

CRIMINOLOGY

PROJECT SAFE NEIGHBORHOODS IN CHICAGO: LOOKING BACK A DECADE LATER

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Project Safe Neighborhoods (PSN) is a federally funded initiative that brings together federal, state, and local law enforcement to reduce gun violence in urban centers. In Chicago, PSN implemented supply-side gun policing tactics, enhanced federal prosecution of gun crimes, and notification forums warning offenders of PSN's heightened criminal sanctions. Prior evaluations provide evidence that PSN initiatives have reduced crime in the first few years of their operation. But over a decade after the program was established, we still know little about whether these effects are sustained over an extended period of time. This Article examines PSN Chicago, an anti-violence program in operation since 2002. Consistent with a previous evaluation, we find that several program components were associated with reductions in violence in the initial target

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areas. These associations, however, are concentrated in the first few years of the intervention. We also examine the effect of PSN in several subsequent expansion areas and find no detectable effects. We suggest that the effects of PSN were diluted as the program expanded to larger areas of the city without an increase in funding or resources. Still, we recommend that future research consider PSN's strategies in Chicago that appeared effective in the early years and leverage those insights for future programs.

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INTRODUCTION

Project Safe Neighborhoods (PSN) is a federally funded initiative that brings together federal, state, and local law enforcement with researchers and community organizations to devise context-specific strategies for reducing gun violence. Since 2001, Congress has allocated over a billion dollars to the U.S. Attorney's Office to oversee PSN programs in the 94 federal districts.¹ Each jurisdiction crafted a set of interventions that typically included increased federal prosecution of gun crimes.²

Prior case studies³ and systematic empirical investigations⁴ of PSN have generally been positive. Most recently, a national evaluation examined

¹ See EDMUND F. MCGARRELL ET AL., PROJECT SAFE NEIGHBORHOODS—A NATIONAL PROGRAM TO REDUCE GUN CRIME: FINAL PROJECT REPORT 1 (2009) [hereinafter FINAL PROJECT REPORT].

² *Id.* at iii.

³ See generally EDMUND F. MCGARRELL ET AL., PROJECT SAFE NEIGHBORHOODS: STRATEGIC INTERVENTIONS, MIDDLE DISTRICT OF ALABAMA: CASE STUDY 5, at 2 (2007), https://www.bja.gov/Publications/MD_Alabama.pdf (finding a decrease in some crime categories, including homicide and gun assault, after the program began); JACK MCDEVITT ET AL., PROJECT SAFE NEIGHBORHOODS: STRATEGIC INTERVENTIONS, LOWELL, DISTRICT OF MASSACHUSETTS: CASE STUDY 6, at 2 (2007), https://www.bja.gov/Publications/Lowell_

the association between PSN dosage and crime in 82 PSN sites and 170 control sites and found that high-dosage PSN sites experienced declines in violent crime relative to control sites, while low-dosage sites experienced no change.⁵

The current study examines the long-term effects of PSN in Chicago, where the program has operated for over a decade. Given the size of the city's population and its high rate of gun violence, the PSN taskforce initially focused on two of the highest crime police districts. The taskforce devised three primary strategies. First, it established a case review process to make strategic decisions about the prosecution of all gun-related cases. Suitable cases are diverted to federal court where more severe penalties are often available.⁶ Second, the taskforce created a multi-agency "gun team" to investigate gun trafficking cases and trace all recovered firearms.⁷ Third, drawing on the experiences of other focused-deterrence initiatives in cities like Boston and Richmond,⁸ the PSN team in Chicago developed offender notification forums in which law enforcement officers and social service providers meet with parolees and probationers convicted of gun-related crimes. Forum attendees are cautioned about the consequences of

MA.pdf (observing a reduction in some crime categories, including aggravated assaults with a firearm, after the program began); *see also* NATALIE KROOVAND HIPPLE ET AL., PROJECT SAFE NEIGHBORHOODS CASE STUDY REPORT: DISTRICT OF NEBRASKA (CASE STUDY 9) 23 (2007), <https://www.ncjrs.gov/pdffiles1/nij/grants/241727.pdf> (observing a reduction in firearm offenses after the program began).

⁴ *See generally* Edmund F. McGarrell et al., *Project Safe Neighborhoods and Violent Crime Trends in US Cities: Assessing Violent Crime Impact*, 26 J. QUANTITATIVE CRIMINOLOGY 165 (2010) [hereinafter *Assessing Violent Crime Impact*]; FINAL PROJECT REPORT, *supra* note 1; Andrew V. Papachristos, Tracey L. Meares & Jeffrey Fagan, *Attention Felons: Evaluating Project Safe Neighborhoods in Chicago*, 4 J. EMPIRICAL LEGAL STUD. 223 (2007) (evaluating the effect of PSN Chicago in the first several years of its operation). *But see* J.C. Barnes et al., *A Partial Assessment of South Carolina's Project Safe Neighborhoods Strategy: Evidence from a Sample of Supervised Offenders*, 38 J. CRIM. JUSTICE 383 (2010) (observing an increase in firearm-related crimes after South Carolina's PSN program began).

⁵ *Assessing Violent Crime Impact*, *supra* note 4, at 173, 176–77.

⁶ Papachristos et al., *supra* note 4, at 233.

⁷ *Id.* at 232–33.

⁸ *See, e.g.*, Anthony A. Braga et al., *Problem-Oriented Policing, Deterrence, and Youth Violence: An Evaluation of Boston's Operation Ceasefire*, 38 J. RES. CRIME & DELINQ. 195 (2001) [hereinafter *Problem-Oriented Policing*] (examining Boston's Operation Ceasefire over a two-year period); Steven Raphael & Jens Ludwig, *Prison Sentence Enhancements: The Case of Project Exile*, in EVALUATING GUN POLICY: EFFECTS ON CRIME AND VIOLENCE 251 (Jens Ludwig & Philip J. Cook eds., 2003) (reassessing the impact of Project Exile on homicide and other crimes).

committing further gun crimes and are offered social, educational, health, and employment services.⁹

An initial evaluation examined the effects of PSN Chicago from 2002 to 2004.¹⁰ The investigation found evidence that several of these PSN initiatives reduced violent crime in the two target districts. Modest reductions in violence were associated with the number of PSN prosecutions and gun seizures, while larger reductions were associated with the percentage of eligible offenders who attended an offender notification forum.¹¹

PSN Chicago did not stop in 2004. On the contrary, the program continues to this day in the original target districts. And, after the initial evaluation, PSN swiftly expanded to other areas of the city, often with no increase in programmatic resources. By 2007, both the case-review process and the multi-agency gun-teams had expanded citywide. And by 2009, the offender notification forums were distributed across 24% of the police districts in the city.

The present study seeks to answer two questions. First, do the original PSN districts continue to experience programmatic effects years later? Much has happened in Chicago since 2002, including an unprecedented decline in the city's homicide rate to a nearly 48-year low.¹² Second, have PSN expansion districts experienced programmatic effects that are comparable to those in the original districts? To answer these questions, the present study builds upon the initial evaluation by examining data that spans roughly eight years of the program and a larger geographic area.

Evaluations of anti-violence programs like PSN face a series of well-known methodological challenges.¹³ Most importantly, the initial target areas for PSN were not selected randomly. They were selected precisely because they had the highest rate of homicide and gun violence in the city.¹⁴ Two challenges result. First, non-random selection creates the potential for

⁹ Papachristos et al., *supra* note 4, at 231–32.

¹⁰ *Id.* at 245, 255.

¹¹ *Id.* at 260.

¹² Andrew V. Papachristos, *48 Years of Crime in Chicago: A Descriptive Analysis of Serious Crime Trends from 1965 to 2013*, at 5 (Inst. for Soc. & Policy Studies, Working Paper No. 13-023, 2013), http://isps.yale.edu/sites/default/publication/2013/12/48yearsofcrime_final_ispsworkingpaper023.pdf.

¹³ See, e.g., Richard A. Berk, *Knowing When to Fold 'Em: An Essay on Evaluating the Impact of Ceasefire, Compstat, and Exile*, 4 CRIMINOLOGY & PUB. POL'Y 451 (2005) (identifying many of the methodological limitations in the design of observational studies of anti-violence programs).

¹⁴ Papachristos et al., *supra* note 4, at 242.

bias because we cannot apply an experimental design to evaluate the effects of the program. Second, PSN's decision to target the highest crime areas of the city creates the potential for statistical imprecision because there are few remaining neighborhoods for our comparison group. This problem is exacerbated by PSN's expansion throughout the city. The comparison groups from the earlier evaluation of PSN Chicago, for example, quickly became the second set of target areas when the program expanded.

The current study represents our best effort to overcome these challenges given the data and methodological tools available. We use propensity score matching to select a set of comparison areas for the original and expansion PSN districts based on neighborhood-level measures that are theoretically linked with violence. We apply growth curve and fixed effect models to estimate the effects on homicide of the dosage of three main program components: PSN prosecutions, gun seizures, and offender notification forums.

Our results are as follows. First, in the original PSN areas we find a modest negative correlation between homicide and the dosage of federal prosecutions and offender notification forums. These results are sensitive to some alternative model specifications, which we discuss in greater detail below. For instance, when we exclude the first three years of PSN from the analysis, the negative correlation between forum dosage and homicide decreases dramatically and is no longer statistically significant. This suggests that PSN may have reduced homicide in its early years, but as with other similar anti-violence programs, the effects may have dissipated over time.¹⁵ Second, we find little evidence of programmatic effects in the expansion areas. Both of these findings may, to some extent, reflect greater levels of error in our measures of PSN dosage due to changes in data collection over time.

The remainder of the paper is organized as follows. Part I describes PSN Chicago, the initial neighborhoods targeted by the program, and the evaluation by Papachristos et al. It then reviews some of the challenges faced by the PSN team in sustaining the program over its long life. Part II describes the design of the current study, and Part III details our results. Part IV explores the implications of our work for PSN, other anti-violence programs, and future research.

¹⁵ See Robin S. Engel et al., *Reducing Gang Violence Using Focused Deterrence: Evaluating the Cincinnati Initiative to Reduce Violence (CIRV)*, 30 JUST. Q. 403, 431–32 (2013) (discussing the failure of some anti-violence programs to sustain effects over longer periods of time).

I. PSN IN CHICAGO

A. PROGRAM OVERVIEW

As described elsewhere, PSN Chicago is not a single program but a bundle of interrelated interventions aimed at reducing gun violence in high-crime communities. Since May 2002, the U.S. Attorney's Office for the Northern District of Illinois has overseen a PSN taskforce of representatives from the Chicago Police Department, the Cook County State's Attorney's Office, the Illinois Department of Corrections, the Cook County Department of Probation, the Bureau of Alcohol, Tobacco, Firearms, and Explosives, the City of Chicago, a group of research partners, and more than twelve community-based organizations. The taskforce meets monthly to coordinate PSN operations.¹⁶

PSN Chicago consists of three core strategies. First, the taskforce increased federal prosecutions of gun crimes.¹⁷ Prosecuting gun offenders in federal rather than state court was thought to increase deterrence because federal prison sentences tend to be longer, federal inmates serve a minimum of 85% of their sentence, and federal prosecutions have a higher overall conviction rate.¹⁸ To increase the number of federal prosecutions, the PSN taskforce designed a case review process in which federal and local prosecutors review all gun cases in Chicago to determine whether to prosecute in state or federal court.¹⁹

Second, the taskforce established a PSN "gun team." The Chicago Police Department recovered between 10,000 and 16,000 guns per year between 1995 and 2002.²⁰ Yet at the time PSN was established, the department's gun-related policing efforts were primarily reactive, occurring after the commission of a gun crime or else as a supplement to criminal investigations of other types of offenses. PSN created a multi-agency "gun team" to investigate supply-side crimes like gun use, sales, and trafficking. The gun team also conducted seizures and served warrants on pending cases involving guns.²¹

¹⁶ Papachristos et al., *supra* note 4, at 229–33.

¹⁷ *Id.* at 233.

¹⁸ Steven D. Clymer, *Unequal Justice: The Federalization of Criminal Law*, 70 S. CAL. L. REV. 643, 674–75 (1997).

¹⁹ Papachristos et al., *supra* note 4, at 233.

²⁰ CHI. POLICE DEP'T, 2002 ANNUAL REPORT 15 (2002), <https://portal.chicagopolice.org/portal/page/portal/ClearPath/News/Statistical%20Reports/Annual%20Reports/02AnnualReport.pdf>.

²¹ Papachristos et al., *supra* note 4, at 233.

Third, the PSN taskforce developed offender notification forums (forums), one of the program's most distinctive features. A forum is a round-table meeting with twelve to twenty offenders who have a history of gun violence and gang participation, and who were recently assigned to parole or probation. These hour-long meetings are held in a non-law enforcement location of civic importance such as a local park building, community organization, or school.²² The first segment of the meeting lasts approximately fifteen minutes and stresses deterrence. A panel of local and federal law enforcement emphasizes the consequences of future gun offenses, including the likelihood of federal prosecution. In the second fifteen-minute segment, an ex-offender involved in local crime prevention programs discusses the seriousness of gun violence in the community and the real-life struggles that ex-offenders face when re-entering society. The final segment stresses the choices offenders can make to avoid gun crime. This thirty-minute segment is presented by service providers, education specialists, health professionals, and employment counselors, all of whom offer services and outreach.²³

The forums are based on two interrelated principles. First, deterrence messages should directly target gun offenders.²⁴ Since the vast majority of the population—including the offending population—does not engage in gun-related crimes, broad sweeping deterrence strategies are an inefficient use of limited resources.²⁵ Second, positive interactions with police officers and prosecutors can increase offenders' perception of the legitimacy of law enforcement and may encourage greater compliance with the law.²⁶

PSN originally targeted the 11th and 15th police districts in Chicago,²⁷ which had experienced some of the highest per capita homicide rates in the

²² *Id.* at 237.

²³ *Id.* at 231–32.

²⁴ Anthony A. Braga & David L. Weisburd, *The Effects of Focused Deterrence Strategies on Crime: A Systematic Review and Meta-Analysis of the Empirical Evidence*, 49 *J. RES. CRIME & DELINQ.* 323, 327 (2012).

²⁵ In 2010, there were approximately 159,000 arrests for weapons violations. See *Crime in the United States*, FBI.GOV, <https://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/tables/10tbl29.xls> (last visited June 30, 2016).

²⁶ See Andrew V. Papachristos, Tracey L. Meares & Jeffrey Fagan, *Why Do Criminals Obey the Law? The Influence of Legitimacy and Social Networks on Active Gun Offenders*, 102 *J. CRIM. L. & CRIMINOLOGY* 397, 432–35 (2012) (finding that offenders are more likely to comply with the law when they perceive police are legitimate government actors).

²⁷ At the time PSN began, the Chicago Police Department operated twenty-five police districts that covered the entire city. These districts were subdivided into roughly 280 “beats” of approximately one square mile. Districts were selected as the unit of intervention by the

city since the early 1980s. When the program began in 2002, there were 75.5 homicides and 13.8 gang homicides per 100,000 residents in these districts—over three times the citywide average.²⁸ The 11th and 15th districts are located on the west side of the city (see Figure 1), and include North Lawndale, West Garfield Park, and Austin. They are predominately African American (97%) and have high levels of poverty, high school dropout, unemployment, and gang membership.²⁹

Importantly, PSN has never operated in a vacuum. A number of other programs have emerged over the last 15 years, including Cure Violence (formerly CeaseFire).³⁰ Many PSN partners—such as the Chicago Police Department, the Cook County State’s Attorney’s Office, Cook County Probation, and the Illinois Department of Corrections—have implemented various other anti-violence programs.³¹ And non-profit groups, community organizations, and churches have organized smaller programs that less frequently garner media attention. We lack systematic data on these other programs, but to our knowledge, none were implementing anything like PSN’s offender notification hearings.

PSN taskforce as they represented the most convenient administrative unit. Most local policing decisions and resources, for instance, are managed by a district commander. CHI. POLICE DEP’T, *supra* note 20, at 22.

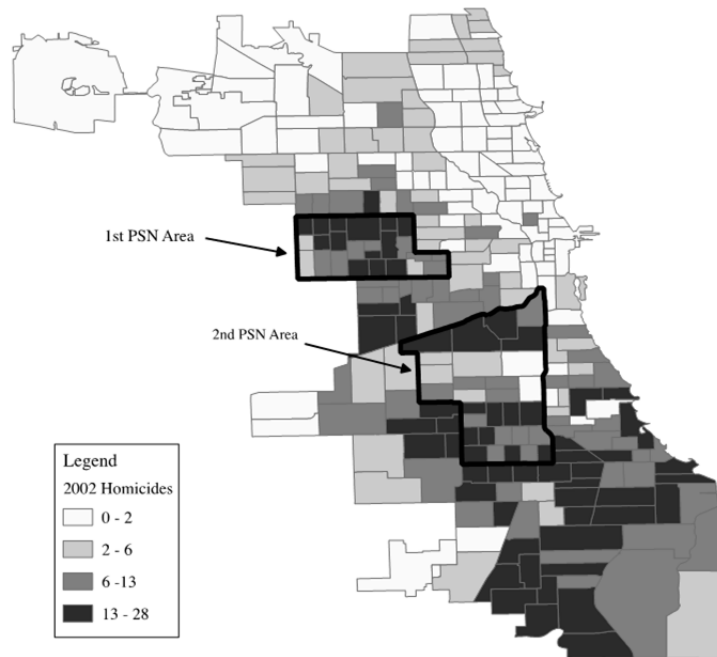
²⁸ Papachristos et al., *supra* note 4, at 242–43.

²⁹ For a thorough description of the development of this area and its relationship to larger social and historical patterns in Chicago, see ARNOLD R. HIRSCH, *MAKING THE SECOND GHETTO: RACE AND HOUSING IN CHICAGO, 1940–1960* (1983).

³⁰ See WESLEY G. SKOGAN, SUSAN M. HARTNETT, NATALIE BUMP & JILL DUBOIS, *EVALUATION OF CEASEFIRE-CHICAGO*, at 1-7 to 1-11 (2008) (describing the CeaseFire model in Chicago, which included a number of programmatic elements including public health and educational outreach, anti-violence street intervention, clergy involvement, and community mobilization).

³¹ See, e.g., CIRCUIT COURT OF COOK CNTY., *Adult Probation Programs*, <http://www.cookcountycourt.org/ABOUTTHECOURT/OfficeoftheChiefJudge/ProbationDepartments/ProbationforAdults/AdultProbationDepartment/Programs.aspx> (follow “Gang Intervention Unit”) (last visited Nov. 14, 2015) (describing the probation department’s gang intervention unit, which was established in 1991).

Figure 1
PSN Treatment Areas & Total Number Homicides in 2002



B. INITIAL EVALUATION: DESIGN AND RESULTS

The initial evaluation by Papachristos et al. examined the effects of PSN from January 2002 to December 2004 in the 11th and 15th police districts.³² The authors addressed the non-random selection of the target areas in two ways. First, they used the 7th and 9th districts as comparison areas because they had some of the highest homicide rates in the city and they were not geographically adjacent to the 11th and 15th.³³ Second, they applied propensity score models to control for known neighborhood-level measures theoretically linked with violence.³⁴

The investigation represented a significant advancement over previous evaluations of focused deterrence anti-violence programs in two ways.

³² Papachristos et al., *supra* note 4, at 245, 255.

³³ *Id.* at 241–42 (illustrating and discussing the concentration of gun violence in the study districts).

³⁴ *Id.* at 253–54.

First, prior evaluations of Boston Ceasefire and Project Exile examined city-level crime rates even though the interventions targeted narrow geographic areas and populations.³⁵ In contrast, the design employed by Papachristos et al. reduced bias by analyzing smaller geographic units that were directly targeted by PSN. Second, whereas prior studies identified programmatic effects based on changes in crime before and after program implementation, the initial evaluation measured the dosage of program elements to identify which elements were associated with changes in crime.³⁶

The initial investigation of the first three years of the program found that several PSN initiatives were associated with statistically significant declines in homicide. Overall, the PSN target communities experienced a 37% drop in homicide.³⁷ Small to moderate reductions in homicide were associated with the number of federal prosecutions and recovered guns. The largest reductions were associated with the proportion of eligible offenders who attended an offender notification forum.³⁸ A follow-up study reports that eligible offenders who attended a forum spent less time in prison than those that did not.³⁹

C. PROGRAM EXPANSION AND OTHER CHANGES IN CHICAGO

Even before the publication of the initial evaluation, PSN expanded quickly throughout the city of Chicago. Both the prosecutorial case review and the gun team went citywide by the end of 2004 (though PSN districts continued to receive priority status for the former). The forums expanded more slowly. In 2005, the PSN taskforce selected the 7th and 9th districts as the next PSN target areas (see Figure 1). These districts cover largely African American neighborhoods with high levels of poverty, high school dropout, unemployment, and violence. In 2006, the program expanded again to the 10th and 4th districts. Thus by 2009, a program that began as a

³⁵ *Problem-Oriented Policing*, *supra* note 8, at 203–18; Anne Morrison Piehl et al., *Testing for Structural Breaks in the Evaluation of Programs*, 85 *REV. ECON. & STAT.* 550, 551–53 (2003); Raphael & Ludwig, *supra* note 8, at 256–77; Richard Rosenfeld et al., *Did Ceasefire, Compstat, and Exile Reduce Homicide?*, 41 *CRIMINOLOGY & PUB. POL'Y* 419, 428–31 (2005).

³⁶ Papachristos et al., *supra* note 4, at 247–49.

³⁷ *Id.* at 254.

³⁸ *Id.* at 256–58.

³⁹ Andrew V. Papachristos, Danielle M. Wallace, Tracey L. Meares & Jeffrey Fagan, *Desistance and Legitimacy: The Impact of Offender Notification Meetings on Recidivism Among High Risk Offenders* 18 (Columbia Law Sch. Pub. Law & Legal Theory Working Paper Grp., Paper No. 13-343, 2013), <http://dx.doi.org/10.2139/ssrn.2240232>.

targeted effort in just 8% of the city's police districts spread to 24%. Today, PSN includes nearly all of the city's high-crime police districts.

Unfortunately, the expansion of the program was not accompanied by an increase in funding or personnel.⁴⁰ Finite resources were spread across a larger area of the city and program efficacy may have suffered as a result. For example, at the program's inception two forums were held each month in the two original PSN districts. By 2006, the two bimonthly forums were split across six districts on a rotating basis. As a result, the aggressive expansion may have overstretched the program's resources.

In the later years of the program, PSN faced a number of other common challenges in maintaining the "network of capacity" essential to its early success.⁴¹ First, focused-deterrence programs often rely on a complex web of agencies and organizations for key services and support.⁴² As priorities shift and budgets change, these large coalitions sometimes drift apart. As just one example, the Ceasefire program in Boston became less effective over time because an organization of African American clergymen withdrew from the initiative.⁴³ Without the clergy, the program lacked sufficient community support and capacity to engage in critical street-level services.⁴⁴ While less extreme, PSN faced similar challenges in Chicago as dozens of community groups participated in varying ways throughout the first decade of the program. Several grassroots organizations that originally participated in the forums ceased to operate. A small group composed of fathers of homicide victims, for example, ceased to exist because of members' other commitments. Small health care providers and drug outreach organizations also came and went as their staffing and funding demands fluctuated. To address these service gaps, PSN quickly sought out new partner organizations.

⁴⁰ Resources here refer not just to money, but also manpower. The PSN team remained a small and dedicated group of individuals. The PSN forums occurred after normal working hours, and PSN team and community members participated in such meetings without compensation.

⁴¹ Mark H. Moore, *Creating Networks of Capacity: The Challenge of Managing Society's Response to Youth Violence*, in *SECURING OUR CHILDREN'S FUTURE: NEW APPROACHES TO JUVENILE JUSTICE AND YOUTH VIOLENCE* 338, 372 (Gary S. Katzmann ed., 2002).

⁴² *Id.* at 372–75.

⁴³ See, e.g., Anthony A. Braga et al., *Losing Faith? Police, Black Churches, and the Resurgence of Youth Violence in Boston*, 6 *OHIO ST. J. CRIM. L.* 141, 156, 160–62 (2008) [hereinafter *Losing Faith?*] (describing the effect of the withdrawal of a group of clergymen from the Ceasefire program).

⁴⁴ *Id.*

Second, mounting an effective anti-violence program requires tremendous organizational skill and personal investment. Turnover in personnel can thus create significant challenges.⁴⁵ Prior team members may be replaced by individuals with fewer skills or a weaker commitment to the program's strategy. Boston's Operation Ceasefire once again provides a useful example as it ended soon after the "operational steward" of the program, a lieutenant in the police department, was transferred to a different unit.⁴⁶

The PSN team experienced significant personnel turnover. Since it began in 2002, there have been two U.S. Attorneys in the Northern District of Illinois, five different Assistant U.S. Attorneys leading daily PSN operations, four police superintendents, ten police commanders, two Cook County State's Attorneys, two mayors, and three different fiscal agents monitoring grant activities.⁴⁷ At times, prior team members were replaced by new personnel with weaker commitments to the program's core principles and strategy. To help address these problems, the PSN coordinators and core support team had to conduct several "reboot" trainings to promote buy-in among new personnel and ensure continued fidelity in the offender notification forums. Thus, despite turnover, PSN remained faithful to most of its core programmatic elements, especially prosecutorial review and the forums.

II. RESEARCH DESIGN AND DATA

The present study has two main objectives. First, it examines data on the effect of PSN in the original PSN districts that span a period of roughly eight years. Second, it tests whether PSN expansion districts experienced effects similar to the initial districts.

The non-random selection of the original PSN sites in Chicago poses serious challenges for causal inference. The non-random expansion of the program further exacerbates the problem. It is, therefore, impossible to test the effect of PSN with a randomized experiment. Instead, we use propensity score matching to select a set of comparison neighborhoods that are similar in historical rates of homicide and in theoretical measures associated with

⁴⁵ Moore, *supra* note 41, at 372, 376–77.

⁴⁶ *Losing Faith?*, *supra* note 43, at 156.

⁴⁷ See, e.g., *Meet Anita*, ANITAALVAREZ.COM, <http://www.anitaalvarez.com/meet-anita/> (last visited June 30, 2016) (noting that Anita Alvarez was elected as the Cook County State's Attorney in 2008); *About the Mayor*, CITYOFCHICAGO.ORG, http://www.cityofchicago.org/city/en/depts/mayor/supp_info/about_the_mayor.html (last visited Nov. 14, 2015) (noting that Rahm Emanuel was elected Mayor of Chicago in 2011).

violence. We then fit fixed effects and growth curve models on beat-level data from 1999 to 2010 to estimate the relationship between measures of PSN dosage and homicide. We begin by describing our strategy for selecting comparison beats and then discuss our models.

A. CHOOSING COMPARISON BEATS

Our unit of analysis is the police beat, the smallest geographic unit used by the Chicago Police Department for which data are consistently available over time. For convenience, we refer to (1) police beats that received PSN as the *treatment group*, (2) beats that did not receive PSN as the *control group*, and (3) beats in the control group that are selected as comparison beats for our analysis as the *matched control group*.

To estimate propensity scores for all beats in the city, we fit a logistic regression model on a dummy variable indicating whether each beat is in a PSN area (1 = yes, 0 = no). Given the manner in which the program expanded, we estimate the logistic regression twice: first, with a dependent variable indicating whether a beat is in the original PSN target areas (the 11th and 15th districts) and then again with a dependent variable indicating whether a beat was in one of the expansion areas (the 7th and 9th districts). These two logistic models estimate the propensity score for each beat at the two selection stages.

The logistic regression models contain six independent variables. The first three variables are neighborhood-level characteristics that are known correlates of violent crime: socio-economic deprivation, concentrated immigration, and residential stability.⁴⁸ These measures were derived through principal components analysis from twelve census variables.⁴⁹ Two further variables indicate the number of homicides in each beat in the two years prior to PSN implementation. The final variable is the size of the residential population.

Next, we use the propensity scores from the logistic regression to select non-PSN beats that are similar to PSN beats. In order to increase balance on relevant covariates, we specify the matching algorithm to favor matched pairs that are not only similar on propensity scores but also on the covariates with which we fit the logistic regression models.⁵⁰ Importantly,

⁴⁸ See Robert J. Sampson & W. Byron Groves, *Community Structure and Crime: Testing Social-Disorganization Theory*, 94 AM. J. SOC. 774, 780–81 (1989) (discussing the theoretical significance of these neighborhood-level characteristics).

⁴⁹ For a more detailed description, see Papachristos et al., *supra* note 4, at 246.

⁵⁰ See PAUL R. ROSENBAUM, *DESIGN OF OBSERVATIONAL STUDIES* 168–72 (2010).

all police beats that have been selected as PSN target areas were dropped before matching to ensure they were not selected as matched controls.⁵¹ A matched control group was selected for each of the two stages of site selection. In the first stage, twenty-four matched control beats were selected for each of the twenty-four treatment beats in the 11th and 15th districts; in the second stage, thirty matched control beats were selected for each of the thirty treatment beats in the 7th and 9th districts. We considered selecting a larger number of matched control beats to increase statistical power, but found an insufficient number of similar beats to do so.

A comparison of pre-treatment covariates in the 11th and 15th districts reveals that propensity score matching has dramatically increased the comparability of the treatment and control group.⁵² Though the shapes of some of the distributions differ in the treatment and matched control group,⁵³ the means of the distributions are all similar. Most importantly, the distributions of the propensity score in the treatment and matched control group are similar. Moreover, on average, there were 4 and 4.2 homicides in 2000 in the treatment and matched control group respectively. In the next year, there were 4.6 and 4.4 respectively. Beats in both treatment and matched control groups had an average population size just over 6,300. A comparison of pre-treatment covariates reveals a similar pattern of results for the 7th and 9th districts (see Appendix Figure 2). The distributions of the propensity scores in the treatment and matched control group are relatively similar. The average number of homicides per beat in 2003 was approximately 3.1 in the treatment group and 2.3 in the matched control group. In 2004, the average number of homicides was 2.3 in the treatment group and 2 in the matched control group. Treatment beats had an average of 8,700 residents, while matched control beats had a higher average of 9,700 residents.

To assess balance between the treatment and matched control groups more systematically, we applied the t-test and Wilcoxon ranked sum test to each of the variables.⁵⁴ The only statistically significant difference at the .05 level was in the immigrant concentration variable for the first site selection.

⁵¹ Thus, the 11th and 15th were dropped because they were the first PSN areas. The 7th and 9th were also dropped because PSN expanded there in 2005. And, finally, the 4th and 10th were also dropped because PSN expanded there later.

⁵² See *infra* Appendix Figure 1.

⁵³ Some distributional differences are to be expected due to the relatively small number of beats in the treatment and matched control groups.

⁵⁴ Unlike the t-test, the Wilcoxon ranked sum test does not assume normality. See MYLES HOLLANDER & DOUGLAS A. WOLFE, *NONPARAMETRIC STATISTICAL METHODS* 36–37 (2d ed.

Ideally, the treatment and matched control group would be equivalent on all variables that affect the homicide rate. In practice, this goal was difficult to achieve. First, we may not have all relevant variables that affect homicide. Second, the program has expanded to cover roughly 20% of all beats in the city, capturing most neighborhoods with high levels of violence and predominately African American populations. PSN thus already encompasses most of the neighborhoods in the city that could serve as viable comparisons. As noted, the initial evaluation of PSN used the 7th and 9th districts as comparison groups for the 11th and 15th PSN areas, but the program has since expanded to the 7th and 9th districts.

Ultimately, the similarity between the treatment and matched control groups is not ideal relative to other non-policing studies in the evaluation literature. However, the use of propensity score matching in the evaluation of criminal justice programs like PSN presents unique challenges because of the nature of gun violence, which can never be truly randomized, and the politics and practical implementations of such programs. The use of propensity score matching in criminal justice research is still in its infancy and our matching procedures do appear to substantially improve the control group. Under these circumstances, it may be more desirable to fit the model with all observations and control for observable covariates rather than to fit the model on a matched control group selected on the basis of the observable covariates. We refit our fixed effect model with all beats in the city (excluding beats in other PSN target districts) and added a variable indicating the propensity score for each beat. The results were substantively similar.

B. ANALYTIC MODELS

We fit two different models to estimate the effect of PSN dosage on homicide in Chicago. First, we fit a fixed effect model with a fixed intercept and slope for time for each beat. Including slopes for time for each beat can lead to large standard errors, but the results are substantively similar when the slopes are removed from the models. Second, we fit a growth curve model with a random intercept and slope for time for each beat.⁵⁵

1999). The value of both tests is limited given the small sample sizes.

⁵⁵ Our growth curve model was a two-level model predicting within-beat trajectories at level 1 and between-beat trajectories at level 2 using the predicted level 1 intercepts and slopes as outcomes. We estimate the following model:

$$Y_{ij} = \mathbf{x}_{ij}\beta + \zeta_{0j} + \zeta_{1j}t_{ij} + \epsilon_{ij}$$

where \mathbf{x}_{ij} represents a matrix of constant and time varying covariates, ζ_{0j} represents a random intercept for beats, and ζ_{1j} represents a random slope for time.

We fit these models on a panel data set for all beats in the 11th and 15th districts and their matched controls, where each beat has a row for each year-quarter from 1999 to 2010. We then refit the models on a similar dataset for the 7th and 9th districts and their matched controls. The results of both the fixed effect and growth curve models are substantively similar. We, therefore, present the results of the fixed effect models, which are more resistant to unobserved bias.

We model two dependent variables: total homicide and gun homicide. Both variables are logged.⁵⁶ Our models include six main independent variables. First, we include a variable indicating whether a beat is a PSN treatment beat (1 = yes, 0 = no). Second, we include a time-varying dummy variable indicating whether PSN was in operation during a particular year-quarter (1 = yes, 0 = no). For the 11th and 15th districts, we set the PSN start date as January 2002, and for the 7th and 9th, the start date is January 2005. Third, we include a count variable representing the number of ATF gun traces in each beat in each period; this variable acts as a proxy for gun seizures. Fourth, we include the log of the number of PSN prosecutions in each beat in each period. Fifth and related to the fourth variable, we also consider a measure of the log of the average PSN sentence in each beat in each period. Finally, we include a variable that measures the log of the estimated number of residents that attended a PSN offender notification forum in each beat in each period.⁵⁷

All of the data used in this study were provided by the agencies participating in PSN, including the Chicago Police Department, the United States Attorney's Office of the Northern District of Illinois, and the Bureau of Alcohol, Tobacco, Firearms, and Explosives. Changes in personnel and data collection procedures over time present special challenges in both replicating the original variables used by the initial evaluation and in creating consistent measures for the entire study period. This problem was particularly acute for the measure of the number of attendees at PSN offender notification forums.⁵⁸ We faced two problems in particular. First, we were unable to locate attendee data from 2007 and 2008. We imputed forum data in each beat for this period with a best-fit line and added random

⁵⁶ Because some values for the dependent variable were zero, we added an offset of 0.0001 to the dependent variable before transforming to the log scale.

⁵⁷ As with the dependent variables, we added an offset of 0.0001 to all logged measures because some values were equal to zero.

⁵⁸ Unlike Papachristos et al., *supra* note 4, we measure forum dosage by the absolute number of participants rather than the proportion of eligible attendants to promote easier interpretation of the results.

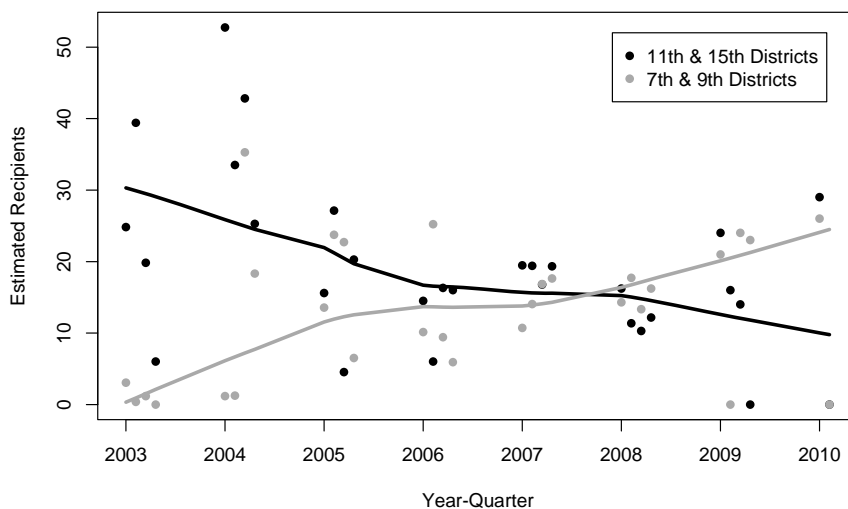
error using the mean of the absolute value of the residuals from the model. We present models that include the imputed data for 2007 and 2008, but the results for the forum variable were substantively similar when these years were excluded. Second, data from 2003 to 2006 were only available at the zipcode-level. We therefore imputed the number of forum participants from zipcode to beat through areal interpolation. For 2009 and 2010, we measured forum attendance by summing the number of forum attendees in each beat at each year-quarter. Precise address-level data for this period were available on a significant fraction of all forum attendees from the 11th and 15th (54%) and 7th and 9th districts (66%).⁵⁹

Figure 2 depicts the total number of forum attendees per year-quarter in the two PSN treatment areas. Roughly twenty-five to thirty participants attended the forums in the 11th and 15th districts per quarter in 2003. Importantly, Figure 2 also shows that the number of forum attendees in the 11th and 15th districts decreased gradually over time, especially as the program expanded to the 7th and 9th districts. The number of forum participants in the 7th and 9th districts increased over time, peaking at roughly twenty-five to thirty participants per quarter in 2009 and 2010.

⁵⁹ Because we are missing a non-trivial fraction of attendees for 2009 and 2010, we also created a second measure of forum attendance for these years. Data on the total number of all forum attendees in each year-quarter were available at the district-couplet level, that is, the combined number of forum participants in the 11th and 15th districts, and the 7th and 9th districts in each year-quarter from 2009 to 2010. Data was also available on the number of *parolees eligible* for a PSN forum during each year-quarter at the zipcode-level. We imputed the zipcode-level eligibility data to the beat-level using areal interpolation. Then, assuming that PSN *forum participants* were distributed proportionately to the number of eligible participants in each beat in each year-quarter, we imputed the total number of participants in each district couplet at each year-quarter to the beat-level. The results for both measures produced broadly similar results. We present models based on the first measure, which we believe is subject to substantially less error.

Figure 2

Estimated Aggregate Number of Forum Participants in the 11th & 15th Districts and the 7th & 9th Districts⁶⁰



III. RESULTS

Figures 3 and 4 display the average homicide rate per beat in each year-quarter for the PSN treatment areas. The vertical dotted line indicates the start date of the program in each area. The black line represents the average number of homicides per beat in the treatment group, and the gray line represents the average number of homicides per beat in the matched-comparison group. Overall, Figure 3 shows that homicide has decreased by approximately 10% since the start of PSN in the 11th and 15th districts. The steepest decline in both the treatment and matched control groups continues until 2006. Since that year, both areas' homicide rates have risen slightly, but they remain lower than the pre-PSN levels. Figure 4 shows that homicide rates in the 7th and 9th districts are more stable but also experienced a steady decline reaching a low point in 2006. In both areas, homicide rates reached their respective lowest points around that year and have risen slightly since. In the following models, we attempt to estimate whether any beat-level declines in homicide can be attributed to the dosage of PSN activities.

⁶⁰ As noted, data on the number of forum recipients for 2007 and 2008 is not available. The data presented for those years is imputed.

Figure 3
Average Homicide Per Beat in 11th and 15th Districts and Matched Control Group

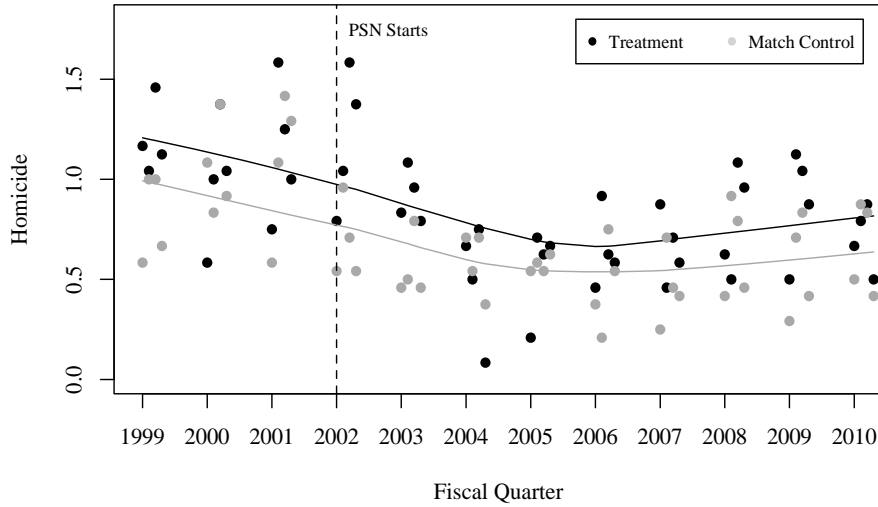
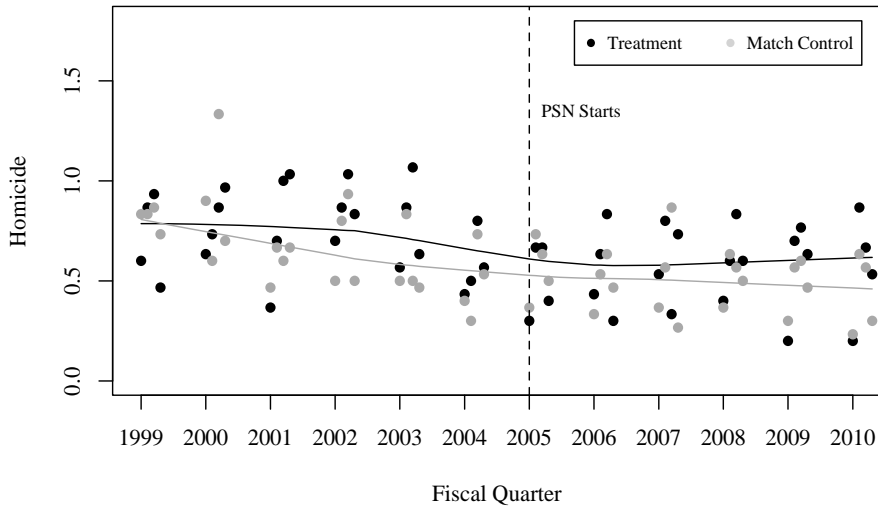


Figure 4
Average Homicide Per Beat in 7th and 9th Districts and Matched Control Group



The results from our analyses provide insights into both the sustained effects within the original PSN districts and the effects in the expansion

districts. Tables 1 and 2 present the coefficients and standard errors (in parentheses) from our regression models for the original and expansion PSN districts.⁶¹

Table 1 displays the results of the fixed effect model for the 11th and 15th police districts using data from 1999 to 2010. Several key findings emerge. First, the PSN Operating coefficient is negative in both the total homicide and gun homicide models, showing a substantial and statistically significant decrease in homicides when PSN is in operation.

Table 1
*Fixed Effect Models on Logged Total and Gun Homicide,
11th & 15th Districts*

	Total Homicide		Gun Homicide	
	B	p-value	B	p-value
PSN Operating	-1.41 (.460)	0.002	-1.06 (.433)	0.014
Trace	0.047 (.021)	0.027	0.044 (.020)	0.031
log(Prosecution)	-0.249 (.076)	0.001	-0.221 (.080)	0.006
log(Sentence)	0.237 (.066)	0.000	0.236 (.066)	0.000
log(Forum)	-0.093 (.034)	0.006	-0.103 (.032)	0.001
Year-Quarter	-0.04 (.015)	0.009	-0.066 (.014)	0.000
Fixed Intercepts for 48 Beats	Yes		Yes	
Fixed Slopes for 48 Beats	Yes		Yes	

Note: Bold coefficients are significant at the .05 level

Second, the coefficients for both of the prosecutorial variables are statistically significant. The logged number of prosecutions is negative and statistically significant for both overall homicides and gun homicides. The coefficient is -0.249 for total homicides and -.221 for gun homicides, which means that a 1% increase in PSN prosecutions is associated with a .25% decrease in total homicides per beat and a 0.24% decrease in gun homicides per beat. The sentence length parameter is positive and statistically significant in both the total and gun homicide models, indicating that higher sentence lengths are associated with more homicides; in this case, the

⁶¹ We present cluster-robust standard errors clustered by beat.

positive association could reflect the association between higher sentences in federal prosecution and the overall more serious nature of crimes diverted to federal court (i.e., very few low-level, nonviolent crimes make their way to federal court). It is important to note once again that the total number of federal PSN prosecutions is low relative to the overall number of gun cases in the city. Furthermore, the average number of cases taken to federal court has declined over the life of the PSN program. In 2005, the State of Illinois developed its own “armed habitual criminal” statute that enhanced penalties for gun offenders in state court.⁶² As a result, cases that might have previously been selected for federal prosecution may have remained in the state system.

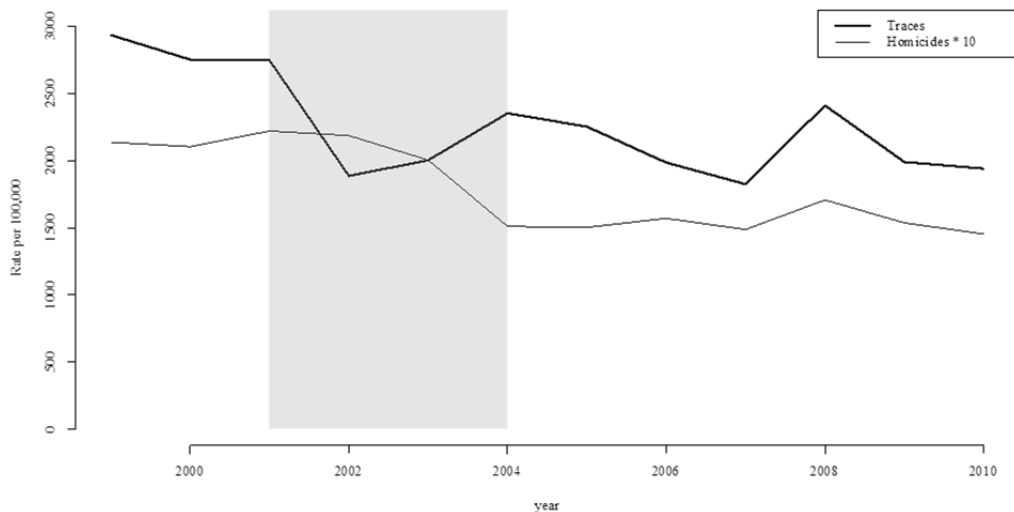
Third, the parameter estimates for the number of guns traced is positive and statistically significant, which suggests that as the number of guns recovered in a beat increases, the number of homicides also increases. Surprisingly, this finding runs contrary to that of Papachristos et al., who found a negative association between gun traces and homicides.⁶³ This directional reversal may arise from changes in the practice of firearms tracing and gun policing more generally in Chicago. When PSN began in 2002, Chicago’s comprehensive tracing program had fallen behind in trace requests; some members of the PSN team also believed that gun seizures had diminished as a policing priority. Getting tracing back on track and making gun seizures a priority became a central goal of PSN, especially within the treatment areas. The formation of the PSN gun teams was viewed as an important step in renewing these commitments to gun policing efforts. Figure 5 plots the annual number of firearms traced and the homicide rate (times 10) during the observation period. The shaded area displays the initial PSN evaluation period. While it is impossible to place causal weight on this explanation, Figure 5 provides suggestive evidence that the original finding by Papachristos et al. captured a unique point in time when gun seizures and traces were at a low point. With the exception of the early years of PSN, Figure 5 shows that the trends between gun traces and homicides are roughly parallel: more guns are recovered in years with more homicides. Whether or not this change in seizures and subsequent tracing can be attributed to PSN is not something we can answer with our data. But the timing of these changes shows that PSN started at an important time for gun policing and gun tracing efforts in Chicago.

⁶² 720 ILL. COMP. STAT. 5/ § 24–1.7 (2005).

⁶³ Papachristos et al., *supra* note 4, at 258.

Figure 5

Annual Number of Gun Traces and Homicides (times 10) in Chicago, 2000 to 2010



Finally, our model in Table 1 provides evidence of an effect for the number of forum attendees. The coefficient is $-.093$ for the total homicide model and -0.103 for the gun homicide model. This means that a 1% increase in the number of PSN forum attendees is associated with a 0.1% decrease in homicide and a 0.11% decrease in gun homicide. The results are generally consistent with the prior evaluation, which also found that the forums were associated with a statistically significant reduction in total and gun homicides in the 11th and 15th police districts.⁶⁴

Results from the 11th and 15th districts are sensitive to several alternative model specifications. First, excluding data on the first three years of the program (2002–2004) leads to dramatically smaller coefficients for the forum variable that are not statistically significant (not shown), suggesting that some of the results in Table 1 are driven by the initial period of the program. Second, in our main model we set the start of PSN in the 11th and 15th districts as January of 2002, but not all of the PSN programs started at that time. The first offender notification forum did not take place until the next year. Setting the start date for PSN in the 11th and 15th districts as January of 2003 cuts the coefficient for the forum variable in

⁶⁴ *Id.* at 256–57.

half and renders it statistically insignificant. We believe that 2002 is the more appropriate start date as other aspects of PSN—including the gun teams, prosecutorial efforts, and media outreach—were all underway; nonetheless, sensitivity in the results warrant caution in interpreting our models.

Table 2 presents our models for the expansion of PSN to the 7th and 9th districts. Consistent with the models for the 11th and 15th, the number of ATF gun traces is positively correlated with homicide. In contrast to the 11th and 15th districts, however, the models detect no statistically significant association for either the number of forum participants or PSN prosecutions. The parameters are directionally consistent but much smaller and not statistically significant.⁶⁵

A number of matched control beats in our primary analysis are directly adjacent to a PSN district (e.g., 7th, 9th, 11th, or 15th) or are located in districts that are adjacent to a PSN district. This raises the possibility of program-effect “spill over.” To accommodate for this problem, we estimated two alternative model specifications. One set of models selected comparison beats excluding all *districts* that are adjacent to any PSN districts. Unsurprisingly, this selection procedure left an insufficient number of comparable non-PSN beats to achieve adequate balance on pre-treatment covariates. In a second set of models, we selected comparison beats by excluding all *beats* that are directly adjacent to PSN districts. This matching strategy produced a matched comparison group with less balance on pre-treatment covariates than the primary matched comparison group, but the difference is not dramatic. The results of the analysis for this alternative matched control group were substantively similar to our primary results.

⁶⁵ Notably, additional models not shown here using the second measure of forum participation estimate directionally consistent coefficients that were close to being statistically significant at the .05 level. We place greater trust in the measure of forum recipients presented in Table 2, which, while also subject to some error, provides a more accurate beat-level measure of forum participants in the later years of the analysis.

Table 2
*Fixed Effects Models on Logged Total and Gun Homicide,
 7th & 9th Districts*

	Total Homicide		Gun Homicide	
	B	p-value	B	p-value
PSN Operating	-0.046 (.397)	0.907	-0.286 (.389)	0.463
Trace	0.035 (.016)	0.027	0.041 (.015)	0.004
log(Prosecution)	-0.109 (.075)	0.147	-0.109 (.068)	0.112
log(Sentence)	0.084 (.066)	0.207	0.094 (.055)	0.100
log(Forum)	-0.027 (.043)	0.533	-0.040 (.042)	0.352
Year-Quarter	-0.080 (.015)	0.000	-0.085 (.015)	0.000
Fixed Intercepts for 48 Beats	Yes		Yes	
Fixed Slopes for 48 Beats	Yes		Yes	

Note: Bold coefficients are significant at the .05 level

IV. DISCUSSION & LIMITATIONS

The initial evaluation of PSN Chicago found that the dosage of several program components was negatively correlated with total homicide and gun homicide in the initial target areas.⁶⁶ The authors found a particularly strong negative correlation between homicide and offender notification forums. Years later, PSN continues to operate in the original target communities and has also expanded to other high-crime areas of the city.

The current study sought to answer two questions. First, do the original PSN target communities continue to experience positive programmatic effects years later? Second, have subsequent PSN target districts experienced programmatic effects that are comparable to those in the initial target districts? With respect to the first question, we find a continued—albeit smaller—negative correlation between the outcome variables and the number of forum participants and PSN prosecutions. A decade after PSN began we can still detect programmatic effects in the original treatment

⁶⁶ See Papachristos et al., *supra* note 4, at 254–59.

communities, at least in the first three years of the program's operation. Our models detected little evidence, however, of programmatic effects associated with the expansion to the 7th and 9th police districts.

Given the longevity of PSN Chicago and the differences between the results of the current analysis and that of the initial evaluation, it is worth considering why the results vary by time and police district.

We believe two theoretical considerations provide the most plausible answers. First, the effects of large-scale social interventions can dissipate over time. It is possible, for example, that focused deterrence strategies like PSN exert a short-term shock on homicide rates that dissipates over time as residents become accustomed to the new and more punitive criminal justice "regime." Although some studies have examined the effect of focused-deterrence strategies over several years, there is little empirical evidence in the literature on the effects of such strategies over the long term.⁶⁷

Second, changes in the implementation and activities of the program may also explain why we observe smaller correlations between program elements and homicide rates in the later years. Perhaps most importantly, the expansion of PSN to other areas of the city may have diluted the treatment dosage in the original PSN districts. By 2009, PSN expanded to 24% of all police districts without any increase in program resources. Program dilution may also help explain why we observe little evidence of a reduction in violence in the expansion districts.⁶⁸ Indeed, the 7th and 9th districts are considerably larger in terms of both geography and population.⁶⁹ These areas may have never achieved the kind of "saturation"

⁶⁷ One study examined the effect of the Cincinnati Initiative to Reduce Violence over a 3.5-year period. See Engel et al., *supra* note 15. Two others have examined Boston's Operation Ceasefire program, first over a two-year period in the 1990s, and then again over a four-year period in the 2000s when it started back up again in response to a resurgence in juvenile crime. See *Problem-Oriented Policing*, *supra* note 8; Anthony A. Braga, David M. Hureau & Andrew V. Papachristos, *Deterring Gang-Involved Gun Violence: Measuring the Impact of Boston's Operation Ceasefire on Street Gang Behavior*, 30 J. QUANT. CRIMINOLOGY 113 (2014).

⁶⁸ See, e.g., *Losing Faith?*, *supra* note 43, at 156–57 (2008) (arguing that the ability of the City of Boston to combat youth violence decreased over time as its anti-violence strategies were expanded "to a broader range of problems such as investigating unsolved shootings, the re-entry of incarcerated violent offenders back into high-risk Boston neighborhoods, and criminogenic families in hot spot areas").

⁶⁹ See CHI. POLICE DEP'T, 2006 ANNUAL REPORT 31 (2006), https://portal.chicagopolice.org/portal/page/portal/ClearPath/News/Statistical%20Reports/Annual%20Reports/06ARRev_2.pdf (stating that the total population in the 11th and 15th districts is 155,128, and in the 7th and 9th is almost 260,000); see *supra* Figure 1 (showing that the 7th and 9th districts are larger geographically than the 11th and 15th).

that took place in the original PSN areas. If dilution is responsible for the diminished effects in the expansion areas, then policymakers should hesitate to expand effective PSN programs to new geographic areas unless commensurate resources are available. Future research may test these two theoretical explanations by examining long-term focused-deterrence interventions that invest a stable level of resources in the original target area throughout the study period.

Several methodological considerations may also help explain the divergent results in the current paper and the initial evaluation by Papachristos et al. First, they used the 7th and 9th districts as comparison areas for the 11th and 15th districts because of similarities in homicide rates and socio-ecological neighborhood features. Since PSN later expanded to these areas of the city, we are unable to use them as comparison beats. To overcome the small number of plausible comparison neighborhoods left untreated by PSN, we used propensity score matching to select matched control beats from across the city. Second, our measure of PSN forums differs somewhat from the measure used in the earlier evaluation. Papachristos et al. measured the treatment dosage as a proportion of eligible parolees who attended a PSN forum while we measure the total number of forum participants. More importantly, our measures of PSN attendance in the later years of the study are subject to greater error due to changes in data-collection practice over time.⁷⁰ For two years, we have no forum data at all. This higher level of error likely attenuated the magnitude of our estimates.⁷¹

Before concluding, we offer several additional words of caution in interpreting the results of our models. First, as noted earlier, we observed imperfect balance on pre-treatment covariates and propensity scores between the treatment and matched control groups. The treatment and matched control groups may also differ on other relevant variables that we were unable to observe. For example, research on “neighborhood effects” consistently shows the importance of various social processes and structures that are related to neighborhood-level crime rates, which we are unable to

⁷⁰ Specifically, our first measure of the forums is missing a significant fraction of all participants. Our second measure involves imputation, both from the zipcode-level to the beat-level, and from the district-level to the beat-level. Both measures produced results that are similar in direction and magnitude, reinforcing confidence in our results.

⁷¹ See JEFFREY M. WOOLDRIDGE, *ECONOMETRIC ANALYSIS OF CROSS SECTION AND PANEL DATA* 81 (2d ed. 2010).

capture in our data.⁷² These considerations further limit the strength of our causal inferences.

Second, some of the matched control beats in our analysis are either directly adjacent to a PSN district or are located in districts that are adjacent to a PSN district, which raises the possibility of program-effect “spill over.” To address this problem, we fit an alternative model specification that excluded all beats directly adjacent to PSN districts. Although this matching strategy produced a matched comparison group with less balance on pre-treatment covariates than the primary matched comparison group, the results of our models were substantively similar.

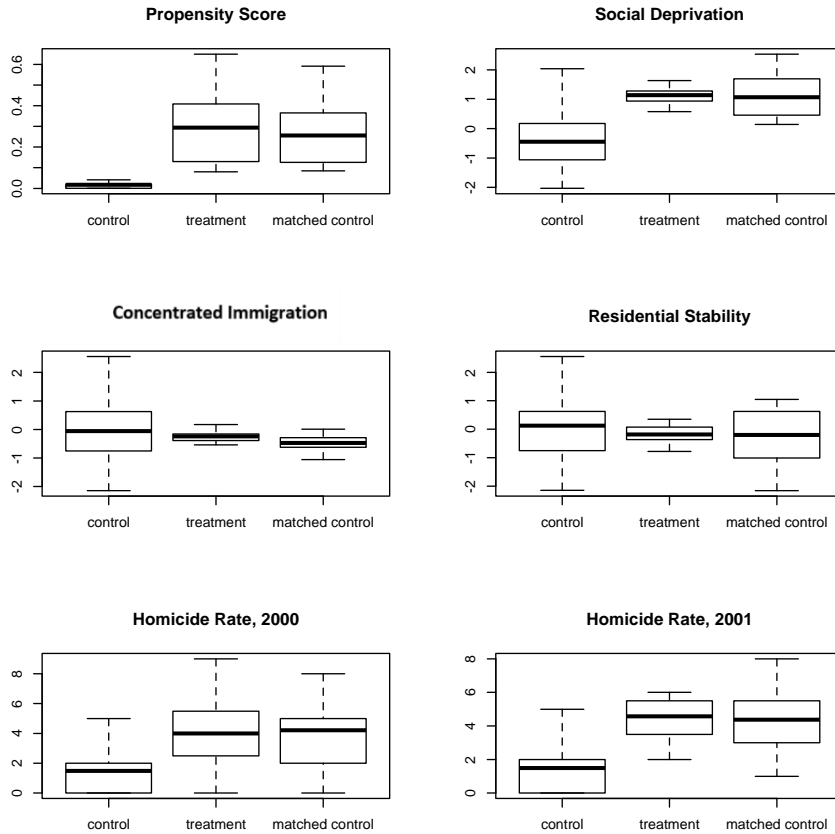
Chicago’s PSN program is one of the longest running and most rigorously studied violence prevention programs. The initial evaluation found statistically significant negative correlations between homicide and the dosage of PSN initiatives in the first few years of their operation. The current study finds similar results for that period, but little evidence of an effect in subsequent years. Something seems to have happened in those early years that is associated with reductions in violence in high-crime communities. We cannot be sure why the dramatic short-term effects did not sustain over time or extend to other areas of the city. But future research would do well to consider the strategies that were effective in the early years and leverage those insights for future programs.

⁷² See, e.g., Sampson & Groves, *supra* note 48, at 780–82.

APPENDIX

Appendix Figure 1

Comparison of Pre-Treatment Covariates for the 11th and 15th Districts



Appendix Figure 2
Comparison of Pre-Treatment Covariates for the 7th and 9th Districts

