evaluating the national PSN program. Additionally, the early intervention years came at a time of a decade drop in violent crime, thus reducing the likelihood of a regression to the mean (Rennison 2002). The results seen here indicate that PSN intervention was correlated with a significant reduction in violent crime, when and where implemented compared to when and where it was not implemented. The degree of confidence that PSN was a causal mechanism behind the reduction in violent and gun-related crime will only be more (or less) certain when the limitations mentioned above become the focus of future research .

Summary

Prior studies of homicide and violent crime trends in specific cities that have implemented strategies similar to those employed in the high and medium PSN implementation cities have suggested the potential for focused, data driven strategies (Piquero 2005). These include studies of directed police patrol focused on illegal gun carrying (Sherman and Rogan 1995; McGarrell et al. 2001; Cohen and Ludwig 2003), "pulling levers" (Braga et al. 2001a, b; Kennedy et al. 2001; McGarrell et al. 2006; Corsaro and McGarrell 2009; Papachristos et al. 2007; Braga et al. 2006; Braga 2008; McDevitt et al. 2007; Wilson et al. 2010) and Project Exile (Rosenfeld et al. 2006; McGarrell et al. 2007; Hipple et al. 2008; but also see Raphael and Ludwig 2003). The current findings, though suggesting a modest impact of PSN, should be considered in light of this line of prior research. Additionally, the current analysis represents a conservative test of PSN impact due to the limitations noted above. Specifically, the global as opposed to site-specific intervention date, the reliance on violent crime indicators as opposed to violent gun crime, and the city-wide trend in violent crime as opposed to specific target areas within cities,[32] made it less likely that the present analysis would detect impact. Given the significant human, social, and economic costs associated with violent crime in America (Cook and Ludwig 2000), the results of this research suggest the need for continued programmatic experimentation and associated research to further move toward evidence-based practice (Piquero 2005).

In addition to improved research designs that could better address the limitations described above, research is also needed to better identify the components of these interventions that are most associated with violent crime reduction. Is it the focused deterrence associated with federal prosecution of illegal gun possession and use? Is it the direct communication of enhanced likelihood of prosecution to at-risk populations? Is it the data-driven processes that better focus limited law enforcement resources? Is it the

[32] For example, Papachristos et al. (2007) reported a homicide decline of approximately 39% for the specific police districts where PSN was implemented in Chicago. This accounted for much of the city's decline in homicide. Our measure of the decline in violent crime in Chicago incorporates the target-specific decline but is a more modest decline than that observed in the PSN target areas.

combination of strategies? Similarly, the analysis does not allow assessment of variation of adherence to the various components. Approaching these complex issues in a systematic manner will provide an opportunity to better advance our understanding of the diverse nature of PSN program implementation and adherence among the various sites across the nation, including an analysis of specific program organizations, characteristics, and contexts (see Chen 1990). These specific questions, which we assert should also be linked to more site-specific firearm related offense outcomes, serve as a framework to guide future national PSN evaluation efforts, as well as similar multi-component crime reduction initiatives.

Finally, important research questions arise about the long-term impact of these strategies. Are the violent crime reductions short-term or lasting? Indeed, the rebound observed among the high implementation target cities suggests the possibility that the impact is short-term. Is the suppression effect of these strategies inherently short-term? Can these coalitions of law enforcement and community partners be sustained and survive turn-over in leadership positions? Are there unintended effects? Given the present findings, the studies cited above suggesting an impact on violent crime, but also the significant limitations of this study and the body of related research, the importance of continued experimentation and enhanced research designs is apparent.

Acknowledgment This project was supported by Grant No. 2002-GP-CX-1003 awarded by the National Institute of Justice, Office of Justice Programs, US Department of Justice. Points of view in this article are those of the authors and do not necessarily represent the official position or policies of the US Department of Justice.

Appendix A

Table 4 Detailed description of PSN dosage components

Measure[a] ($N = 82$)	Indicator	Coding	Source	Time collected
Collaborative implementation[b]				
Identify enforcement/deterrence/community focused strategies that your PSN task force has implemented:				
Federal prosecution of firearms offenses	No/yes	0–1	USAO survey	FY 2005
Local/state prosecution of firearm offenses	No/yes	0–1	USAO survey	FY 2005
Deployment of specialized firearms unit	Directed patrol *or* street-level enforcement	0–1	USAO survey	FY 2005
Parole/probation enforcement	Offender notification meetings *or* offender home visits	0–1	USAO survey	FY 2005
Community programs: Task force implemented any of the following programs:	Clergy outreach, substance abuse, education, vocation, school-based, *or*, other programs	0–1	USAO survey	FY 2005
Supply-side intervention	No/yes	0–1	USAO survey	FY 2005
Gang investigations and interventions	No/yes	0–1	USAO survey	FY 2005

Table 4 Appendix continued

Measure[a] ($N = 82$)	Indicator	Coding	Source	Time collected
Cronbach's alpha (collaborative implementation) = .659	Item total	0–7		
Research Integration[b]				
Has data analysis helped the PSN task force focus on gun violence?	Not really, somewhat, very much	0–2	USAO survey	FY 2005
In what areas has research provided a tangible result?	Problem identification, program development, evaluation, program revision, resource allocation	0–5	Research partner Survey	2005
To what extent are you (the research partner) integrated into the task force	Research is peripheral, routine but not active involvement, integral part of project and active participation	0–2	Research partner survey	2005
Was the research team able to conduct analysis of the local gun crime problem	No/yes	0–1	Research partner survey	2005
Data submission (completeness and quality) to MSU: none (0), very poor (0), poor (0), good (1), very good (1)	Annual/monthly firearm related violent crime measures submitted = good/very good	0–1	MSU PSN research team coding	2000–2004
Cronbach's alpha (research integration) = .628	Item total	0–11		
Federal prosecution[b]				
Number of federal cases for gun violence (post intervention minus pre intervention)	Percentage change	Principal components extraction = .70		2000–2004
Per capita federal prosecution rate change (post intervention minus pre intervention)	Percentage change	Principal components extraction = .66		2000–2004
Item range (z-score: −1.7–4.4)	Item total	Eigenfactor = 1.65		

[a] These items were collected by Zimmermann (2006)

[b] Each district's item total for the three elements of PSN (collaborative implementation, research integration, and enhanced federal prosecution) was transformed into a standardized z-score. The Cronbach's alpha cumulative reliability for the final overall dosage measure was .598. Given this modest between-measure reliability, we then created an ordinal scale for each item (bottom 1/3, middle 1/3, top 1/3 of districts) and summed them for each district's cumulative dosage score, which had a final range from 3 (lowest on all items) to 9 (highest on all items). The main reason we combined these ordinal rankings rather than relying on a combined factor score was to down-weight the potential influence of adherence/ignorance of one specific program policy (e.g., it was not uncommon for some districts to significantly increase prosecutions but have little adherence to collaboration implementation or research integration). An evenly weighted factor score among these three items would allow extreme adherence/ignorance of *one policy type* to bias an *overall* dosage score

Appendix B

The HGLM model presented in Table 2 can be written as follows (Note: All level 1 measures were group-centered, all level 2 measures were grand-mean centered, and no random variance components were estimated for the annual dummy variables because all cities had the same fixed value for this measure):

$$\eta_{ti} = \pi_{0i} + \pi_{1i}(\text{PSN Dosage}) + \pi_{2i}(\text{Prison Rate}) + \pi_{3i}(\text{Police Density}) + \pi_{4i}(2001) \\ + \pi_{5i}(2002) + \pi_{6i}(2003) + \pi_{7i}(2004) + \pi_{8i}(2005) + \pi_{9i}(2006) + e_{ti}$$

where η_{ti} = the expected violent crime rate, $\pi_{0i} = \beta_{00} + \beta_{01}(\text{Disadvantage}) + \beta_{02}(\text{Pop-Density}) + r_{oi}$, $\pi_{1i} = \beta_{10} + r_{1i}$, $\pi_{2i} = \beta_{20} + r_{2i}$, $\pi_{3i} = \beta_{30} + r_{3i}$, e $\pi_{4i} + \cdots + \pi_{9i} = \beta_{40} + \cdots + \beta_{90}$. Thus, the reduced two-level equation can be written as:

$$\eta_{ti} = [\pi_{0i} + \pi_{1i} + \pi_{2i} + \pi_{3i} + \pi_{4i} + \ldots + \pi_{9i}] + [r_{oi} + r_{1i} + r_{2i} + r_{3i} + e_{ti}]$$

The outcome in the HGLM is the count of violent crimes at level 1 and includes time-varying covariates within the cities (i.e., level 1 measures) and time invariant measures at level 2. As seen in the above equations, PSN dosage is included as a time-varying measure at level 1. Here, the HGLM model uses an overdispersed Poisson sampling model at level 1 and a log link function to equate the transformed count into a linear structural model. The log link function in the HGLM is used to equate the transformed count into a linear structural model, consistent with the regression-based analytic approach.

In the analysis, the HGLM Poisson model assumes an expected violent crime count

$$E(Y_{it}|\lambda) = m_{it}\lambda_{it},$$

where λ_{it} is expressed as the violent crime rate of a city i at time t and m_{it} is the exposure measure, which is expressed as the city population in 100,000 s. The expected violent crime rate for a city is transformed through a natural logarithmic function, where $\eta_{ti} = \ln(\lambda_{it})$. The logged event rate, η_{ti}, becomes the dependent variable in the level 1 model.

Appendix C

The HGLM model presented in Table 3, which was the alternative growth curve model where the PSN treatment city designation is a static measure at level 2, is written as follows (Note: all level 1 measures were group-mean centered, and level 2 measures were grand-mean centered):

$$\eta_{ti} = \pi_{0i} + \pi_{1i}(\text{Prison Rate}) + \pi_{2i}(\text{Police Density}) + \pi_{3i}(\text{Time}) + e_{ti}$$

where $\pi_{0i} = \beta_{00} + \beta_{01}(\text{PSN Treatment City}) + \beta_{02}(\text{Disadvantage}) + \beta_{03}(\text{PopDensity}) + r_{oi}$, $\pi_{1i} = \beta_{10} + r_{1i}$, $\pi_{2i} = \beta_{20} + r_{2i}$, $\pi_{3i} = \beta_{30} + \beta_{31}(\text{PSN Treatment City})$.

Thus, the reduced two-level equation can be written as:

$$\eta_{ti} = [\pi_{0i} + \pi_{1i} + \pi_{2i} + \pi_{3i}] + [r_{oi} + r_{1i} + r_{2i} + e_{ti}]$$

The outcome in the HGLM is the count of violent crimes at level 1 and includes time-varying covariates within the cities (i.e., level 1 measures) and time invariant measures at level 2. As seen in the above equations, PSN treatment is a level 2 static measure. Here, the HGLM model uses an overdispersed Poisson sampling model at level 1 and a log link function to equate the transformed count into a linear structural model. The log link function in the HGLM is used to equate the transformed count into a linear structural model, consistent with the regression-based analytic approach.

In the analysis, the HGLM Poisson model assumes an expected violent crime count

$$E(Y_{it}|\lambda) = m_{it}\lambda_{it},$$

where λ_{it} is expressed as the violent crime rate of a city i at time t and m_{it} is the exposure measure, which is expressed as the city population in 100,000 s. The expected violent crime rate for a city is transformed through a natural logarithmic function, where $\eta_{ti} = \ln(\lambda_{it})$. The logged event rate, η_{ti}, becomes the dependent variable in the level 1 model.

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