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- Provide more informed dialogue about criminal justice policies and practices and the empirical evidence related to these policies and practices
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# EDITORIAL INTRODUCTION

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## PATHWAYS TO PRISON

### Toward a Criminology of Prison Downsizing

**Todd R. Clear**

*Rutgers University*

For the first time in more than a generation, our national conversation about imprisonment policy is changing. Back in the early 1970s, coming partly out of a shared sense of alarm in the aftermath of the Attica prison revolt, an American prison abolition movement took as a point of departure the observation that too many people were in U.S. prisons. This idea might seem quaint now, but it was authoritative in policy circles at the time. The 1973 report of the National Advisory Commission on Criminal Justice Standards and Goals, established by President Richard M. Nixon to advise national policy, envisioned a corrections system in which probation was the sentence of choice, most adults and juveniles accused of crimes were diverted from formal justice processing, and imprisonment was restricted only to the “dangerous” minority of people convicted of crimes.

The irrelevance of the National Advisory Commission and the impotence of prison abolition groups cannot be overstated. The early 1970s was the beginning of a nearly four-decade pattern of growth in the number of people held in U.S. prisons and jails. By the 1980s, a national political chorus for being “tough on crime” drowned out any other point of view, both in the polity and in mainstream criminology. In the 1990s, the escalation in imprisonment became a stable fact of American political life. By 2000, the United States had become the worldwide leader in the size and scope of its incarcerated population.

Today, in stunning and sudden fashion, all of that has changed. A consensus has arisen on the political left and right that America has too many prisoners and that its burgeoning prison population is not good for the nation. The public imagination is now focused on the problem of “mass incarceration,” and the policy agenda entails a search for ways to reduce the number of people behind bars, which is consistent with public safety.

Sarah Tahamont, Shi Yan, Shawn D. Bushway, and Jing Liu (2015, this issue) tackle the need for prison reform by investigating the justice histories of a large sample of people

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who entered the New York prison system for the first time. Studying individuals entering prisons for the first time is interesting because preventing them from going to prison will have a significant impact on the prison population. Among the states, New York has a low rate of imprisonment and has had a declining prison population for several years. By implication, if a set of people can be identified for diversion from New York's prisons, this is surely instructive about prison reform possibilities nationally.

Tahamont et al. (2015) argue that people who enter prison for the first time do so through identifiable "pathways." Some get there because of serious crimes. But for a substantial minority, the first experience of prison follows a long history of involvement with the justice system. This observation suggests that the justice system had many opportunities to engage this group in programs that would have diverted them from continued justice system involvement and eventual incarceration. In a national agenda to decrease the use of incarceration, getting this subgroup into justice system strategies that reduce their patterns of criminality will, according to Tahamont et al., have a high payoff. They note that the other "pathways to prison" might suggest alternative ways of prison diversion.

In their essay responding to Tahamont et al.'s (2015) study, Monteiro and Frost (2015, this issue) argue for an alternative to individually based programmatic strategies for reducing the size of the prison population. By putting their analysis in context of a nation in shock about the racial implications of police and prison policy, they call for a program of social investment in high-crime communities to create the kind of infrastructure that would reduce crime in the first place.

In a recent essay, criminologists Joan Petersilia and Francis Cullen (2015) have called for the development of a "criminology of downsizing." No doubt this is true. No doubt, as well, that Tahamont et al.'s (2015) study and Monteiro and Frost's (2015) policy essay are precisely the kind of criminological enterprise that Petersilia and Cullen have envisioned. Tahamont et al. offer an empirically based exploration of the way individual-level arrest trajectories produce new prison entries. They conclude that different trajectories suggest different policy strategies for prison diversion. Monteiro and Frost (2015) argue for a shift in focus from regulating individuals to investing in communities. As criminologists turn their attention to the problems outside the U.S. prison population, these papers serve as telling examples of the kind of empirical and policy work we can expect to see.

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Tahamont, Sarah, Shi Yan, Shawn D. Bushway, and Jing Liu. 2015. Pathways to prison in New York State. *Criminology & Public Policy*, 14: 431–453.

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**Todd R. Clear** is a distinguished professor at Rutgers University—Newark. He has held professorships at Ball State University, Rutgers University, Florida State University, and John Jay College of Criminal Justice. Prof. Clear has authored 15 books and more than 100 articles and book chapters. His most recent book is *The Punishment Imperative* by NYU Press (October 2013). Clear has also written on correctional classification, prediction methods in correctional programming, community-based correctional methods, intermediate sanctions, and sentencing policy. He was the founding editor of *Criminology & Public Policy*. Clear has served as president of the American Society of Criminology, Academy of Criminal Justice Sciences, and Association of Doctoral Programs in Criminology and Criminal Justice. His work has been recognized through several awards, including those of the American Society of Criminology, the Academy of Criminal Justice Sciences, The Rockefeller School of Public Policy, the American Probation and Parole Association, the American Correctional Association, and the International Community Corrections Association. He is a fellow of the American Society of Criminology.



# Pathways to Prison in New York State

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### Research Summary

*In this study, we use a novel application of group-based trajectory modeling to estimate pathways to prison for a sample of 13,769 first-time prison inmates in New York State. We found that 12% of the sample was heavily involved in the criminal justice system for 10 years prior to their first imprisonment. We also found that less than one quarter of the sample had little contact with the criminal justice system prior to the arrest that resulted in imprisonment.*

### Policy Implications

*Slightly less than one quarter of first-time inmates are not known to the criminal justice system prior to the commitment arrest. For these inmates, crime-prevention interventions that identify participants through criminal justice processes will not be effective. However, the arrest rates for a substantial portion of the sample over the 10-year period before imprisonment suggest a staggering number of opportunities for intervention as these individuals churn through the system.*

**C**haracterizations of the criminal histories of prison inmates commonly neglect the temporal element of past involvement with the criminal justice system, which results in a “flat,” dimensionless picture of criminal justice involvement. For example, in the most recent Bureau of Justice Statistics description of the prison population

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This research was funded by a partnership between the New York State Division of Criminal Justice Services and the University at Albany, SUNY (PI: Shawn Bushway). The authors would like to thank Deputy Commissioner Terry Salo and Research Director Leslie Kellam for their support of this project as well as their insightful comments along the way. Direct correspondence to Sarah Tahamont, Hindelang Criminal Justice Research Center, University at Albany, SUNY, 135 Western Avenue, DR241, Albany, NY 12222 (e-mail: stahamont@albany.edu).

in the United States (Carson, 2014), no attempt was made to describe the criminal history of individuals in prison. By contrast, most of the 19 tables in that report include information about the nature of the crime that resulted in a prison sentence. When descriptions of criminal history are included, they are often limited to the mean and median numbers of arrests and convictions (Durose, Cooper, and Snyder, 2014). Expanding the description of criminal history to include the distribution of criminal justice contact over time adds richness to the criminological understanding of the different pathways of criminal justice involvement that precede a first prison sentence.

In this article, we use methods from life-course criminology to present a sense of the “trajectory,” or path, of arrests that individuals follow up to 10 years prior to their first prison term. We use detailed data on all individuals who received a prison sentence for the first time in New York State from 2009 to 2012. This novel use of trajectory modeling generates two very striking insights. First, almost a quarter of the people who are sentenced to prison for the first time have very little involvement in the criminal justice system in the 10 years prior to their prison sentence. Not surprisingly, these individuals are more likely to be sentenced to prison for serious crimes as their first contact with the criminal justice system. The criminal justice system could have done very little with these individuals directly to prevent the crimes that generate these prison sentences because the criminal justice system had little or no direct involvement in the lives of these individuals prior to the crime that led to the prison sentence. By contrast, 12% of the sample has a consistently high probability of arrest (approximately 50%) in each 6-month period for the entire decade prior to the first prison admission. In other words, although some people experience a sudden increase in involvement with the criminal justice system immediately prior to their first prison admission, a sizeable proportion of the sample has prolonged involvement with the system prior to imprisonment. This latter finding suggests that policies that deflect these individuals during the early stages of their criminal careers might be an additional avenue for efforts to reduce incarceration over and above the current options most often considered by policy makers.<sup>1</sup>

## Literature Review

A substantial amount of work has investigated the predictors of criminal behavior and, by extension, contact with the criminal justice system (Bergman and Andershed, 2009; Bonczar and Beck, 1997; Farrington, Ttofi, and Coid, 2009; Pettit and Western, 2004; Pulkkinen, Lyyra, and Kokko, 2009; Sampson and Laub, 1990). Not surprisingly, the best predictor of future criminal behavior is past behavior, which holds for predicting both self-reported

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1. Most policy suggestions focus on one of three types of solutions: policies that eliminate prison sentences for certain types of crimes (e.g., mandatory minimums), policies that reduce recidivism after reentry for those still sentenced to prison, and social programs that exist outside of the criminal justice system, like education and training programs, that reduce crime in general (Mauer and Ghandnoosh, 2014; National Research Council, 2014).

criminal activity and arrests. This result is also supported by evidence from recidivism studies among samples of individuals who have recently been convicted or incarcerated (Bonta, Harman, Hann, and Cormier, 1996).

However, the question of who will engage in criminal behavior is only partially relevant for the question of what the history looks like for people who are incarcerated for the first time. With the exception of wrongful convictions, criminal behavior precedes a prison sentence. Therefore, the question is not whether those with records are more likely to engage in criminal behavior, but it is what the records look like for those who are ultimately incarcerated. For this question, the relevant literature comes not from life-course criminology but from criminal justice sentencing. The evidence here is unequivocal. Sentencing research has supported the finding that two factors are roughly equally important for sentencing to prison: the type of crime and criminal history (Mitchell, 2005). This basic premise is demonstrated clearly in the theoretical framework undergirding most sentencing research, in which criminal record and type of crime are the only two categories of factors that are “legitimate” considerations for sentencing. In fact, these are the only two categories of factors used in most sentencing guideline grids (Tonry, 1996). An examination of a simple grid like the one in Maryland (Bushway and Piehl, 2001) reveals two basic insights:

- (1) For some very serious crimes, like murder, everyone is sentenced to prison.
- (2) A lengthy criminal history can still result in incarceration for even the least serious felony.

For crimes in the middle of the severity distribution, a nonincarceration sentence is usually possible but only for those individuals with minor criminal histories. This basic pattern is repeated across all sentencing grids with the variation coming from the particular weights assigned to crime severity or criminal history.

A sentencing grid from a guidelines state makes it clear that there are various possible paths into prison, but it tells us little about the actual distribution of paths into prison, in particular, for a sample of first-time prisoners. The problem is more complex in nonguidelines states like New York because the absence of a grid makes the trade-offs between criminal record and crime type less clear.

We are not aware of any sentencing article or study that has examined the probability of incarceration among those who have not yet been incarcerated, nor are we aware of any study that has explicitly examined the pathways into prison for first-time prisoners in either a guidelines or a nonguidelines state. To make progress on the study of pathways into prison, we appeal to methods from the life-course literature in criminology (Farrington, 1986; Farrington et al., 2009; Sampson and Laub, 2003, 2005a). Most of this literature has focused on tracking pathways of criminal behavior, usually starting with a group of younger individuals and tracking their criminal justice contacts as they age (Blokland, Nagin, and Nieuwebeerta, 2005; Sampson and Laub, 2003; Wiesner, Capaldi, and Kim, 2007). For

example, a trajectory analysis of young men using the Oregon Youth Study reported that approximately 70% of the youth in the sample were rarely arrested, whereas approximately 9% of the young men were arrested more than once a year through their early 20s (Wiesner et al., 2007).

Several different “growth curve” models identify trajectories of arrest over time, including the group-based trajectory model (GTM), which allows for different patterns, or “trajectories,” over time (Nagin, 1999; Nagin, Farrington, and Moffitt, 1995). The two statistical approaches used most commonly to study criminal justice involvement throughout the life course in criminology are the standard growth curve model (GCM) and GTM. GCM assumes that there is one basic pathway, but it allows for continuous variation around the pathway. GTM, in contrast, assumes that the continuous distribution can be approximated by a discrete number of fixed points, which together comprise multiple pathways. Both approaches attempt to describe the underlying distribution. The GTM lends itself more easily to the task of creating discrete graphs of different parts of the distribution, parts that might not necessarily follow the path of the overall sample.

The standard GCM is more efficient (i.e., has fewer parameters), but it requires stronger parametric assumptions than the GTM. A priori, it is hard to know whether the GCM framework is flexible enough to accommodate the different shapes available in the distribution. There has been a fairly long discussion in criminology about the merits of GTM and, by comparison, GCM. Much of the controversy about the use of GTM involves concerns that the trajectory groups, which are useful for exploring basic patterns in the data, can then be reified and interpreted as existing prospectively. Critics fear that policy makers will interpret the existence of a “chronic group of offenders” to mean that a real, identifiably distinct group exists comprising individuals who are predetermined to be chronically involved with the criminal justice system (Sampson and Laub, 2005b).<sup>2</sup> This concern, as articulated by Sampson and Laub (2005b), is specific to the application of GTM to prospective data. It is not applicable to our article because our approach is deliberately retrospective. We are starting from a known outcome—an individual’s first known prison term in New York—and then looking backward to understand more clearly the pathways that individuals follow into prison.

We adopt the GTM in this analysis to capitalize on the method’s ability to describe visually the different pathways of arrest leading up to a first prison sentence. Because our focus is on the pathways into prison and not on the offending trajectories for individuals as they age, our application of the GTM also differs from many prior applications in another

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2. This possibility is not implicit in the method—in fact, the basic GTM statistical model does not identify which individuals are in each group, but it provides an estimate of the proportion of the population in each group. Bayesian methods are then required to assign an individual to a group. Although done less often, it is also possible to use Bayesian methods to identify retrospective trajectories or paths for each individual to accompany GCM. Neither method says anything about whether these paths are predetermined (Bushway, Sweeten, and Nieuwbeerta, 2009; Nagin, 2005; Nagin and Tremblay, 2005).

way—the time dimension on the horizontal axis is the number of years before the prison admission rather than age. While this is unusual, nothing is inherent in the method that forces the time dimension of GTM (or GCM) to be age.

## Data and Methods

### *Data*

The data source for this analysis includes all inmates admitted to the New York State Department of Corrections and Community Supervision for the first time between 2009 and 2012 ( $N = 35,635$ ). We used the computerized criminal history (CCH) data maintained by New York State Division of Criminal Justice Services to track these individuals' involvement in the criminal justice system. Knowing that individuals are detained often during case processing prior to prison admission and thereby lose their opportunity for criminal involvement, we started the “look back” from the date of the last arrest prior to prison admission rather than from the date of prison admission itself. A small percentage of individuals ( $n = 2,660$ ) had been last arrested more than 2 years prior to their prison admission date; we excluded these individuals from the analyses, thus, slightly reducing the total sample size ( $N = 32,975$ ).<sup>3</sup>

A reasonable way to start this exercise is to examine the summary statistics for the full sample. Column 1 in Table 1 shows basic demographics and the extent of involvement with the criminal justice system among this cohort of first-time prisoners. These individuals are predominately male (92%) and mostly non-White (69%); 41% had their cases indicted in New York City. On average, they were almost 22 years old at the time of their first adult arrest<sup>4</sup> and almost 28 years old at the last arrest prior to prison admission. The average individual had accumulated 6.3 arrests, including 2.5 felony arrests and 3.7 misdemeanor arrests, and had been convicted 2.4 times but mostly for misdemeanors (1.8 times) prior to the arrest that resulted in a prison sentence. The average individual had also been sentenced to jail once but rarely to probation (0.5 times on average when counting both probation and “split sentences”).<sup>5</sup> It is also worth noting that the standard deviations of the criminal history variables displayed in Table 1 suggest large variations in levels of criminal justice involvement across the sample.

3. Omitting these individuals from the sample does not change the substantive conclusions of the analysis.

4. It is worth noting that the age of majority in New York State is 16 years old, so the adult criminal history data we use in this article tracks criminal history information from the time the individuals are 16 years old forward. Lifetime criminal history counts are calculated from January 1, 1985 to the date of the last arrest prior to the first prison admission for two reasons: first, prison admission data date back to 1985, and second, there are known data quality issues in the early CCH records. As a consequence, these lifetime counts might be underestimated. We do not believe that having data truncated in 1985 is too much of a concern because three quarters of the sample have fewer than 15 years between their first arrest and the last arrest prior to first prison admission, which occurs between 2009 and 2012.

5. In New York, split sentence refers to the sentence that consists of a jail and a probation term.

**T A B L E 1**

**Descriptive Statistics**

Variable	(1) Full Sample	(2) Trajectory Sample
Demographics		
Non-White	0.69 (0.46)	0.63 (0.48)
Male	0.92 (0.28)	0.88 (0.32)
NYC	0.41 (0.49)	0.41 (0.49)
Age at last arrest	27.85 (10.37)	36.56 (9.41)
Age at first lifetime arrest	21.83 (8.39)	26.74 (10.39)
Number of Prior Arrests (1985 and later)		
Any	6.28 (7.03)	7.70 (9.04)
Felony	2.53 (2.84)	2.88 (3.41)
Misdemeanor	3.74 (5.16)	4.81 (6.78)
Number of Prior Arrests (10-year observation period)		
Any	5.18 (5.38)	5.33 (6.30)
Felony	2.15 (2.41)	2.04 (2.62)
Misdemeanor	3.04 (3.83)	3.29 (4.60)
Number of Prior Convictions (1985 and later)		
Any	2.37 (4.23)	3.45 (5.78)
Felony	0.70 (0.85)	0.69 (0.88)
Misdemeanor	1.82 (4.03)	2.82 (5.59)
Number of Prior Convictions (10-year observation period)		
Any	1.57 (3.06)	2.26 (4.00)
Felony	0.40 (0.62)	0.36 (0.60)
Misdemeanor	1.28 (2.91)	1.92 (3.88)
Number of Prior Sentences (1985 and later)		
Jail	0.98 (2.57)	1.45 (3.58)
Time served	0.27 (1.14)	0.42 (1.64)



TABLE 1

Continued

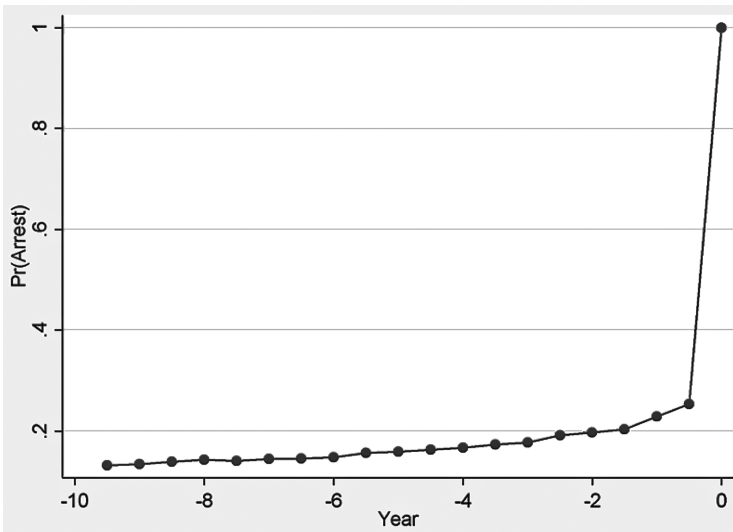
Variable	(1) Full Sample	(2) Trajectory Sample
Probation	0.27 (0.60)	0.40 (0.71)
Jail + probation	0.18 (0.46)	0.22 (0.50)
Number of Prior Sentences (10-year observation period)		
Jail	0.69 (2.04)	1.03 (2.78)
Time served	0.19 (0.67)	0.28 (0.87)
Probation	0.19 (0.48)	0.26 (0.55)
Jail + probation	0.13 (0.39)	0.15 (0.41)
<i>n</i>	32,975	13,769

Although criminal history counts provide an overview of the extent of criminal involvement, they lack a temporal component that describes an individual's path of criminal justice involvement prior to imprisonment. To investigate these pathways for this group of first-time prison inmates, we selected an analytic sample from the full sample ( $n = 13,769$ ) based on two criteria. First, given that the age of majority in New York State is 16 years old, we selected only individuals who were 26 years old or older at their last arrest prior to prison admission to allow for a 10-year "look back" period.<sup>6</sup> Second, we selected only individuals sentenced to prison directly as shown in their CCH records, which was necessary to observe the commitment crime; this variable is essential for the analysis.<sup>7</sup> Column 2 in Table 1 presents the descriptive statistics for the analytic sample. Compared with the full sample, the analytic sample has higher percentages of individuals who are female (12% vs. 8%) and White (37% vs. 31%), which is not surprising because choosing a longer observation period means the sample is necessarily older. In addition, Black males are represented

6. Given that we omit a substantial portion of the overall cohort of first-time prison admissions by imposing a 10-year observation period, we estimated a trajectory model for all individuals 21 years of age and older ( $n = 23,492$ ) examining their arrest patterns in the 5 years prior to prison admission. We found that the substantive conclusions of the 5-year look back are qualitatively similar to the 10-year look back presented in detail in this article. Detailed results for the 5-year look back are available upon request.
7. Of the 32,975 individuals in the full sample, we first excluded 17,887 individuals who were 25 years old or younger at the last arrest, and then we excluded 1,319 individuals who did not have a direct prison sentence shown in the CCH record.

**FIGURE 1**

**Average Probability of Any Arrest Per Semiannual Interval First-Time Prison Admission, 25 Years of Age and Older**



*Note:* The x-axis is standardized so that Year 0 is the date of the last arrest prior to prison admission for all observations.

disproportionately among those who are incarcerated prior to 26 years of age (Steffensmeier, Ulmer, and Kramer, 1998). The analytic sample also tends to have more involvement with the criminal justice system as they have higher counts in most history variables. Again, this can be explained by the fact that cumulative criminal history counts are correlated with opportunity window or time at risk for criminal involvement, which is further correlated with age. The analytic sample includes older individuals who have had a longer time to accumulate a more extensive criminal history.

To introduce a temporal perspective, we plotted arrest rates (average probabilities of any arrest) for individuals in the analytic sample by 6-month intervals across the 10 years prior to their first prison admission. Figure 1 shows the probability of arrest for any felony or misdemeanor for an average individual in the subsample at each of the 20 semiannual intervals during the 10 years prior to the last arrest before prison admission. For the average individual, the probability of arrest increased steadily over time, almost doubling from 13.3% at 10 years prior to 25.4% at 6 months before the arrest immediately preceding prison admission. This pattern suggests that the average person gradually increased his probability of criminal involvement during the years leading up to his first prison sentence.

### *Trajectory Analysis*

These summary statistics, although informative, still omit important information on the heterogeneity of individual patterns of criminal justice contact across the cohort. Assuming that all or even most individuals follow the same pattern risks gross oversimplification and biased conclusions. To accommodate this potential heterogeneity in pathways to prison, we adopted GTM to identify patterns of individuals who follow different paths on their way to prison for the first time.

**Model specification.** We estimated trajectories of arrests during the 10-year period looking backward from the first prison admission (Jones and Nagin, 2012). We used 6-month (semiannual) intervals, resulting in 20 measures over the 10-year period. The variable of interest is whether the individual had any arrest in the given semiannual interval, coded dichotomously. For the arrest trajectories, we estimated a series of cubic logit models:

$$\ln\left(\frac{p_{ijt}}{1 - p_{ijt}}\right) = \beta_{0j} + \beta_{1j}\text{Period}_{it} + \beta_{2j}\text{Period}_{it}^2 + \beta_{3j}\text{Period}_{it}^3 + \varepsilon_{it} \quad (1)$$

where the dependent variable is the log odds of any arrest (the natural log of the ratio between the probability of arrest and the probability of no arrest) for individual  $i$  at time  $t$ .  $\text{Period}_{it}$  denotes the time period, which is the time difference between the observation of the last arrest prior to prison admission, counted in half-years. The letter  $j$  represents the group. We selected the total number of groups  $k$  prior to estimating a model. We first estimated a model with one group ( $k = 1$ ), and then we estimated subsequent models with increasing numbers of groups (two groups, three groups, etc.) until the models no longer converged.<sup>8</sup> We then determined the number of groups that best fits the data by using several model selection criteria.

**Model selection.** We estimated a series of group-based trajectory models, up to 12 groups. In this section on model selection, we focus on statistics for models ranging from four to eight groups. The reason is that the models with more than 10 groups consistently failed to converge, and the models with more than 8 groups, while converging, consisted of several very small groups.<sup>9</sup> The most common model selection statistic for GTM is the Bayesian information criterion (BIC; Nagin, 2005), which measures the degree of model fit. According to Nagin (2005), higher BIC values indicate better model fit. As Table 2 demonstrates, the BIC value increases as the number of groups increases, and the eight-group model has the highest BIC, which seems to have the best model fit.

8. Although the programs for estimating these models provide start values for these equations, it is standard practice to estimate the models with a range of start values. This practice is designed to make sure that the final model obtains the best overall fit and is robust (i.e., not a local maximum). We used a minimum of 10 sets of start values for each  $k$ , and as we narrowed in on the final models, we ran the model with more than 100 different sets of initial start values for the two “competing”  $k$ s (i.e., six and seven group models).

9. The models for groups 1–3 were clearly outclassed by the models with more groups.

**T A B L E 2**

**Model Selection Statistics for the Four- to Eight-Group Models**

Model	Total Number of Groups				
	4	5	6	7	8
BIC	-110,699.74	-110,210.04	-109,748.99	-109,575.87	-109,358.89
AvePP					
Group 1	0.89	0.92	0.87	0.88	0.86
Group 2	0.82	0.70	0.73	0.72	0.61
Group 3	0.88	0.74	0.73	0.73	0.73
Group 4	0.88	0.87	0.76	0.74	0.74
Group 5	—	0.87	0.88	0.83	0.74
Group 6	—	—	0.86	0.82	0.83
Group 7	—	—	—	0.83	0.82
Group 8	—	—	—	—	0.82
OCC					
Group 1	22.04	31.15	21.49	22.56	19.45
Group 2	14.77	14.25	15.54	15.09	15.57
Group 3	14.38	17.90	21.58	21.59	34.25
Group 4	37.73	14.95	50.97	51.11	24.37
Group 5	—	36.11	15.03	12.38	51.51
Group 6	—	—	47.30	24.89	12.24
Group 7	—	—	—	298.19	25.24
Group 8	—	—	—	—	298.96

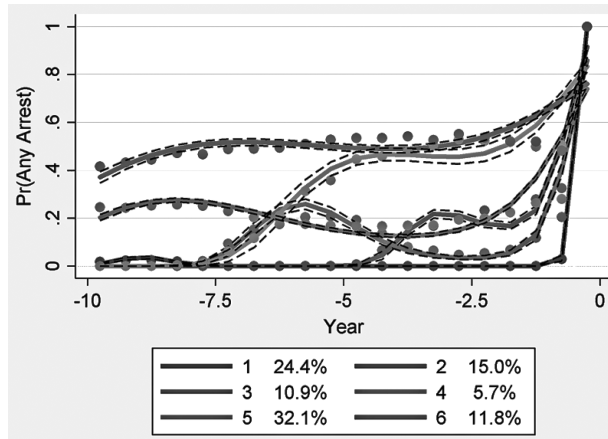
Yet Nagin (2005: 74) cautioned that “BIC does not always cleanly identify a preferred number of groups,” and sometimes “the BIC score continues to increase as more groups are added.” Under such circumstances, additional diagnostic statistics would be necessary to identify the best model. Nagin (2005) suggested two particular group-level diagnostic statistics: the average posterior probability (AvePP) of assignment and the odds of correct classification (OCC).

After the estimation of a model, GTM assigns each individual a set of posterior probabilities of assignment (PPA). PPA describes the probability that the specific individual belongs to each group. We assign each individual to the group for which he or she has the highest PPA (see Nagin, 2005). For each group, AvePP is the mean of the PPAs of the individuals assigned to that group. A high AvePP value indicates that most individuals are assigned to the group without much ambiguity. The rule of thumb is that for a model to be considered adequate, the AvePPs in *all groups* should be higher than 0.70. OCC measures the ratio of two odds: the odds based on maximum probability classification (AvePP) and the odds based on estimated population base rate by random assignment ( $\pi_j$ ):

$$OCC_j = \frac{\frac{AvePP_j}{1-AvePP_j}}{\frac{\pi_j}{1-\pi_j}} \tag{2}$$

FIGURE 2

## Trajectories Estimated in the Six-Group Model



*Note:* The x-axis is standardized so that Year 0 is the date of the last arrest prior to prison admission for all observations.

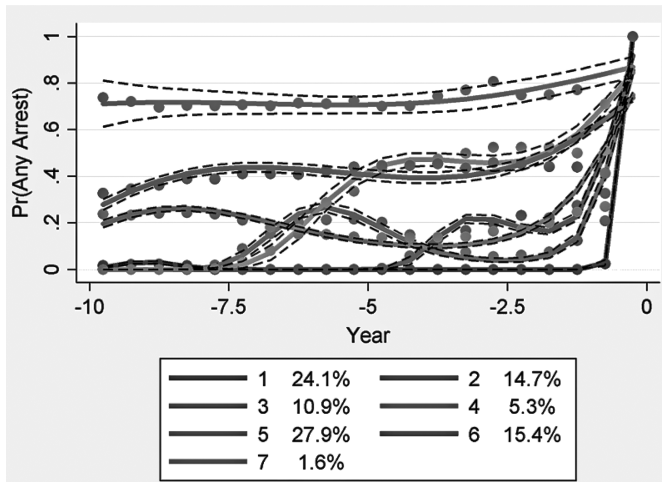
A high OCC value indicates that most individuals are assigned to the group correctly. The OCC should be above five for *all groups* for a model to be considered adequate.

Table 2 also presents the diagnostic statistics for all models. Given that all OCC values presented are satisfactory (greater than five), OCC does not help us differentiate among the models. However, we notice that in the eight-group model, Group 2 has an insufficient AvePP of 0.61, indicating that the assignment of cases to that group is not sufficiently clear-cut. As a result, we decided to discard the eight-group model and narrowed our choice to the six-group and seven-group models, as shown in Figures 2 and 3.

Figures 2 and 3 show that most of the basic pathways identified in these two models are very similar. The only meaningful difference lies in the groups with the highest arrest probabilities. The six-group model identifies a group with high probabilities of arrest (Group 6,  $p \approx 0.50$ ) for the entire period, whereas the seven-group model identifies one group with extremely high arrest probabilities (Group 7,  $p \approx 0.70$ ) and one group with arrest probabilities that are slightly lower but still high (Group 6,  $p \approx 0.40$ ). Although the seven-group model has a higher BIC, we focus on the six-group model for two reasons. First, the size of the extremely high-probability-of-arrest group is small (1.6% of the entire sample). Second, for policy purposes, a constant arrest probability of 0.50 every 6 months for an entire decade already indicates substantial involvement in criminal activities. Breaking this group into two adds very little to the substantive takeaway of identifying this high-contact group. This decision echoes Nagin's (2005: 77, emphasis added) recommendation to "select a model with no more groups than is *necessary to communicate the distinct features* of the data."

**FIGURE 3**

**Trajectories Estimated in the Seven-Group Model**



*Note:* The x-axis is standardized so that Year 0 is the date of the last arrest prior to prison admission for all observations.

**TABLE 3**

**Parameters of the Six-Group Model**

Group	Intercept	Linear	Quadratic	Cubic	Estimated Membership Proportion	Assigned Membership Proportion
Group 1	-18.99	-21.50	21.58	29.81	24.44	26.92
Group 2	-7.31	39.54	-82.94	54.80	15.04	17.42
Group 3	-1.42	-4.04	-7.90	16.56	10.91	9.30
Group 4	-0.27	1.77	-6.73	7.31	5.70	5.44
Group 5	-1.77	-1.28	1.79	2.82	32.12	29.73
Group 6	-0.02	-0.27	0.37	1.28	11.78	11.18

**Results**

Table 3 presents the parameters of the six-group model. In addition to the intercept and linear to cubic coefficients, Table 3 also presents the group membership percentages estimated by the model, as well as the group membership percentages calculated by using the number of individuals actually assigned to the group based on PPA. These are quite close, suggesting once again that a model with six groups is a good-fitting representation of the data.

Substantively, the six-group model presents several interesting and distinctive pathways. The most important result is that the average curve displayed in Figure 1 obscures a great

**TABLE 4****Percentage Distribution of Commitment Crime at Arrest and Conviction for the Six Groups Estimated by the GTM**

<b>Variable</b>	<b>1</b>	<b>2</b>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>Total</b>
<b>Commitment Crime at Arrest</b>							
Murder/rape	11.36	6.13	5.39	3.47	4.42	3.18	6.49
Other violent	31.27	22.72	21.55	26.03	21.99	25.06	25.14
Property	17.40	22.55	19.20	26.57	21.60	25.00	21.06
Drug	25.95	30.72	27.32	30.57	28.76	33.38	28.83
Weapons	5.18	7.54	7.65	6.01	7.31	7.86	6.80
Driving/other public order	1.73	5.63	13.43	4.14	11.70	3.18	6.75
Other/unknown	7.12	4.71	5.46	3.20	4.23	2.34	4.94
<b>Commitment Crime at Conviction</b>							
Murder/rape	9.06	4.42	4.14	2.14	3.42	2.47	5.00
Other violent	32.94	23.76	21.70	28.57	22.09	25.19	25.96
Property	18.10	23.84	19.91	25.37	22.04	25.97	21.72
Drug	25.55	30.64	26.54	30.04	28.12	32.60	28.32
Weapons	6.10	8.00	8.35	7.34	8.43	8.83	7.71
Driving/other public order	1.13	5.29	13.04	4.67	11.87	2.86	6.54
Other/unknown	7.12	4.04	6.32	1.87	4.03	2.08	4.74
<b>Conviction Class of Commitment Crime</b>							
Class A felony	11.06	4.50	4.61	1.20	3.49	1.69	5.48
Class B felony	31.37	23.88	22.25	22.83	20.43	22.66	24.53
Class C felony	18.40	19.67	17.17	17.09	15.83	15.52	17.35
Class D felony	29.43	33.81	33.96	33.91	35.70	36.36	33.50
Class E felony	9.63	17.97	21.94	24.70	24.41	23.51	18.99
Misdemeanor	0.11	0.17	0.08	0.27	0.15	0.26	0.15

deal of heterogeneity in the distribution of criminal justice contacts during the observation period. The story in Figure 1 is of a gradually increasing probability of criminal justice contact, increasing from a 13% chance of an arrest in the 10th year prior to the arrest that leads to imprisonment to a 23% chance of arrest in the 6 months before the arrest that leads to the first prison commitment. Figure 2 demonstrates that the story of gradual increase in criminal justice involvement prior to a first prison sentence is misleading. Clearly, there are several very different pathways into prison.

For example, Figure 2 reveals three trajectories that experience very little change during the entire period prior to the arrest that leads to prison. Trajectory 1, which represents 24.4% of the population, follows a pathway that is solidly at zero for the entire 10 years prior to the last arrest before prison admission. Table 4 shows the distribution of the types of charges associated with the commitment arrest and conviction for each trajectory as well as

the distribution of felony and misdemeanor classes for the arrest and conviction charges.<sup>10</sup> Not surprisingly, many individuals assigned to Trajectory 1 were sentenced to prison for a fairly serious crime (Bushway and Piehl, 2001); more than 42% were convicted of a Class A or B felony compared with less than 31% Class A or B felony convictions in any of the other trajectories. Compared with individuals in other trajectory groups, the individuals in Trajectory 1 are much more likely to have been convicted of a violent crime. Approximately 42% of individuals assigned to Trajectory 1 were convicted of a violent crime compared with fewer than 31% for any of the other trajectories. Of the violent crimes, Trajectory 1 also has the highest concentration of individuals convicted of murder or rape at 9% compared with the other trajectories in which less than 5% were convicted of murder or rape.

By contrast, 11.8% of the population is assigned to Trajectory 6, which has an essentially constant rate of criminal justice contact of 50% in any given 6-month period for the entire decade preceding the instant arrest. By looking at their prior contacts with the criminal justice system, as shown in Table 5, individuals assigned to this trajectory accrue an average of 17 arrests during this time period, which is a surprisingly high total. Fifty-eight percent of individuals assigned to Trajectory 6 were convicted of drug and property crimes, which are crime types associated with high rate criminal behavior (Piquero, 2000). Trajectory 5, which represents 32.1% of the population, also has a constant rate of criminal justice contact throughout the decade preceding their first prison term, hovering around 20% probability of arrest in each of the 6 months over the entire period. Individuals assigned to Trajectory 5 accrue an average of six arrests during the time period; although this total is not as dramatic as the number of arrests for individuals assigned to Trajectory 6, it still suggests a relatively high level of consistent criminal justice involvement for a long period of time. Taken together, Trajectories 5 and 6 document that almost 45% of the population have elevated levels of arrest risk for the entire period prior to their arrest. It is also the case that the convictions in these two trajectories are concentrated among lower level felonies; 60% of the convictions for individuals assigned to Trajectories 5 and 6 were for D or E felonies.

The remaining three trajectories (2, 3, and 4) show patterns of change over time. The smallest group, Trajectory 4 with 5.7% of the population, starts low but then accelerates quickly approximately 8 years before the incident that leads to incarceration. In the 5 years before their imprisonment, these individuals resemble the highest group, Trajectory 6. Like Trajectory 6, Trajectory 4 has a relatively high concentration of drug convictions (30%) for the commitment crime. Although the pattern of criminal justice contacts over the

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10. We refer to the event (arrest, conviction, etc.) that results in the prison sentence as the "commitment" event. It is not always the case that the last arrest preceding prison admission is the commitment arrest; 30.9% of individuals had one or more arrests after the commitment arrest. This finding is not surprising given that often multiple arrest charges are disposed together. Less than 1% of individuals in the sample had an arrest after the disposition date for the commitment offense.



TABLE 5

## Descriptive Statistics for the Six Groups Estimated by the GTM

Variable	1	2	3	4	5	6
Demographics						
Non-White	0.61 (0.49)	0.59 (0.49)	0.55 (0.50)	0.71 (0.45)	0.61 (0.49)	0.76 (0.43)
Male	0.88 (0.33)	0.87 (0.34)	0.87 (0.33)	0.88 (0.32)	0.90 (0.31)	0.91 (0.28)
NYC	0.44 (0.50)	0.41 (0.49)	0.30 (0.46)	0.53 (0.50)	0.36 (0.48)	0.56 (0.50)
Age at last arrest	39.55 (10.72)	36.03 (8.90)	37.78 (9.40)	33.73 (7.59)	35.43 (8.42)	33.60 (7.97)
Age at first lifetime arrest	34.95 (11.61)	28.88 (9.00)	25.63 (8.46)	23.58 (6.48)	21.61 (6.54)	19.81 (5.24)
Number of Prior Arrests (1985 and later)						
Any	1.25 (2.61)	4.63 (4.23)	5.18 (3.79)	12.77 (7.48)	9.59 (5.78)	22.55 (14.03)
Felony	0.61 (1.32)	1.96 (2.04)	2.13 (1.95)	4.38 (3.25)	3.71 (2.90)	7.51 (4.92)
Misdemeanor	0.65 (1.85)	2.68 (3.18)	3.06 (2.83)	8.39 (6.25)	5.89 (4.31)	15.05 (12.10)
Number of Prior Arrests (10-year observation period)						
Any	0.50 (1.16)	3.32 (2.83)	3.04 (2.03)	10.52 (6.05)	6.22 (3.31)	17.07 (8.28)
Felony	0.30 (0.94)	1.47 (1.66)	1.38 (1.42)	3.61 (2.87)	2.48 (2.04)	5.70 (3.85)
Misdemeanor	0.20 (0.59)	1.84 (2.16)	1.66 (1.50)	6.91 (5.12)	3.74 (2.53)	11.37 (6.98)
Number of Prior Convictions (1985 and later)						
Any	0.46 (1.31)	1.99 (2.47)	2.29 (2.12)	6.06 (5.82)	3.96 (3.42)	11.21 (12.00)
Felony	0.19 (0.50)	0.58 (0.74)	0.73 (0.83)	0.91 (0.90)	1.00 (0.96)	1.14 (0.96)
Misdemeanor	0.29 (1.14)	1.44 (2.35)	1.60 (1.88)	5.20 (5.78)	3.08 (3.26)	10.21 (11.95)
Number of Prior Convictions (10-year observation period)						
Any	0.07 (0.32)	1.10 (1.67)	1.20 (1.11)	4.59 (4.89)	2.47 (2.02)	8.52 (7.67)
Felony	0.03 (0.18)	0.24 (0.47)	0.39 (0.57)	0.51 (0.63)	0.57 (0.71)	0.68 (0.72)
Misdemeanor	0.05 (0.26)	0.86 (1.60)	0.81 (1.02)	4.09 (4.85)	1.94 (1.94)	7.89 (7.67)
Number of Prior Sentences (1985 and later)						
Jail	0.11 (0.69)	0.67 (1.49)	0.62 (1.14)	3.04 (4.24)	1.38 (2.13)	6.01 (7.74)
Time served	0.03 (0.26)	0.22 (0.67)	0.16 (0.46)	0.75 (1.33)	0.40 (0.88)	1.78 (4.20)

(Continued)

**TABLE 5**

**Continued**

Variable	1	2	3	4	5	6
Probation	0.09 (0.34)	0.27 (0.55)	0.49 (0.72)	0.50 (0.82)	0.62 (0.83)	0.60 (0.88)
Jail + probation	0.05 (0.25)	0.16 (0.43)	0.27 (0.52)	0.26 (0.53)	0.34 (0.59)	0.35 (0.63)
Number of Prior Sentences (10-year observation period)						
Jail	0.02 (0.16)	0.38 (1.09)	0.31 (0.66)	2.42 (3.69)	0.87 (1.35)	4.81 (6.02)
Time served	0.00 (0.08)	0.13 (0.50)	0.08 (0.31)	0.58 (1.10)	0.24 (0.61)	1.31 (1.81)
Probation	0.02 (0.14)	0.17 (0.43)	0.29 (0.53)	0.36 (0.65)	0.42 (0.66)	0.42 (0.71)
Jail + probation	0.01 (0.13)	0.10 (0.35)	0.18 (0.42)	0.19 (0.45)	0.24 (0.49)	0.27 (0.56)
<i>n</i>	3,707	2,399	1,281	749	4,093	1,540

observation period for individuals in Trajectory 4 is different than the constant patterns indicated for Trajectories 5 and 6, the three trajectories (4, 5, and 6) have nearly identical distributions with respect to the relative severity of commitment crime at conviction. Approximately one quarter of individuals assigned to Trajectories 4, 5, and 6 were sentenced to prison for a Class A or B felony, just more than one half were sentenced to prison for a Class C or D felony, and the remaining one quarter were toward the bottom of the severity distribution with convictions for E felonies.

By contrast, Trajectory 2, representing 15.0% of the population, does not start to increase in probability of arrest until 5 years preceding imprisonment, and although this group has a nonzero probability of arrest for those 5 years before incarceration, they never reach higher than a 20% chance of an arrest in any given 6-month period. Finally, Trajectory 3, representing 10.9% of the population, accelerates at the same time as Trajectory 4, approximately 8 years prior to the arrest that precedes imprisonment, but after reaching a peak arrest probability approximately 6 years prior to imprisonment, this group seems to dip down to lower levels of criminal justice involvement. Indeed, Trajectory 3 is close to the “zero” group, Trajectory 1, during the 3 years prior to the last arrest before prison admission. Trajectory 3 has the highest concentration of prison sentences for convictions of driving-related and other public-order crimes (13%). Taken together, the trajectories reveal an underlying distribution of pathways into prison that is much richer and more dynamic than would be imagined if we were limited to the story provided by the average path shown in Figure 1.

## Policy Implications

Several interesting findings arise from examining the pathways of criminal justice contact prior to imprisonment in this way. Two findings have particular policy relevance. First, nearly one quarter of the distribution of first-time prisoners is almost entirely unknown to the criminal justice system prior to the arrest that results in a prison sentence. As Table 5 shows, this group of prisoners tends to be older both at the time of arrest for the crime that results in their first prison sentence (39.6 years old, standard deviation [ $SD$ ] = 10.7 years) and at first lifetime arrest (35.0 years old,  $SD$  = 11.6 years). The other characteristic that distinguishes individuals assigned to this trajectory is that, as we noted previously, the distribution of crimes that result in their prison sentences is concentrated among violent crimes, which is to be expected given the fact that they were sentenced to prison with little or no criminal history. These individuals accrued an average of 0.5 arrests ( $SD$  = 1.16) and 0.07 convictions ( $SD$  = 0.32) during the 10-year observation period. As a matter of policy, although other social institutions might interact with these individuals throughout their lives with the potential to divert them from the crimes that result in imprisonment, there does not seem to be a way for the criminal justice system to divert these individuals because, for the most part, they are not known to the criminal justice system prior to the arrests that result in their imprisonment.

By contrast, there is an even greater proportion of the distribution that has sustained contact with the criminal justice system for the entire decade before the first prison admission. Approximately one third of the distribution has around 20% probability of arrest every 6-month period prior to the arrest that results in the first prison incarceration. The trajectory models predict that these individuals get arrested approximately at least once every 2.5 years. The criminal justice system has even more contact with 12% of the distribution, who have approximately 50% probability of arrest in every 6-month period prior to the arrest that results in a prison sentence. The trajectory models predict that these individuals get arrested approximately once a year for the entire decade, but in reality the average number of arrests is even higher. On average, these individuals accrue 17.1 arrests ( $SD$  = 8.28) during the 10-year observation period. During the course of the available lifetime criminal history for these individuals, they are arrested 22.6 times ( $SD$  = 14.03) on average. For both these groups, the criminal justice system has many potential opportunities to divert these medium- and high-contact individuals from criminal activity during the decade that precedes their first imprisonment and during their lifetime.

Determining *how* to divert these individuals from their criminal activities is a question as old as criminology. Although we cannot offer specific policy recommendations, we believe it is worthwhile to make some observations about the way the criminal justice system has responded to the arrests that high-contact individuals accrue during the decade prior to imprisonment. First, although the high-contact individuals are accruing several arrests during the decade prior to imprisonment, they have only about half as many convictions as

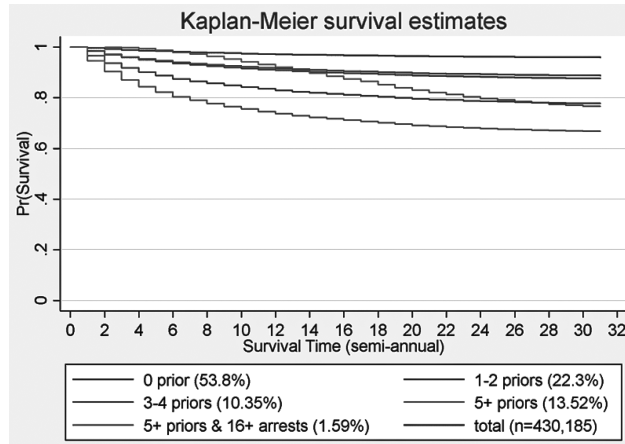
arrests. Individuals assigned to the high-contact trajectory, Trajectory 6, have 8.5 convictions ( $SD = 7.67$ ) during the 10-year observation period and 11.2 ( $SD = 12$ ) in their lifetime. Given that the sample consists only of first-time prison inmates, almost all of those convictions are for misdemeanors (7.9,  $SD = 7.67$  over the 10-year observation; 10.2,  $SD = 11.95$  lifetime). When the high-contact individuals are convicted, more than 70% of the time they are sentenced to jail or released at the time of disposition with a sentenced of time served in jail. By contrast, high-contact individuals are sentenced to probation only 5.4% of the time.<sup>11</sup> In thinking about the potential for intervention with these high-contact individuals, this very low potential for community supervision seems noteworthy. This is not to say that all individuals on probation are being heavily supervised as the conditions of probation and levels of supervision vary both by jurisdiction and by predicted risk-level. However, individuals sentenced to jail or released with time served have absolutely no supervision post-release.<sup>12</sup>

Our heavy focus on the high-contact individuals among a group of first-time prison inmates might raise the concern that this is an exceptional group of outliers who can avoid imprisonment for such a long period of time prior to incarceration.<sup>13</sup> To address this concern, we turned to a cohort of individuals arrested in 1997. Figure 4 shows a survival curve for the 1997 arrest cohort; the curve starts at 100% where all arrestees are free in the first semiannual period in 1997. The curve then follows these individuals through time; individuals are removed from the curve if they are admitted to prison during a given semiannual period, and they are not returned to the curve even if they are released during the time period. Among the 430,185 arrestees in the 1997 arrest cohort, 11.2% are imprisoned during the 15-year period between 1997 and 2012. The overall survival curve for the arrest cohort is fairly flat. However, because so many individuals in a given arrest cohort are arrested one time and then never again, we estimate separate curves to distinguish criminal justice involvement among the arrest cohort. As shown in Figure 4, individuals with no arrests prior to 1997<sup>14</sup> have a 96% survival rate through 2012; by contrast, individuals who had five or more arrests before 1997 had a 66.7% survival rate between 1997 and 2012. Having 5 or more arrests prior to 1997 does not necessarily indicate that an individual will continue to have sustained contact with the criminal justice system during the entire period

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11. The remaining dispositions consist of some form of conditional discharge (16.2%), fines (2.2%), or unconditional discharge or conviction with no sentence (0.3%).
  12. Combined jail plus probation sentences are available, but they are relatively rare (3.2% of dispositions).
  13. As noted by Canela-Cacho, Blumstein, and Cohen (1997), individuals with high rates of criminal behavior will, based on stochastic processes, eventually be incarcerated because they have the most opportunities to be arrested. There might be a concern that this group of high-contact individuals is just a small remainder of the total population of high-rate individuals who have, for the most part, already been incarcerated during the 10-year period.
  14. For the survival analysis, we measured arrest history between 1990 and the first arrest in 1997 because recent involvement with the criminal justice system (during a period of up to 7 years) was sufficient to identify heterogeneity in the frequency of criminal justice contact among the arrest cohort.

FIGURE 4

## Survival Estimates for the 1997 Arrest Cohort



between 1997 and 2012; we also identified a group of individuals with more than 5 arrests prior to 1997 and 16 or more arrests during the observation period. This group has a similar level of criminal justice contact to the individuals assigned to the high-contact Trajectory 6, and actually had a higher survival rate (76.5%) during the period between 1997 and 2012.<sup>15</sup> This group is not precisely the same group as the individuals assigned to the high-contact trajectory, but these individuals have similarly sustained contact with the criminal justice system for a long period of time. Although very high-contact individuals represent a small portion of the arrest cohort, most of them “survive” the observation period of the survival analysis without being sentenced to prison. We feel confident that the high-contact individuals assigned to Trajectory 6 are not the rare residual who managed to avoid prison.

With such high rates of arrest, and thus, contact with these individuals, there are many opportunities to implement crime-prevention interventions. Furthermore, it is possible to identify high-contact individuals (at least within some margin) by using only prior criminal justice contact as a predictor. Almost half of the individuals in the survival analysis sample who had accrued 5 or more arrests before 1997 went on to accrue an additional 5 or more arrests during the 15-year observation period, and approximately one quarter of those

15. Given that the individuals in the trajectory analysis have been admitted to prison, they are a much more selected sample than the individuals who are not incarcerated from the arrest cohort. As a consequence, even though they have similar rates of criminal justice contact, we do not anticipate that they will be very similar based on demographic characteristics. Indeed, the survival analysis sample of individuals who have more than 5 arrests prior to 1997 and more than 16 arrests over the 15-year observation period are less likely to be male and less likely to be White than the high-contact individuals in the trajectory sample.

individuals accrued 10 or more arrests during that same period. From a policy perspective, when attempting to identify candidates for crime-prevention interventions, it is perhaps more appealing to use criminal history as opposed to demographic predictors, which might raise ethical concerns when used to target individuals for interventions a priori (DeLisi, 2001; Gottfredson and Moriarty, 2006).

## Conclusion

Our novel application of GTM, looking back at a group of individuals who were admitted to prison for the first time, has demonstrated that individuals follow several different pathways of criminal justice contact prior to a first prison sentence, some of which involve constant rates of criminal justice contact (or, in one case, noncontact) during a sustained period of time (Trajectories 1, 5, and 6), and others suggest patterns of change over time (Trajectories 2, 3, and 4). Exploring heterogeneous patterns of criminal justice contact preceding a first prison sentence is a policy-relevant exercise because it suggests different approaches for diverting individuals from future criminal behavior. Nearly one quarter of those individuals sentenced to prison for the first time is almost unknown to the criminal justice system prior to the arrest that results in their prison sentence. That said, a substantial portion of the individuals sentenced to prison for the first time have sustained contact with the criminal justice system for several years before they are sentenced to prison for the first time. Approximately 18% of those sentenced to prison for the first time have a 50% probability of an arrest for the 5 years preceding the last arrest before their prison sentence; 12% of those individuals sustain that level of criminal justice contact for the entire decade before prison admission. Although these individuals accrue a large number of arrests, they are only convicted approximately half of the time, and when they are convicted, generally they are sentenced to jail or released with a sentence of time served in jail. Although it is not the point of this article to detail *how* to divert these individuals from future criminal activity, our results do suggest that current criminal justice practices in New York result in churning many individuals through the system in a sustained cycle of arrest, conviction, and jail time. Each of these criminal justice contacts represents an opportunity to intervene and potentially to reduce the flow of individuals into prison in the future.

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# Altering Trajectories Through Community-Based Justice Reinvestment

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Tahamont, Yan, Bushway, and Liu (2015, this issue) use group-based trajectory modeling to identify trajectories of groups of offenders incarcerated for the first time at age 26 or older. Although they find many different pathways to prison, the two key insights to be gleaned from this work are that (a) a relatively small group of offenders (approximately 12%) has many contacts with the criminal justice system prior to first incarceration and (b) a somewhat larger group (around 24%) has very little criminal justice system exposure prior to first incarceration. Although identifying pathways that different groups follow prior to their first imprisonment is important for building our understanding of the dynamics of incarceration and its potential effects, in a sense, there is nothing particularly startling about what Tahamont et al. (2015) have found. In the early 1980s, similar insights about offending trajectories drove arguments for the selective incapacitation approach (Greenwood and Abrahamse, 1982) and the subsequent backlash against it (Auerhahn, 2001; Gottfredson and Hirschi, 1986; Miles and Ludwig, 2007). The argument back then: If only we could identify those chronic, repeat offenders—the career criminals—really early, then we could selectively target them for early intervention, or enhanced supervision, or—if we got really good at identifying the most active 5% to 6%—incapacitation through incarceration. The problem is those trajectories are always easier to identify in hindsight and for types (or groups) of individuals rather than for actual individuals.

Our science of prediction is imprecise at best, and predictive instruments rely heavily on what have long been the two best predictors of risk for future criminal behavior: criminal history and age. Not surprisingly, these two covariates are highlighted as particularly important in these analyses as well. As group-based trajectory modeling cannot be used to identify prospectively who will end up in any particular group, the policy implications of this

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work are decidedly less than obvious. Indeed, Tahamont et al. (2015) offer no specific policy recommendations, instead they offer some general observations related to the potential of crime prevention approaches for different types of trajectories.

In a more policy-oriented footnote, Tahamont et al. (2015) allude to the relatively recent efforts to reduce our reliance on incarceration noting that strategies offered for reducing prison populations in the United States have tended to take one of three approaches:

- (1) Restricting the use of incarceration through eliminating mandatory sentences and mandatory minimums
- (2) Reducing recidivism through targeted programming for incarcerated offenders
- (3) Reducing crime through investment in social programs

The most explicit policy recommendation in the article appears when the authors make passing reference to a fourth type of policy choice noting that: “policies that deflect these individuals during the early stages of their criminal careers might be an additional avenue for efforts to reduce incarceration over and above the current options most often considered by policy makers.” The four approaches for reducing our reliance on incarceration that they mention neglect one of the most important paradigm shifts in decades. That shift—toward a new vision of community-based justice reinvestment—relies on several relatively noncontroversial facts.

America’s four-decade experiment in increasingly harsh penal policy has resulted in incarceration rates that are more than five times higher than they were in the 1970s. Reducing our reliance on incarceration will require recognizing that these incredibly high incarceration rates are not a historic accident but are the result of deliberate policy choices (Clear and Frost, 2014). The size of a prison population depends entirely on how many people go to prison and on how long they stay there, and across all of the states, we have witnessed increases on both sides of that incarceration equation (Frost, 2008). Get-tough policies and mandatory sentences ensured that more and more people would be sentenced to prison, whereas mandatory minimums, truth-in-sentencing laws, and three-strikes—like habitual offender statutes guaranteed that they would stay much longer once they got there. Zero-tolerance policies turned our nation’s schools into pipelines to our nation’s prisons, particularly in the most troubled schools serving the most disadvantaged communities (Heitzeg, 2014). The collateral consequences of felony convictions have guaranteed that those who come into contact with the criminal justice system will continue to feel the effects long after they have done their time and served their judicially imposed sentences (Mauer and Chesney-Lind, 2003). The punitive policies heralded from the 1970s through the 1990s and into the 2000s are expensive, have been largely ineffective, and have exacerbated racial disparities in criminal justice outcomes (Tonry, 2011). Perhaps most importantly, one does not need group-based trajectory modeling to know that the trajectory for many residents of America’s most disadvantaged communities overwhelmingly leads to prison.

In the midst of protests and riots surrounding police–community race relations across the country (most recently in Ferguson, Missouri, and West Baltimore, Maryland), we use this policy essay to make an argument for a wholly different type of policy approach—one that recognizes a related, yet too-often neglected, set of trajectories that lead from our nation’s most disadvantaged neighborhoods, to our nation’s most underperforming schools, and through that pipeline to our nation’s overpopulated prisons. As the indictments against police officers involved in the arrest and subsequent death of Freddie Gray were announced in early May 2015, former resident John Blake wrote an editorial about his old West Baltimore neighborhood for CNN (Blake, 2015). Having noted the absence of older male role models, he asked a question that scholars of penology have been asking for some time now: “What happens when [Black men] disappear from an entire community?” (Blake, 2015, para. 8). As he walked the streets, Blake talked with residents of his old neighborhood:

I asked 28-year-old Zachary Lewis about the absence of older men. He stood by a makeshift memorial placed at the spot where Freddie Gray, the man whose death ignited the riots, was arrested. “This is old here,” he said, pointing to himself. “There ain’t no more ‘Old Heads’ anymore, where you been? They got big numbers or they in pine boxes.” In street syntax, that meant long prison sentences or death. (Blake, 2015, para. 6–7)

About his old West Baltimore neighborhood, Blake noted the following:

Something else was missing when I returned: places for kids to play or meet the men who could mentor them. Baltimore is a sports-crazy town. I grew up playing Little League baseball, running around the track at the high school across the street from my home, and playing tennis at public courts scattered through West Baltimore. There were public swimming pools, pickup basketball games, and plenty of recreation centers. On some days, I barely ate because I spent so much time outside playing sports. Yet when I returned to my old playing fields, they were overgrown with weeds or barred with locked gates. I heard the same story from residents. The city had closed the pools, removed the basketball goals and, as recently as 2013, closed 20 recreation centers. I didn’t see any kids playing baseball or football in the streets. “They’ve taken the city away from us. We have nowhere to go and nothing to do,” says Grant, the young man who wants to be a role model. (Blake, 2015, para. 34–37)

Blake’s editorial drives home the following point: Those neighborhoods hardest hit by crime have only been further damaged by policies that have made the criminal justice system the most prominent institution in the community and prison a near-ubiquitous experience in the lives of young, mostly Black and Brown, males (Western, 2006).

According to a recent Justice Policy Institute report, at least \$5 million taxpayer dollars are spent annually to incarcerate people from each of the 25 high incarceration

communities in Baltimore City (Petteruti, Kajstura, Schindler, Wagner, and Ziedenberg, 2015). In the highest incarceration community, Baltimore spent more than 17 million dollars on incarceration. Perhaps not surprisingly, these Baltimore communities with the highest incarceration rates were also the places with the greatest disadvantage as indicated by lower life expectancy rates, higher percentages of households receiving public assistance, lower school attendance rates and educational attainment levels, and disproportionately high levels of unemployment. It seems likely that, absent daring and deliberate criminal justice policy reform, these communities will suffer the consequences of mass incarceration for decades to come.

It seems in many ways counterintuitive to suggest that a reduced reliance on incarceration might make our communities safer. But in reality, most of the communities in which Americans live are relatively safe, and those communities that are hardest hit by crime tend to be the communities hardest hit by incarceration. Disadvantaged communities have long suffered the consequences of crime policies that both overpolice their neighborhoods and overincarcerate their residents. For almost two decades now, criminologist Todd Clear has argued that incarceration policies intended to control crime might backfire and actually increase crime (Clear 1996, 2007). It is now well documented that disadvantage concentrates in a few inner-city communities and that those communities contribute more than their fair share of offenders to prisons (Rose and Clear, 2004). During the past several decades, Clear and colleagues have focused on this movement of offenders back and forth between prisons and communities and have set out to demonstrate that this coercive form of mobility could result in more, not less, disadvantage and that such disadvantage would lead to more, not less, crime (Rose and Clear, 1998). In their earliest work testing the “coercive mobility thesis” in Tallahassee, Florida, Clear, Rose, Waring, and Scully (2003) demonstrated that incarceration served to reduce crime to a certain extent, but in the most disadvantaged communities, the level of incarceration eventually reached a tipping point—a point at which each additional incarceration actually appeared to increase crime. In other words, they uncovered a tipping point at which incarceration backfired as a crime-prevention approach.

In a much more recent test of the thesis using data from the city of Boston, Clear et al. (2014) divided Boston neighborhoods based on the level of concentrated disadvantage. Their research showed that prison cycling (the churning of offenders into prison from communities and back again) has different effects in different kinds of neighborhoods, which is consistent with the idea of a “tipping point” but is more clearly expressed as an interaction between crime policy and type of neighborhood. In the more advantaged Boston neighborhoods, no evidence was found that prison cycling was damaging; indeed, cycling offenders in and out of prison from such communities appeared to reduce the crime rate in those places. However, in Boston neighborhoods with higher than average concentrated disadvantage, prison cycling increases the rate of crime in the community. In sum, it seems that in more advantaged communities, incarceration works to reduce crime, but in more

disadvantaged neighborhoods, incarceration backfires and increases crime. Importantly, rates of prison cycling in Boston communities overlap far more with rates of disadvantage than they do with rates of crime. Findings like these give justice reinvestment approaches a competitive edge over other crime-control approaches.

Justice reinvestment approaches are designed to reorient crime policy toward reducing the reliance on incarceration by keeping offenders in the community at a substantial cost savings and then reinvesting the money saved into the communities hit the hardest by crime (and criminal justice intervention). Justice reinvestment approaches that reduce incarceration and actually reinvest in communities have the potential to bring about both short-term and long-term crime-prevention benefits. In the short term, a reduced reliance on incarceration has the potential to prevent further damage to already disadvantaged neighborhoods; in the long term, true justice reinvestment approaches will begin to rebuild those communities in ways that ameliorate some of the disadvantage and make a more limited use of incarceration more effective.

One of the most obvious places to begin investing justice savings would be in the schools of the neighborhoods from which large numbers of offenders are drawn. For years, assessments have identified a culture of failure among schools in disadvantaged communities with dismal school conditions marked by large achievement gaps. One defining characteristic of schools serving high-poverty communities is the proliferation of zero-tolerance policies. The growing literature on the school-to-prison pipeline provides an important context for understanding mass incarceration. Zero-tolerance policies are fast-tracking students toward a different set of trajectories, where imprisonment is a likely outcome. One national report noted that out-of-school suspensions in schools across the country nearly doubled, rising from 1.7 million in 1974 to approximately 3.3 million in 2006 (Planty et al., 2009). The policy of forcing students out of school for deviant, but not criminal, behavior is short sighted considering how this ultimately has the potential to alter students' trajectories. Although many factors contribute to dropout rates, several studies have noted that dropout rates are highest among the students who were suspended repeatedly throughout their middle- and high-school tenures (American Psychological Association Zero Tolerance Task Force, 2008; Heitzeg, 2014; Miller et al., 2011; Rodriguez, 2013). Students removed through out-of-school suspensions or expulsions fall behind academically, and those experiencing repeated suspensions and expulsions are at greater risk of dropping out of school.

Concerns about the impact of zero-tolerance policies have been highlighted in studies that linked the deleterious effect of school suspension as a predictor of involvement in the juvenile and criminal justice systems (Costenbader and Markson, 1998). In Florida, for example, it was reported that more than 80% of the students referred to the juvenile justice system had at least one record of in-school or out-of-school suspension (Florida Department of Juvenile Justice, 2013; Rodriguez, 2013). Above-average school dropout rates, high suspension and expulsion rates, disproportionate numbers of students on special education tracks, and below-average rates of academic achievement (measured by graduation

and grade promotion) have turned the public schools in our poorest communities into pipelines to prisons.

After cataloguing the various disinvestments in the West Baltimore community that he grew up in, John Blake noted:

Yet there is one institution the city seems to find money to invest in, some residents say: law enforcement. Funding for public schools, libraries, jobs programs and recreation centers may lag, but the budget for jails and police never seems to run dry, Walter Boyd and others say.

Some wonder if it's deliberate.

"If you don't invest in them now, you're just going to have to build more prisons," Boyd says about kids in West Baltimore. "And that just seems like that's what the plan is. They won't educate you. But they'll incarcerate you in a minute." (Blake, 2015, para. 48–50)

Although reinvesting in schools and community recreation areas may not be an all-encompassing solution, these investments offer some of the more promising approaches for altering the trajectories of youth residing in our most disadvantaged communities. For too many, by the time they are identified as at risk for incarceration, it is likely too late to alter their trajectory.

Community-based justice reinvestment is a vision not yet fully realized. Although the U.S. Department of Justice continues to promote justice reinvestment approaches through its Justice Reinvestment Initiative, unfortunately many of these federally funded efforts have not remained true to the original "reinvestment" conception (Frost, Monteiro, and Strah, 2015). Most of the justice reinvestment demonstration projects have taken the savings from reduced use of incarceration and have "reinvested" them in other agencies of correctional control—most notably, probation. When Susan Tucker and Eric Cadora first proposed the justice reinvestment approach, they were describing a much bigger idea—a type of reinvestment that would double as a community-development strategy and prevent crime in the long term (Tucker and Cadora, 2003). Some of the savings associated with diverting people from prisons to communities must go toward paying for supervising those offenders in their communities, but most of the money diverted from corrections should go to rebuilding infrastructure, attracting businesses, investing in schools, building playgrounds, and increasing a community's capacity for informal social control.

Criminal justice policy reform is inherently risky, especially for those holding elected office, because the potential for things to go wrong is relatively high. Nonetheless, serious efforts to decrease our reliance on incarceration will by necessity involve an approach that is simultaneously bold and risky (Clear, 2011). The most promising way to do this is to adopt an approach that will not only reduce our reliance on incarceration in the short term but also will offer demonstrable crime-prevention benefits in the long term. Community-based



justice reinvestment strategies, starting with reinvestments in the schools and recreational areas in the communities hardest hit by crime and incarceration, are among the most viable strategies for doing that.

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# EDITORIAL INTRODUCTION

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## FOCUSED DETERRENCE IN NEW ORLEANS

### Focused Deterrence and the Promise of Fair and Effective Policing

**Anthony A. Braga**

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When any part of the American family does not feel like it is being treated fairly, that's a problem for all of us. . . . It's not just a problem for some. It's not just a problem for a particular community or a particular demographic. It means that we are not as strong as a country as we can be. And when applied to the criminal justice system, it means we're not as effective in fighting crime as we could be. (Remarks made by President Barack Obama when establishing the Task Force on 21st Century Policing, December 18, 2014)

The act of policing communities, especially minority communities, always involves a delicate balance (Meares and Kahan, 1998). On the one hand, research has suggested that the police benefit from the general willingness of community members to cooperate with them to report crimes, identify criminals, assist in investigations, and address conditions that might facilitate crime (Moore, 1992; Reisig, 2010; Tyler and Fagan, 2008). On the other hand, research also has suggested that effective policing invariably involves tactics that bring the police into close and regular contact with community residents. This contact can be viewed by community residents, particularly minority residents, as intrusive and unwarranted, leading citizens to doubt whether the police respect their rights or care about their well-being (Brunson and Miller, 2006; Brunson and Weitzer, 2009; Carr, Napolitano, and Keating, 2007). Despite whether individuals have personal contact with police officers, their perceptions of the legitimacy of police have important consequences for police effectiveness (Tyler, 2004, 2006). Policing is far more difficult without the support of the public. Therefore, police effectiveness is powerfully influenced by the consequences of different tactical and policy choices for their legitimacy.

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Among the members of the communities involved, police legitimacy is regarded as the appropriate role that police play in making and implementing rules governing public conduct (Sunshine and Tyler, 2003; Tyler and Huo, 2002). Although many factors influence police legitimacy (e.g., Bottoms and Tankebe, 2012), police departments need to develop, implement, and sustain crime-control practices that are both fair and effective. As suggested by the National Research Council's Committee to Review Research on Police Policy and Practices, "policing that is perceived as just is more effective in fostering a law-abiding society, and that success in reducing crime enhances police legitimacy" (Skogan and Frydl, 2004: 2). More recently, the President's Task Force on 21st Century Policing (2015) noted that, "law enforcement's obligation is not only to reduce crime but also to do so fairly while protecting the rights of citizens" (p. 42).

Focused deterrence strategies represent a relatively new crime-reduction approach that holds great promise in reducing serious violence while improving strained relationships between minority neighborhoods and the police departments that serve them (Kennedy, 2011). Focused deterrence strategies honor core deterrence ideas, such as increasing risks faced by offenders, while finding new and creative ways to deploy traditional and nontraditional law-enforcement tools to do so, such as directly communicating incentives and disincentives to targeted offenders (Kennedy, 1998, 2008). Corsaro and Engel (2015, this issue) show that the focused deterrence approach can be used to good effect in reducing serious gang violence in New Orleans, a city known for persistently high levels of violence, concentrated disadvantage, police misconduct, and political corruption. The New Orleans evaluation joins a growing body of rigorous scientific evidence that has suggested these strategies indeed generate noteworthy crime-prevention gains (Braga and Weisburd, 2012; Land, 2015, this issue).

Focused deterrence programs, such as the New Orleans Group Violence Reduction Strategy, seem to be well positioned to be regarded as fair and just crime-reduction approaches (Brunson, 2015, this issue). First, community leaders, social service providers, and others are engaged in the planning, design, and execution of these violence-prevention initiatives. Collaborative partnerships between police and community members improve the transparency of law-enforcement actions and provide residents with a much-needed voice in crime-prevention work. Second, by using analysis to identify the gangs and other criminally active groups central to violence, these programs are highly focused on very risky people rather than on subjecting uninvolved individuals to indiscriminate enforcement. Third, during "call-in" communication sessions, targeted individuals are warned of the consequences associated with continued violent behavior and advised to take advantage of the services and opportunities being offered to them. In the eyes of community members, there is an inherent fairness in offering targeted offenders a choice and in providing resources to support their transition away from violent behavior rather than simply arresting and prosecuting them.

Fourth, focused deterrence takes advantage of recent theorizing regarding procedural justice and legitimacy (Braga, 2012). Studies have suggested that when procedural justice approaches are used by the police, not only will citizens evaluate the legitimacy of the police more highly, but also they will be more likely to obey the law in the future (e.g., Paternoster, Brame, Bachman, and Sherman, 1997). Advocates of focused deterrence strategies argue that targeted offenders should be treated with respect and dignity, reflecting procedural justice principles (Kennedy, 2011). The Chicago Project Safe Neighborhoods strategy, for instance, sought to increase the likelihood that the offenders would “buy in” and voluntarily comply with the prosocial, antiviolence norms being advocated by interacting with offenders in ways that enhance procedural justice in their communication sessions (Papachristos, Meares, and Fagan, 2007).

Effective violence reduction requires proactive law-enforcement actions to address the high-risk people and high-risk places that generate the bulk of urban violent crime problems. However, there is no legitimate reason why police departments cannot be proactive while being fair and respecting the rights of citizens. The New Orleans experience described by Corsaro and Engel (2015, this issue) represents an important addition to our knowledge base on police programs and policies that seem to strike the important balance between fairness and effectiveness.

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### Most Challenging of Contexts

### Assessing the Impact of Focused Deterrence on Serious Violence in New Orleans

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#### **Research Summary**

*The use of focused deterrence to reduce lethal violence driven by gangs and groups of chronic offenders has continued to expand since the initial Boston Ceasefire intervention in the 1990s, where prior evaluations have shown relatively consistent promise in terms of violence reduction. This study focuses on the capacity of focused deterrence to impact lethal violence in a chronic and high-trajectory homicide setting: New Orleans, Louisiana. Using a two-phase analytical design, our evaluation of the Group Violence Reduction Strategy (GVRS) observed the following findings: (a) GVRS team members in the City of New Orleans closely followed model implementation; (b) homicides in New Orleans experienced a statistically significant reduction above and beyond changes observed in comparable lethally violent cities; (c) the greatest changes in targeted outcomes were observed in gang homicides, young Black male homicides, and firearms violence; and (d) the decline in targeted violence corresponded with the*

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*implementation of the pulling levers notification meetings. Moreover, the observed reduction in crime outcomes was not empirically associated with a complementary violence-reduction strategy that was simultaneously implemented in a small geographic area within the city.*

### **Policy Implications**

*The findings presented in this article demonstrate that focused deterrence holds considerable promise as a violence prevention approach in urban contexts with persistent histories of lethal violence, heightened disadvantage, and undermined police (and institutional) legitimacy. The development of a multiagency task force, combined with unwavering political support from the highest levels of government within the city, were likely linked to high programmatic fidelity. Organizationally, the development of a program manager and intelligence analyst, along with the use of detailed problem analyses and the integration of research, assisted the New Orleans working group in identifying the highest risk groups of violent offenders to target for the GVRS notification sessions. The impacts on targeted violence were robust and consistent with the timing of the intervention.*

“(In 2004), researchers conducted an experiment in which police fired 700 blank rounds in a New Orleans neighborhood in a single afternoon. No one reported the gunfire.” (Associated Press, 2005, para. 1)

**T**he City of New Orleans has a rich history with unique cultural traditions (Hirsch and Logsdon, 1992). Although perhaps best known for its blend of Creole, French, and African cultures that influences its music, cuisine, activities, and celebrations, it is also well known as a city of misfortune, ravished by Hurricane Katrina in 2005, enduring decades of political corruption, police misconduct, poverty, and persistently high levels of lethal violence (Moore, 2010). The city demonstrates both the best and the worst of urban America, and it is where the most innovative criminal justice reform efforts are most needed but are least likely to succeed. Within this challenging context, city and public officials adopted a violence-reduction strategy to combat persistent patterns of lethal gun violence.

Since the early 1990s, the national urban homicide rate in the United States has hovered between 13.0 and 20.0 per 100,000 in large U.S. cities with a population of 250,000 and greater, and approximately between 8.0 and 10.0 in all U.S. cities with 100,000 or more residents (Uniform Crime Reports, 2013). Based on Uniform Crime Reports homicide data for the years 2008–2012, New Orleans averaged 55 homicides per 100,000 residents. Additionally, since the mid-1970s, New Orleans has maintained a citywide homicide rate trajectory that is significantly higher than 90% of all U.S. cities (McCall, Land, and Parker, 2011). Certainly, the underlying factors that contribute to lethal violence within New

Orleans have been lasting—and require an innovative approach to disrupt the stability in the city’s persistently high homicide levels. As such, city officials implemented the Group Violence Reduction Strategy (GVRS) to address the conditions and dynamics that generate the ongoing criminal homicide problem.

The New Orleans GVRS drew on the focused deterrence framework (Kennedy, 1997, 2009a) and operated through interagency partnerships to impact persistent citywide patterns in violence by using data-driven approaches (i.e., homicide incident reviews and gang audits) to identify key offenders (specifically gangs and criminally active groups) responsible for a disproportionate share of the city’s serious violence. Officials in New Orleans attempted to increase the perceived risk of apprehension as well as the consequences leveraged against gangs and criminally active groups through offender notification strategies backed up by credible enforcement actions. The approach was specifically designed to halt ongoing violent behavior by the most violent gangs and groups in the city. In the current study, we conduct an empirical evaluation of the New Orleans GVRS first implemented by the City of New Orleans and the New Orleans Police Department (NOPD) in 2012.

This article proceeds as follows: First, we review briefly the principles that guide focused deterrence violence-reduction strategies and the body of evidence that demonstrates their potential effectiveness. Second, we discuss the distinctive context of New Orleans by outlining its unique challenges and describe the problem analyses used by the local criminal justice working group to identify which gangs and groups drive lethal violence within the city. We likewise document the specific tactics that were used to enhance the perceived risk of future sanctions against such gangs and groups. Third, we present a two-phase quasi-experimental design to examine the potential impact of the New Orleans GVRS. Despite the challenging context presented in the City of New Orleans, the findings of this study demonstrate that the focused deterrence initiative corresponded with statistically significant reductions in targeted violence, which likewise corresponded with the timing of the intervention. Finally, we conclude with a discussion of the importance of these findings for both research and practice. This study adds to the growing body of literature that shows focused deterrence has the potential to impact persistent violence in some of the most challenging urban contexts.

## Literature Review

A deterrence-based theoretical framework posits that three primary components deter high-risk individuals from engaging in future patterns of offending: the certainty, severity, and celerity of punishment (Cook, 1980; Gibbs, 1975; Nagin, 1998; Paternoster, 1987; Zimring and Hawkins, 1973). Based on an extensive review of both classic and contemporary studies that assessed the various dimensions of deterrence, Nagin (2013) noted that there is little evidence that severity-based deterrence approaches (e.g., life without parole or “three-strikes” laws) are effective (see also Durlauf and Nagin, 2011); however, the evidence in support of deterrent effects related to the certainty of punishment is far more consistent. Moreover,

Nagin contended that most compelling deterrent effects are seemingly linked to the police generating an increased *certainty of apprehension*, which can be accomplished in one of two ways:

1. By apprehending the suspect
2. By creating the perception that apprehension risk is sufficiently high

A promising deterrence-based crime prevention initiative, pioneered in Boston during the 1990s, was the “pulling levers” violence-reduction strategy. The well-known Operation Ceasefire initiative was developed in an effort to increase the risks of criminal justice sanctions faced by active, chronic offenders (Braga, Kennedy, Waring, and Piehl, 2001; Kennedy, 1997, 2009a). The cornerstone of the pulling levers focused deterrence strategy is to communicate incentives and disincentives directly to targeted chronic violent offenders to curb illicit and violent behavior and to obtain positive crime control gains. To change the perception of apprehension risk, highly active and violent street gangs (and other criminally active groups) are summoned by the police to “call-in” sessions to make them aware of the specific penalties that will be leveraged against each individual associated with the group if any member continues to engage in serious violence after the notification session.

Deterrence in these types of interventions is most likely achieved by identifying and delivering a message of swiftness and certainty of apprehension (and punishment) to groups of chronically violent offenders who are responsible for most of the city’s crime problems. Ultimately, practitioners across various criminal justice agencies work together to convey that violence will no longer be tolerated and that further violations will be followed with legal, but harsh, sanctions available when future violence occurs (Kennedy, 1997, 2009a). High-risk groups of offenders are susceptible to coordinated criminal justice responses such as implementing strict probation and parole enforcement, shutting down otherwise nonviolent sources of income (e.g., gambling houses), paying attention to low-level street crimes such as public intoxication, and ensuring direct and nonlenient prosecutorial attention.

Focused deterrence is rooted in the problem-oriented policing framework. This facilitates the customization of the focused deterrence approach to local conditions. Problem-oriented policing is a highly focused law enforcement approach that is designed to assess, recognize, and disrupt the underlying causes behind chronic crime problems (Goldstein, 1979). In a review of the most effective police approaches to crime prevention, the National Academy of Sciences (2004) described the problem-oriented policing model as follows:

The heart of problem-oriented policing is that this concept calls on police to analyze problems, which can include learning more about victims as well as offenders, and to consider carefully why they came together where they did. The interconnectedness of person, place, and seemingly unrelated events needs to be examined and documented. Then police are to craft responses that may go beyond traditional police practices. (National Research Council, 2004: 91)

What is perhaps most significant, from an organizational standpoint, is that the problem-oriented policing model (including focused deterrence) is achieved by (a) diagnosing local problems, (b) using research to inform the problem analysis, (c) developing interagency partnerships to address local crime problems, and (d) customizing the operational strategy locally to build long-term capacity. Indeed, the problem-oriented policing model has shown considerable impact on targeted crime problems (National Research Council, 2004; Weisburd, Telep, Hinkle, and Eck, 2010).

It is also worth noting that the notification sessions are held publicly in crime-stricken communities (e.g., neighborhood churches) to illustrate a collective public response to the violence (see Kennedy, 2009a). An emerging body of research has framed the use of offender notification meetings as a way to enhance the perceived legitimacy of the criminal justice system by providing an unbiased and procedurally just response to violence by involving multiple members from the criminal justice agency, citizens, and clergy from high-risk communities in the “deterrence-based” (i.e., threat of enhanced sanctions) notification processes (see Brunson, Braga, Hureau, and Pegram, 2013; Papachristos, Meares, and Fagan, 2007; Wallace, Papachristos, Meares, and Fagan, in press). Thus, various sound theoretical perspectives and organizational approaches guide focused deterrence in real-world settings.

### *Prior Evaluations of Focused Deterrence Strategies*

In field settings that have focused on group and gang violence, reductions in the levels of citywide and/or community violent crime, homicide, and gang-related (or group-related) violence have been observed in Boston (Braga et al., 2001; Piehl, Cooper, Braga, and Kennedy, 2003), Chicago (Papachristos et al., 2007), Cincinnati (Engel, Tillyer, and Corsaro, 2013), Lowell (Braga, Pierce, McDevitt, Bond, and Cronin, 2008), Indianapolis (McGarrell, Chermak, Wilson, and Corsaro, 2006), and Los Angeles (Tita et al., 2004), and Stockton (Braga, 2008). Similar crime-prevention benefits have been documented in urban settings attempting to decrease drug-market-related neighborhood crime when drawing on the focused deterrence model (Corsaro, 2013; Corsaro, Brunson, and McGarrell, 2010; Corsaro, Hunt, Hipple, and McGarrell, 2012; Saunders, Lundberg, Braga, Ridgeway, and Miles, 2014). The timing of the broader reduction in violence in each of these high-risk urban settings typically corresponds with the onset of programmatic implementation. In a recently completed, Campbell systematic review and meta-analysis of focused deterrence programs, Braga and Weisburd (2012: 341) reported that these interventions were associated with an overall effect size on crime outcomes that was generally between .47 and .61, which is consistent with a medium (or moderate) standardized effect size (Cohen, 1988).<sup>1</sup>

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1. The studies described here employed at least a quasi-experimental (sometimes with case-control matching) design to rule out (where possible) extraneous influences on targeted crime outcomes (Braga and Weisburd, 2012). Sherman et al. (1998) documented a process by which evaluation rigor can be measured, which is referred to as the “scientific methods scale” (or SMS) that ranges from 1 to 5 (low

More recent advancements in the literature have begun to illustrate the mechanisms by which focused deterrence approaches potentially impact aggregate crime rates. Indeed if the strategy is true to form, then groups and gangs who receive a threat of enhanced sanctions combined with social service provisions should experience appreciable declines in offending post implementation (i.e., post call-in). When the Boston Ceasefire strategy was reinstated in the late 2000s after a period of discontinuation from the initial intervention (in the 1990s), research found that gangs called into notification sessions were significantly less likely to generate shootings and to be victimized by firearms when compared with highly comparable (matched) control gangs (Braga, Hureau, and Papachristos, 2014). Additionally, gangs in Boston that were associated with treated gangs (i.e., socially networked), but were not direct recipients of the call-in sessions, also experienced significantly fewer shootings than match-control gangs (Braga, Apel, and Welsh, 2013). A similar study in Chicago showed that gang factions who attended a call-in experienced a 23% reduction in overall shooting and a 32% reduction in firearm victimization in the year that followed the treatment (Papachristos and Kirk, 2015, this issue).

In terms of individual-level studies, a cross-national implementation of the strategy in Glasgow likewise showed evidence that gang-involved youths from treatment neighborhoods were significantly less violent than nontreated gang-involved youths from comparably economically distressed areas (Williams, Currie, Linden, and Donnelly, 2014). Within the United States, research (again from Chicago) showed that individuals who attended focused deterrence notification sessions had a lower recidivism rate, across multiple re-offense categories, when compared with offenders from the same neighborhoods who were not identified and selected to attend call-ins (Wallace et al., in press). The literature has thus illustrated that where focused deterrence strategies are implemented with a high degree of fidelity, aggregate rates of violence often experience significant and sizeable declines; more recent studies have highlighted that the crime reduction benefits are driven by reductions in offending by the very groups and gangs as well as the individuals who receive the treatment (both direct and indirect recipients of the notification sessions). Based on this research foundation, problem analyses within New Orleans suggested that focused deterrence would be a viable intervention strategy because detailed problem analyses illustrated that a discrete number of groups and gangs was responsible for driving disproportionately high rates of lethal violence within the city.

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to high). Studies with multiple units that receive or do not receive the program and attempt to control for other factors (i.e., a classic quasi-experimental design) or that use time-series methods are coded with a score of "3" in the SMS scale. More rigorous studies that use multiple cases and controls and include matching are coded as a "4" on the SMS scale, whereas a score of "5" is reserved for randomized controlled trials. Only evaluations that are graded with a score of "3" or higher are typically included in rigorous systematic reviews (see also Braga and Weisburd, 2014).



## **GVRs in New Orleans**

Practitioners often insist that the unique local context of *their* city, political environment, violence problem, level of resources, neighborhoods, citizens, and so on would prevent the successful implementation of violence-reduction initiatives that have demonstrated success in other jurisdictions. New Orleans certainly presented such a challenge. The legitimacy of the city government of New Orleans, and the NOPD specifically, has previously been called into question (Moore, 2010). Unfortunately, prior allegations of corruption, misconduct, and abuse of force have continued to plague the city. For example, in July 2014, former New Orleans Mayor Ray Nagin was sentenced to 10 years in prison for various acts of bribery and corruption while in office. Likewise, based on a 10-month investigation by the Department of Justice (DOJ) and a subsequent scathing written report documenting unconstitutional conduct by the NOPD, in 2012 the City of New Orleans, the NOPD, and the DOJ entered into the “nation’s most expansive Consent Decree” in an effort to force sweeping department-wide reform.<sup>2</sup> Despite recent reform efforts, the perceived legitimacy of the NOPD remains a challenge as scandals of police misconduct continue to be exposed (e.g., a damaging report released in November 2014 documented extensive failures in reporting and investigations conducted by the NOPD Special Victims Section). These scandals led to a continual questioning of the legitimacy of the NOPD (Tyler, 1990).

Despite such difficulties, between 2010 and 2012 specifically, government officials in New Orleans including the Mayor’s Office, the NOPD, federal and local prosecutors, and federal law enforcement drew on promising strategies such as the GVRs and the Project Safe Neighborhoods (comprehensive gun violence-reduction approach—see McGarrell, Corsaro, Hipple, and Bynum, 2010) to build the interorganizational capacity necessary for strategic implementation. The city also supplemented the GVRs with the *CURE Violence* model (formerly *CeaseFire Chicago*) as part of the broader *NOLA for Life* murder reduction strategy (Skogan, Hartnett, Bump, and Dubois, 2009), which relied on violence interrupters and outreach workers to mediate conflicts between conflicting groups within the Central City area (City of New Orleans, 2013). Thus, the city relied on both a multiagency and a comprehensive problem-solving framework to address the persistent citywide patterns in violence.

As part of the GVRs problem identification phase (with a specific focus on the causes and correlates of lethal violence), law enforcement officials in New Orleans partnered with researchers to conduct a series of homicide incident reviews (the first beginning in June 2012) as well as gang audits to identify potential groups most prone to violence across the different police districts within the city. The incident reviews included NOPD officers; Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF) special agents; researchers from the Institute of Crime Science at the University of Cincinnati; and members of the

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2. For more details, see [nola.gov](http://nola.gov).

Cincinnati Police Department who had extensive experience with gang audits.<sup>3</sup> Information about violent street gangs was converted into actionable intelligence by organizing it along the following dimensions: (a) individual gang members, (b) geography, (c) social networks, and (d) participation in violence.

The working group identified 59 potential street gangs in six of the seven police districts within the city. Officials estimated that there were approximately 600–700 individual members within these gangs. Social network analyses indicated that a handful of gangs were diverse and at risk for violence via their social networks (such as active feuds or alliances with other gangs). Social network and geographic analyses supported officers' descriptions of the changing nature of groups and gangs in New Orleans. Officers described these groups as less likely to be hierarchical, intergenerational, structured gangs, and more likely to be loosely knit with continual changes in membership and affiliations. In addition, officers suggested that the structure and territorial nature of violent groups and gangs changed dramatically after Hurricane Katrina because of the displacement of low-income residents (see also Kirk, 2009) and the subsequent rebuilding phase, which separated group and gang members and disrupted traditional territorial boundaries (where applicable).

Loose gang network structures and corresponding definitional issues have previously served as a challenge within the City of New Orleans. For example, in a DOJ-sponsored review of homicides from 2009 to 2010, Wellford, Bond, and Goodison (2011) found that only 1.0% to 2.5% of all homicides in the city were officially classified as gang related. Wellford et al. (2011: 12) cited cases in which the gang units specified that small groups (three or four individuals) of unorganized young men often identified with geographic areas where they lived committed high levels of violence within the city; however, because they were not in "structured" and formalized "gangs" the NOPD at that time did not define such activity as gang related. In their conclusion, Wellford et al. suggested the use of homicide incident reviews would likely better unravel the network of loosely structured groups of offenders. In the homicide reviews conducted in preparation for the GVRs, the research team and NOPD officials placed a greater emphasis on identifying the loosely affiliated networks of offenders engaging in violence. Based on this more in-depth and comprehensive review, the current investigation identified 54.3% of all lethal incidents between January 1, 2010 through March 31, 2014 as group or gang member involved

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3. The Cincinnati Initiative to Reduce Violence was a replication of the Boston Ceasefire strategy that began in 2007. Key officials from the enforcement team from the Cincinnati Police Department worked with the University of Cincinnati research team and the NOPD to explain their previous organizational experiences with the Cincinnati Initiative to Reduce Violence (see Engel, Baker, Tillyer, Eck, and Dunham, 2008), conduct several gang audits, and lead their homicide incident reviews. Additionally, David Kennedy and members from the National Network for Safe Communities played a key role by providing project oversight and guidance throughout the various stages of the strategy to the NOPD and City of New Orleans officials.

(GMI), indicating that the incident involved a group or gang member as a victim, suspect, or both.<sup>4</sup>

As part of the GVRs, officers within the NOPD suggested that the structure of groups and gangs in New Orleans was more fluid and less geographically based than in the past. Similar descriptions have been observed regarding the changing nature of group and gang affiliations in other cities as well (Engel et al., 2013; Kennedy, 2009b). Law enforcement officials most familiar with these groups and gangs provided detailed feedback to the criminal justice working group about gang participation in violence leading to a continually updated list of potential gangs to include in the call-ins. The identified (mostly loosely structured) gangs became the focus of a multipronged approach that included law enforcement, the threat of enhanced prosecution, and the use of social services. During the course of the strategy evaluated in this study, the NOPD conducted five separate offender notification sessions to deliver antiviolence messages to offenders associated with problematic gangs that were incarcerated or were on probation or parole (between October 2012 and March 2014). During these combined sessions, 158 individuals (representing 54 high-violence gangs) directly received communication that enhanced sanctions would follow any involvement in violence, and the notified offenders were asked to disseminate the message to other members. More specifically, the notified group and gang members were warned that the next murder or shooting committed by any individual associated with the notified gang would result in immediate and enhanced law enforcement scrutiny of the entire group for any criminal activity. Illustrations of previous gangs that were apprehended and facing rigid federal and state prison terms were shared to underscore the seriousness of the message.

In terms of organizational structure, a multiagency law enforcement task force (including local and federal partners) was created to track gang violence, review data sources and intelligence, and build criminal cases on violent gang members. Two newly created positions (i.e., program manager and criminal intelligence analyst) helped provide direction. In addition to the 158 individuals who attended the call-ins, six individuals were visited by police and received a personalized antiviolence message, which was referred to as a “custom notification” session (see Kennedy and Friedrich, 2014). Social service provisions were

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4. GMI homicides were classified by the NOPD and researchers during the homicide incident reviews (see Azrael, Braga, and O'Brien, 2013). The name of the victim and suspect(s) (if known) as well as the totality of situational homicide characteristics were considered. Such characteristics included the following: location of the offense, suspected involvement of the victim in illicit acts preceding the homicide, manner and type of death, known characteristics of the victim, time of day, likely suspects, and other relevant characteristics of the incident. If the totality of the circumstances suggests that group members were involved in the incident, then it was coded as a GMI homicide unless evidence existed to the contrary. Each case was reviewed retrospectively for proper GMI determination when additional suspect information was gathered. The same team of NOPD officers and researchers was responsible for the final GMI classification of all homicides examined in these analyses; therefore, there are no concerns regarding coder inter-rater reliability.

presented to those individuals in attendance at the call-in sessions; 59 of the 158 individuals (37.3%) signed up for some type of social services, although only 25 of the 59 individuals actually participated in or received such services.<sup>5</sup>

To summarize, the process (or implementation) of the GVRS strategy in New Orleans is consistent with the overall model of the focused deterrence framework adopted in other cities (Braga and Weisburd, 2012; Kennedy, 2009a). Specifically, high-risk groups and gangs were identified through problem analyses, notified of future sanctions in call-in sessions, subjected to enhanced enforcement actions when antiviolence rules were broken, and provided access to social service opportunities.

### **Assessment of Impact: Two-Phase Analytical Approach**

Assessing the potential impact of the New Orleans focused deterrence strategy presents a unique challenge because the intervention was implemented throughout all areas in the city that experienced high homicide and persistent gun-violence problems. Given the political and social climate that led to programmatic implementation, prioritization was placed on identifying and addressing citywide violence rather than on optimizing the evaluation design. We thus employ a two-phase methodological design. First, we compare the relative homicide rate change in New Orleans with cities that have been classified as having the most volatile and stable homicide rates (see McCall et al., 2011). Next, we examine changes in targeted violence within New Orleans by employing a standard interrupted time-series design that compares multiple targeted crime outcomes (i.e., homicides, gun homicides, gang homicides, and firearm assaults) in the postintervention period relative to preintervention trends after controlling consistent shocks and drifts in the longitudinal data to better isolate potential programmatic effects within the city (Cook and Campbell, 1979). We also include a comparative time-series analysis on nontargeted outcomes (i.e., overall violent crimes, property crimes, and nongang homicides) to assess whether the potential changes in targeted outcomes correspond with a more general trend in crime within the city. Finally, we include a series of sensitivity and placebo tests in both analytical phases and control for the potential influence of simultaneous strategies (i.e., *CURE Violence*) within New Orleans to rule out, where possible, the impact of confounding influences on the outcomes examined.

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5. We note that the current study does not attempt to disentangle the various potential intervention mechanisms. Although the literature has framed the focused deterrence framework under the umbrella of “deterrence” (Kennedy, 2009a) as well “legitimacy” (Papachristos et al., 2007), it is possible that the increased use of social service provisions by high-risk offenders could increase levels of institutional engagement. It is important to consider that structural criminological research would suggest that enhanced institutional engagement could potentially lead to reductions in homicide (see McCall, Land, Dollar, and Parker, 2013). The Cincinnati GVRS evaluation did not find any significant relationship between service provisions and changes in city-level violence (Engel et al., 2013: 28). However, parsing out such mechanisms is both an analytical challenge and a vital next step in future evaluation research where interventions are guided by various theoretical frameworks.

*Phase I: Homicide Rate Change in New Orleans Contrasted with Similar High-Trajectory Cities*

Phase I is designed to assess whether New Orleans experienced a change in homicide above and beyond cities with highly comparable homicide rates. The City of New Orleans has maintained a persistently high rate of lethal violence since the 1970s (McCall et al., 2011). Prior research has demonstrated the benefits of using group-based trajectory analysis when attempting to achieve balance between cases and controls in observational (i.e., nonexperimental) settings (see Haviland and Nagin, 2005, 2007). Previous studies that have classified geographic units such as street segments, neighborhoods, and cities into different trajectory groups consistently illustrate that places with the highest *levels* of violence are also responsible for the largest peaks and valleys (i.e., *variability*) in violence, whereas moderate and lower trajectory classifications typically experience far fewer ebbs and flows in crime (Braga, Papachristos, and Hureau, 2010; Griffiths and Chavez, 2004; McCall et al., 2011; Weisburd, Bushway, Lum, and Yang, 2004).

As an initial step, we drew from work by McCall et al. (2011) that was based on a long-term trajectory analysis of homicide rates in U.S. cities from 1976 to 2005. McCall et al. identified 15 cities (including New Orleans) that had the most persistently high homicide rates, similar structural factors that predicted group classification, and vastly similar changes in homicide rates during a sustained period of time.<sup>6</sup> By restricting our comparison of changes in homicides in New Orleans with these previously validated high-trajectory homicide cities, we restricted our comparison to cities that are also more likely to experience similar shifts in their homicide rates over time. The homicide data examined in this analysis are for the years 2008 through 2013. We conducted a series of difference-in-difference Poisson regression models (with an offset exposure variable accounting for the annual population for each city—thus transforming the outcome into a homicide rate) based on Equation (1):

$$\log(\text{Homicides})_{it} = \alpha + I(\text{NewOrleans})_{it} B_1 + I(\text{Treatment})_{it} B_2 + I(\text{NewOrleans})_{it} \times I(\text{Treatment})_{it} B_3 + \log(\text{Population})_{it} + \varepsilon_{it} \tag{1}$$

where  $\log(\text{Homicides})_{it}$  denotes the homicide count for each city between 2008 and 2013 (which is transformed into a homicide rate via the natural logarithm with the inclusion of the population exposure variable on the right-hand side of the equation),  $I(\text{New Orleans})_{it}$  is an indicator variable that equals 1 if the city is New Orleans and 0 otherwise,  $I(\text{Treatment})_{it}$  is

6. The 15 cities that were classified as high-trajectory homicide rate cities (and thus were used as treatment and comparison cities in subsequent trend comparisons) were as follows: Atlanta, Georgia; Baltimore, Maryland; Birmingham, Alabama; Cleveland, Ohio; Dallas, Texas; Detroit, Michigan; Flint, Michigan; Gary, Indiana; Miami, Florida; New Orleans, Louisiana; Newark, New Jersey; Oakland, California; Richmond, Virginia; St. Louis, Missouri; and Washington, DC. All annual homicide data from 2008 to 2013 were obtained from the Federal Bureau of Investigation’s Uniform Crime Reports (2013).

**T A B L E 1**

**Difference-in-Difference Model Estimates (New Orleans Compared with 14 High-Trajectory Homicide Cities)**

Estimate	Model 1 Treatment = 2013		Model 2 Treatment = 2012		Model 3 Treatment = 2011	
	<i>b</i>	(SE)	<i>b</i>	(SE)	<i>b</i>	(SE)
Intercept	-8.276**	(.011)	-8.290**	(.012)	-8.287**	(.014)
New Orleans	.765**	(.034)	.786**	(.039)	.767**	(.045)
Treatment	-.012	(.027)	.035	(.021)	.018	(.020)
Difference-in-difference	-.266**	(.090)	-.191**	(.068)	-.088	(.064)

\*\*  $p < .01$ .

an indicator variable that equals 1 if the year is in the posttreatment period and 0 otherwise, and where  $+ I(\text{New Orleans})_{it} \times I(\text{Treatment})_{it}$  is the difference-in-difference estimator to examine the direct impact of the change in homicides in New Orleans compared with other highly chronic lethal violent U.S. cities.<sup>7</sup>

Model 1 in Table 1 illustrates that if the regression model corresponds with an intervention date that equals 2013 (i.e., the *first full year* that is in the true postintervention period), then the City of New Orleans experienced a statistically significant homicide rate decline above the average homicide rate change for the 14 highly comparable cities identified by McCall et al (2011). Specifically, the incident rate ratio is written as  $e^{-0.266}$  or 0.766, which equates to  $-0.23$ , or a 23% homicide rate decline that was specific and unique to New Orleans. However, true implementation began in late October 2012, and thus, we alter the intervention period to include both 2012 and 2013 as the postimplementation period (Model 2). Similarly, the results show a statistically significant decline unique to New Orleans that was 17.3% lower ( $b = -.191$ , standard error [SE] = .068) when compared with the homicide rate change in the 14 comparison sites. Although the magnitude of the effect via the point estimate is reduced (from  $-23\%$  in Model 1 to  $-17\%$  in Model 2), the analysis still shows a robust reduction in homicides that was unique for New Orleans in 2012–2013. Finally, as a sensitivity test, we model the postimplementation period in the analysis as 2011–2013 (Model 3), which thus includes a statistical implementation period one year longer than the true postimplementation period. The difference-in-difference estimate for

7. A key assumption of the difference-in-difference framework is the parallel trend assumption. Research by McCall et al. (2011) highlighted that New Orleans and the other 14 high-trajectory cities have a long criminological history with similar levels as well as comparable rates of homicide rate change. Additionally, graphical analyses illustrated two consistent trends. First, New Orleans typically had homicide rates that were on the high end of the distribution, and second, no noticeable differences were observed in the shifts in homicide rates between New Orleans, as most comparison sites during the preintervention period were examined in this study.

Model 3 was no longer statistically significant ( $b = -.088$ ,  $SE = .064$ ), which indicates that the relative homicide rate change in New Orleans was specific only for years 2012 and 2013 (i.e., the *true* postintervention period in New Orleans). In sum, the homicide rate in New Orleans experienced a decline that was unique when compared with other chronic, high-trajectory homicide cities.

**Supplemental analyses to test potential regression to the mean.** Although the previous analysis provides evidence that the homicide rate change was unique to New Orleans, the highly comparable control sites were based on a classification using city crime and structural data that ranged from 1976 to 2005 (McCall et al., 2011). To generate the closest possible city-level homicide rate trajectories, we next follow a comparative analysis procedure outlined by Haviland, Nagin, and Rosenbaum (2007: 250–251). Specifically, we modeled homicide rate data from 2009 to 2011 for all U.S. cities (>100,000 population as of the 2010 U.S. Census) since this 3-year period immediately preceded the GVRs implemented in New Orleans in late 2012. All group-based trajectory analyses (GBTAs) relied on the Proc Traj procedure in SAS version 9.1 (Jones, Nagin, and Roeder, 2001; SAS Institute Inc., Cary, NC). Latent growth curves were operationalized as annual homicide rates for the years 2009–2011 by a set number of trajectories. Following the model identification procedures outlined by Nagin (2005), linear models were found to be the most appropriate, which was anticipated because only three observational periods were used to estimate the trajectory models. The Bayesian information criterion (BIC) was also evaluated to select the appropriate number of trajectory groups. The BIC can be viewed as an approximate standardized model fit indicator because it penalizes when an increase in the number of trajectory groups ( $k$ ) is estimated.<sup>8</sup> As shown in Table 2, most large U.S. cities were in the low homicide trajectory group ( $n = 219$ , 81.4%), which averaged four homicides per 100,000 from 2009 to 2011. A total of 43 cities (15.9%) were classified in the moderate trajectory group that had an average homicide rate of 16 per year. A small number of cities ( $n = 7$ , 2.7%) was classified in the high-trajectory group for the years 2009–2011 (i.e., the period that immediately preceded the 2012 New Orleans intervention), which averaged 39 homicides per 100,000. The cities in this trajectory include New Orleans along with Detroit, Baltimore, St. Louis, Newark, Flint, and Richmond (California).

We conducted a sensitivity difference-in-difference regression analysis to compare the shift in homicides in New Orleans with these six (immediate) high-trajectory cities. These

8. The BIC values clearly indicated the three-group solution was the most appropriate for city homicide rate trends. The three-group solution was identified as follows:  $BIC = -2341.48$ , where  $N = 269$  (269 cities). In terms of model diagnostics, Nagin (2005) suggested that average posterior probabilities above .70 are acceptable, whereas values closest to 1.0 are ideal. The average posterior probability of group membership for each group was higher than 0.95 for each of the three groups identified. Nagin (2005: 89) also specifically indicated that the odds of a correction classification for each group should have an estimated value above 5.0 to achieve high assignment accuracy (i.e., low residual deviation). The lowest odds of a correction classification for any group were 18.93, which exceeds the acceptable diagnostic criteria.

**T A B L E 2**

**GBTA Model Estimates—Urban Homicide Rates (2009–2011)**

Parameter	Low Group ( <i>k</i> = 1)	Moderate Group ( <i>k</i> = 2)	High Group ( <i>k</i> = 3)
Group probability	0.989	0.959	1.000
Homicide rate 2009	4.58	16.53	39.45
Homicide rate 2010	4.30	16.41	37.13
Homicide rate 2011	4.30	16.31	40.25

**T A B L E 3**

**Difference-in-Difference Model Estimates (New Orleans Compared with Six High-Trajectory Homicide Cities)**

Estimate	Model 1 Treatment = 2013		Model 2 Treatment = 2012		Model 3 Treatment = 2011	
	<i>b</i>	( <i>SE</i> )	<i>b</i>	( <i>SE</i> )	<i>b</i>	( <i>SE</i> )
Intercept	-7.888**	(.041)	-7.915**	(.036)	-7.929**	(.031)
New Orleans	.415**	(.082)	.459**	(.096)	.475**	(.127)
Treatment	.057	(.076)	.109	(.080)	.104	(.068)
Difference-in-difference	-.375**	(.102)	-.314**	(.149)	-.241	(.162)

\*\* *p* < .01.

six control sites were most comparable with New Orleans in terms of homicide rate levels for the 3-year period that immediately preceded the implementation of the intervention in 2012. The difference-in-difference findings displayed in Table 3 mirror those presented previously; specifically, New Orleans experienced a statistically significant 31.2% homicide rate divergence ( $b = -.375$ ,  $SE = .102$ ) when the intervention date in the model is set to year 2013 (Model 1). Likewise, Model 2 shows that New Orleans had a 26.9% statistically significant reduction ( $b = -.314$ ,  $SE = .149$ ) in homicides in the years 2012–2013 when compared with the control cities. Finally, the difference-in-difference estimate was not statistically significant in Model 3 ( $b = -.241$ ,  $SE = .162$ ), which indicates that the homicide rate departure in New Orleans did not occur in 2011—or before the intervention was actually implemented.

The combined evidence from both sets of models shows the homicide rate decline in New Orleans was unique in two important respects. First, the relative homicide rate change between the preintervention and postintervention periods in New Orleans was distinctive when compared with cities that followed similar homicide rate trajectories. Second, the multiple models used in this analysis illustrate the relative homicide rate change was greatest for New Orleans in 2013 (the first full year in the postintervention period), was marginally



lower for New Orleans in 2012–2013 (the intervention was implemented in late 2012), and did not seem to exist in 2011 (the year prior to the New Orleans intervention). These combined results suggest that New Orleans experienced a distinctive change in homicides that corresponded with the implementation of its citywide GVRs strategy.

*Phase II: Time-Series Analyses on Homicides and Firearm Assaults within New Orleans*

To assess the programmatic impact *within* New Orleans more fully, we move to an interrupted time-series analysis that accounts for unique changes in additional types of targeted crime outcomes. The interrupted time-series design is appropriate when over-time data are analyzed to assess the degree to which a treatment shifts the trajectory of a single case over time (McCleary and Hay, 1980). Although any analytical design has limitations, time-series modeling can be enhanced in many ways. In the context of the current study, Morgan and Winship's (2007: 245–252) review of the original Boston Ceasefire DOJ report that relied on time-series analysis (see Braga et al., 2001) found the Boston evaluation was of “high quality” largely because of the use of supplemental analyses, which included the examination of multiple outcomes that were hypothesized to be influenced by the citywide intervention. Morgan and Winship (2007) specifically noted that the use of multiple within-city outcomes improved the rigor of the time-series design, which can bolster the case for causal assertion.

Research has suggested that the underlying dynamics that generate homicides, gun homicides, and gun assaults are powerfully influenced by gang and group conflicts (e.g., Braga et al., 2014), which are the same dynamics targeted by the GVRs. Therefore, we examined four key citywide monthly outcome variables as a way to triangulate possible program impacts:

- (1) Overall homicides<sup>9</sup>
- (2) Firearm-related homicides
- (3) Firearm assaults<sup>10</sup>
- (4) GMI homicides

The various violent crime incident data examined here were provided by the NOPD Crime Analysis Unit.<sup>11</sup>

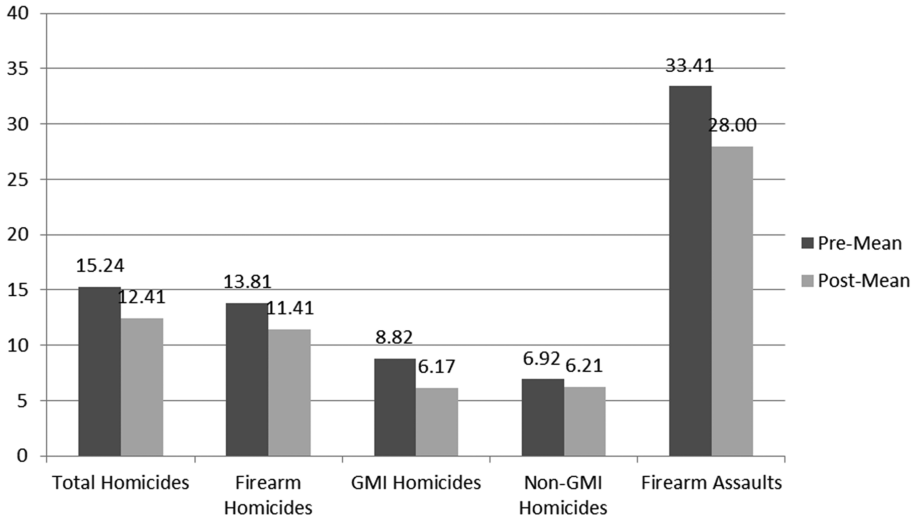
9. Homicides excluded police intervention shootings and those classified as self-defense or justifiable.

10. A change in the data reporting structure by the NOPD did not allow for an analysis of earlier shooting data prior to 2010; likewise, given the detailed nature of the GMI incident reviews, the research team was not confident that retrospective classifications of homicide incidents that occurred prior to 2010 would yield the same level of measurement validity as those examined between January 1, 2010 and March 31, 2014.

11. Officially reported crime incident data are not without limitations, such as citizen reporting biases (Warner and Pierce, 1993) and officer decisions to alter crime classifications and actions taken on

**FIGURE 1**

**Mean Monthly Difference in Targeted Crime Outcomes Between Preintervention and Postintervention Periods**



We use November 2012 as the period of implementation onset in our monthly preintervention and postintervention time-series comparisons given that the first pulling levers call-in meeting occurred on October, 25, 2012, when approximately 40 probationers and other high-risk prior offenders affiliated with gang networks were called into a session in an Orleans Parish courtroom. Prior evaluations of focused deterrence indicate that the strategy can have a “light-switch” (i.e., immediate and sustained) impact on homicide and gang-related offenses (Kennedy, 2006). Therefore, the analyses presented in this article are modeled to examine immediate and sustained changes in violent crime.<sup>12</sup>

Figure 1 shows the unconditional bivariate change in the various types of outcomes that the focused deterrence strategy was intended to impact (as well as the non-GMI homicides, which we use as a point of comparison). Specifically, we find that after implementation, the mean monthly count of total homicides decreased from 15.2 pretest monthly incidents to 12.4 posttest monthly incidents or by 18.6%. A similar percentage reduction (17.4%) in firearm-related incidents was observed during this same period—from 13.8 pretest mean

incidents reported to police (Black, 1970). Although it is important to consider the appropriate limitations of official incident level data, police incident reports have been widely used to assess trends and patterns of lethal gun violence (Blumstein and Rosenfeld, 1998) and previous focused deterrence initiatives (Braga and Weisburd, 2012).

12. Alternative specifications are examined in the sensitivity analyses.

monthly offenses to 11.4 posttest monthly incidents. The largest and most substantive reduction was specifically observed for GMI homicides; a 30.1% decline was detected (a change from 8.8 GMI homicides per month to 6.2 in the postintervention period). Comparatively, non-GMI incidents also declined but the reduction was much less pronounced (–10.3%) between the preintervention and postintervention periods (6.9 average monthly incidents to 6.2 postmonthly incidents). Finally, firearm assaults experienced a sizeable reduction from 33.4 per month to 28.0 per month (–16.2%).

Figure 2 displays the monthly counts of homicides (all types) and firearm assaults, which were distributed as event counts. These events do not approximate a normal, continuous distribution (King, 1988). Because crime rates are discrete events that can suffer from low counts during a specified time period, there are a variety of problems when analyzing such data by using ordinary least-squares regression estimation (Osgood, 2000). Most importantly, normal or symmetrical error distributions cannot be expected when crime counts are small because the error distribution becomes rapidly skewed. A similar assumption is required when using Autoregressive Integrated Moving Average time-series analysis (Box and Jenkins, 1976).

The conventional analytical approach in criminology for analyzing event counts, and in particular crime events, has been to rely on Poisson regression via maximum-likelihood estimation (Long, 1997; Osgood, 2000). The Poisson distribution as it relates to this analysis is expressed as follows:

$$P(Y_i = y_i | x_i) = \frac{\exp(-\lambda) \lambda^{y_i}}{y_i!} \quad (2)$$

where  $Y_i$  is a random variable representing a violent crime count (i.e., homicide event or firearm assaults in this sample),  $y_i$  is a count value that denotes the number of monthly events observed for a discrete time period, and  $\lambda_i$  represents different values in violent crime counts at distinct points (i.e., months) in time. To predict  $\lambda_i$ , we relied on the loglinear function of the following model:

$$\ln(\lambda_i) = x_i^T \beta \quad (3)$$

where  $x_i^T \beta$  is a linear combination of predictors for each case ( $i$ ). When estimating the interrupted time-series models, this combination of measures included a postintervention variable (1 = November 2012 onward), trend measures that account for a general decline (i.e., fluctuations) in the time series, and monthly dummy variables to control for consistent seasonal shifts in the data.

It is important to note the conditional Poisson process assumes equidispersion between the expected mean and variance for the outcome variables modeled (Long, 1997). We reestimated each Poisson regression model by relying on the conditional negative binomial distribution because the overdispersion in the models that were estimated could lead to

FIGURE 2

Monthly Count of Targeted Crime Outcomes (Total Homicides, Firearm Homicides, GMI Homicides, and Firearm Assaults) Preintervention and Postintervention

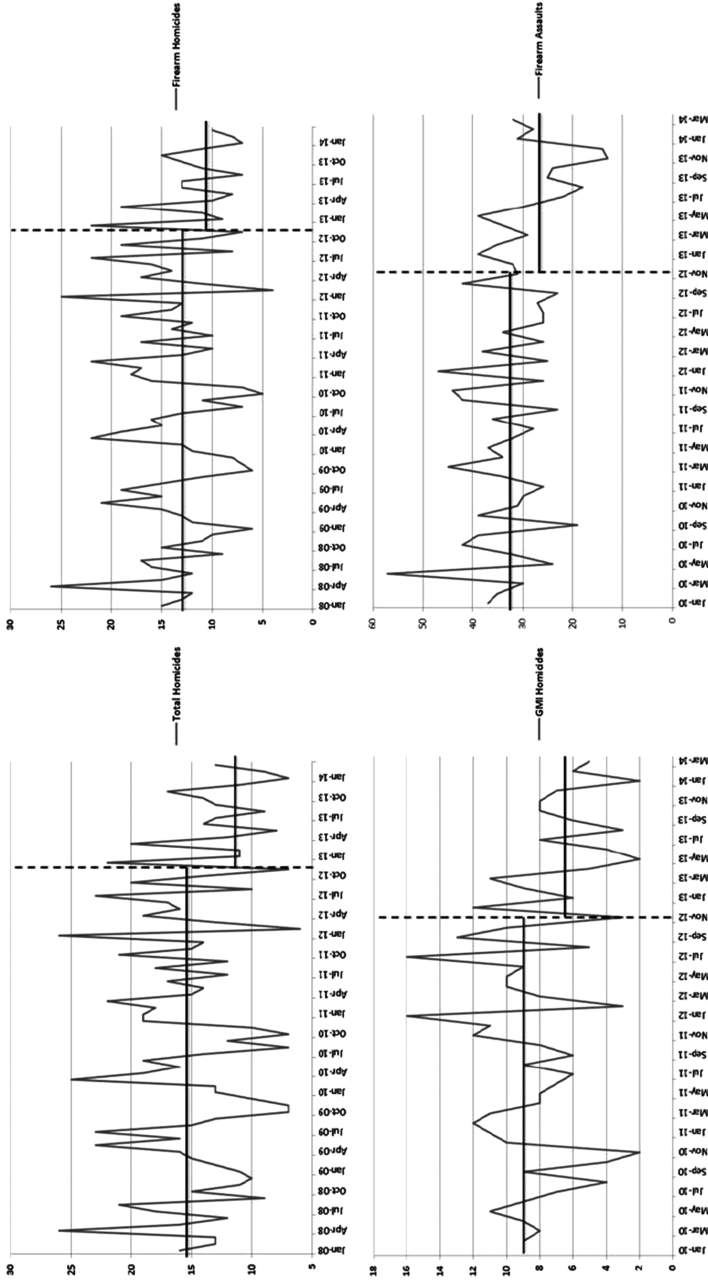


TABLE 4

**Annual Changes in Homicides Relative to Changes in Overall Violent and Property Offenses (2006–2012)**

Offense Type	2006	2007	2008	2009	2010	2011	2012*
Homicide	162	209	179	174	175	200	161
Percentage change	—	+29.01	-14.35	-2.79	+0.57	+14.28	2.42
Overall Violent Crime	2,225	3,451	2,869	2,614	2,593	2,748	2,240
Percentage change	—	+53.03	-16.68	-8.08	-0.83	+6.00	-0.04
Overall Property Crime	12,178	15,583	14,880	12,940	12,645	14,013	11,431
Percentage change	—	+27.96	-4.51	-13.04	-2.28	+10.81	-1.06

\*The 2012 total only includes incidents between January 1, 2012 and December 31, 2012. The percentage change for 2012 is also relative to the total number of incidents between January 1, 2011 and October 31, 2011. In 2011, 165 homicides, 2,241 overall violent crimes, and 11,553 total property crimes occurred between January 1 and October 31.

biased statistical inferences (Hilbe, 2007; Osgood, 2000). In each outcome examined, we used the Kolmogorov–Smirnov goodness-of-fit test and found that none of the distributions examined were overdispersed. Thus, we display the results from the conventional Poisson regressions with monthly fixed-effects parameters as a way to control for omitted static influences on specified outcomes that were not included in our models (Allison and Waterman, 2002). We also include the Huber–White robust sandwich estimator to ensure the coefficient variances were robust to violations of homoscedastic error distributions. STATA 12.0 SE software (StataCorp, College Station, TX) was used in all analyses presented herein. However, as with any interrupted time-series estimation, we note that statistical inferences in the subsequent analyses should be tempered because of the limitations of the design (see McDowall, McCleary, Meidinger, and Hay, 1980: 7).

It is first necessary to assess whether potential changes in targeted crime outcomes, such as homicides, occurred simply as a result of a broader fluctuation in crime within New Orleans during the period of examination. Table 4 displays annual Uniform Crime Reports crime counts from 2006 to 2012, which serves as a 7-year preintervention window that illustrates the relative stability between annual changes in homicides with changes in overall violent crime (homicides, rapes, robberies, and assaults combined) and changes in property crime (burglaries, larcenies, and motor vehicle thefts). When examining year-by-year variations, the annual changes in homicides followed extremely similar patterns to changes in overall violent crime and property crime. The shifts (increases or decreases) were virtually identical among homicides, overall violent crimes, and property crimes in 6 of the 7 years from 2006 to 2012. In the year where there was a minor degree of relative variation (2010), crimes of all types were stable relative to the earlier baseline year (2009), with a difference less than two percentage points between each

**TABLE 5**

**Poisson Regression Results and Percentage Change Estimates on Targeted Outcomes (January 1, 2008 to March 31, 2014)**

Variable	Total Homicides		Overall Violence		Overall Property	
	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100
Intervention	-.191* (.085)	-17.38%	.128** (.035)	13.65%	.085** (.021)	8.87%
February	-.215 (.169)	19.50%	-.045 (.102)	-4.40%	-.151* (.061)	-14.01%
March	.161 (.168)	-16.99%	.086 (.090)	8.98%	-.020 (.041)	-1.98%
April	.170 (.166)	-18.29%	.075 (.067)	7.88%	-.020 (.040)	-1.98%
May	.030 (.166)	-2.63%	.100 (.079)	10.51%	.125* (.049)	13.31%
June	.051 (.141)	-4.70%	.023 (.066)	2.32%	.070 (.045)	7.25%
July	.132 (.164)	-13.31%	.085 (.077)	8.87%	.136* (.031)	14.65%
August	-.120 (.203)	12.10%	-.007 (.069)	-0.69%	.096* (.032)	10.08%
September	-.145 (.165)	13.32%	-.055 (.086)	-5.35%	.010 (.046)	1.00%
October	-.158 (.207)	14.61%	.002 (.072)	0.20%	.016 (.034)	1.61%
November	-.267 (.193)	23.73%	-.065 (.077)	-6.29%	-.051 (.040)	-4.97%
December	-.017 (.199)	2.73%	-.041 (.067)	-4.01%	.027 (.043)	2.73%
Intercept	2.69** (.152)	—	5.41** (.063)	—	6.48** (.049)	—

\* $p < .05$ . \*\* $p < .01$ .

outcome examined. In short, from 2006 through almost all of 2012, homicides, overall violent crimes, and property crimes followed extremely similar patterns of change in New Orleans.

**Multivariate Results**

Moving to the multivariate framework, Table 5 presents the results of the interrupted time-series analyses that examine the impact of the GVRS on homicides while controlling for other important covariates (i.e., seasonal monthly shocks). The targeted gang and criminally active group members that were called into notification sessions were identified

based on their participation in gun violence; thus, it was anticipated that the strategy would have the greatest impact on lethal violence. Comparative estimates are also provided for changes in overall violent crimes and property crimes, which were beyond the specific scope of the focused deterrence strategy. Unstandardized coefficients and standard errors are presented within the table, along with the estimated percentage change in the monthly incidents, which is expressed as incident rate ratios (IRRs). The IRRs are exponentiated coefficients given the use of the logarithmic transformation in the Poisson regression models and percentage changes  $([IRR - 1] \times 100)$  in the intervention estimates.

The findings presented in Table 5 indicate that the GVRS was associated with a statistically significant 17.4% decline ( $b = -.191, SE = .085$ ) in the mean monthly number of homicides in the postintervention period relative to the preintervention period. Additionally, none of the monthly dummy variables were statistically significant, which suggests relative seasonal stability in homicide incidents in New Orleans. Comparatively, both overall violence and overall property crimes experienced statistically significant increases during the same period that homicides experienced a significant decline. A significant 13.6% increase in violent crimes and an 8.8% increase in property crimes occurred after November 2012. These findings, combined with the earlier year-by-year trends (displayed in Table 4), suggest that the New Orleans strategy had a crime prevention impact that was unique to homicides that occurred beyond chance, whereas overall Part I crimes significantly increased during the postintervention period.

Table 6 provides more precise details on the types of homicides and firearm-related outcomes that were impacted by the GVRS. Several prior focused deterrence evaluations showcase a consistent effect: The greatest and most sizeable decreases in homicides are those that are group and gang related, which is expected because the most chronically violent gangs are the specific target of the multiagency task force (see Braga et al., 2008; Corsaro and McGarrell, 2009; Engel et al., 2013). In New Orleans, GMI homicides significantly reduced by 32.1% in the postintervention period ( $b = -.387, SE = .116$ )—net of seasonal controls. Comparatively, non-GMI homicide incidents (i.e., lethally violent incidents that were considerably more likely to be domestically related and involve nonchronic offenders) did not experience a statically significant mean difference. Specifically, an 8.9% nonsignificant decline occurred in non-GMI homicides ( $b = -.094, SE = .093$ ). When compared with a significant reduction in GMI homicides by 32.0% in the postintervention period, these results highlight that the driving force behind the overall homicide decline was the specific reduction in lethal violence that was group or gang involved.

Table 6 also illustrates that both lethal and nonlethal firearm-related incidents experienced similar changes that corresponded with the New Orleans GVRS. The mean number of monthly firearm-related homicides significantly declined by 16.3% ( $b = -.178, SE = .092$ ). Likewise, the mean monthly number of nonlethal firearm assaults significantly

**T A B L E 6**

**Poisson Regression Results and Percentage Change Estimates on Targeted and Comparison Outcomes**

Variable	GMI Homicides <sup>a</sup>		Non-GMI Homicides <sup>a</sup>		Firearm Homicides <sup>a</sup>		Firearm Assaults <sup>b</sup>	
	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100
Intervention	-.387** (.116)	-32.09%	-.094 (.093)	-8.97%	-.178* (.092)	-16.30%	-.177** (.067)	-16.22%
February	-.120 (.260)	-11.30%	-.575** (.216)	-43.72%	-.165 (.210)	-15.21%	-.136 (.117)	-12.71%
March	-.023 (.220)	-2.27%	.446* (.230)	56.20%	.169 (.205)	18.14%	-.033 (.112)	-3.24%
April	-.149 (.187)	-13.84%	.239 (.167)	26.99%	.217 (.203)	24.23%	.021 (.177)	2.12%
May	-.180 (.240)	-16.47%	-.121 (.243)	-11.39%	.031 (.201)	3.14%	-.097 (.146)	-9.24%
June	-.247 (.239)	-21.88%	-.121 (.216)	-11.39%	.100 (.175)	10.51%	-.207 (.110)	-18.69%
July	-.003 (.278)	-0.29%	.381** (.162)	46.37%	.144 (.197)	15.48%	-.224 (.146)	-20.06%
August	-.570* (.257)	-43.44%	-.037 (.160)	-3.63%	-.168 (.219)	-15.46%	-.207 (.145)	-18.69%
September	-.088 (.231)	-8.42%	-.121 (.243)	-11.39%	-.097 (.199)	9.24%	-.495** (.124)	-39.04%
October	-.213 (.249)	-19.18%	.001 (.335)	0.10%	-.153 (.247)	14.18%	.005 (.117)	0.50%
November	-.304 (.250)	-26.21%	-.055 (.265)	-5.35%	-.248 (.222)	-21.96%	-.173 (.183)	-15.88%
December	.165 (.218)	17.93%	.024 (.282)	2.43%	.022 (.231)	2.24%	-.327* (.157)	-27.39%
Intercept	2.31** (.154)		1.89 (.159)		2.62** (.173)		3.65** (.099)	

<sup>a</sup>Time series: January 1, 2008 to March 31, 2014.

<sup>b</sup>Time series: January 1, 2010 to March 31, 2014.

\*  $p < .05$ . \*\*  $p < .01$ .

declined by 16.2% ( $b = -.177, SE = .067$ ). Firearm assaults seemed to have more seasonal fluctuations (particularly during the late summer and fall months) than all other targeted offenses examined, as evidenced by the significance levels of the monthly dummy variables.

In an effort to assess whether changes in homicides were observed for the most “at-risk” groups for homicide victimization and to minimize concerns related to gang “definitional issues” (see Maxson, 1999), a series of time-series analyses is presented and distinguished by victim age and race demographics (given that such demographic classifications are not



**T A B L E 7**

**Poisson Regression Results and Percentage Change Estimates on Race and Age-Specific Homicides (January 1, 2010 to March 31, 2014)**

Variable	Black Male Victims 20–29 Years Old		Black Male Victims 30+ Years Old		All Other Victims 20–29 Years Old		All Other Victims 30+ Years Old	
	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100
Intervention	–.311** (.104)	–26.72%	.100 (.129)	10.51%	–.012 (.337)	–1.19%	–.388 (.290)	–32.15%
February	–.234 (.243)	–20.86%	.213 (.360)	23.73%	–2.079* (.990)	–87.49%	–1.466* (.503)	–76.91%
March	–.097 (.219)	–9.24%	.593 (.342)	60.94%	–.287 (.549)	–24.94%	–.262 (.353)	–23.04%
April	–.030 (.277)	–2.95%	.380 (.348)	46.22%	.558 (.419)	74.71%	–.842* (.385)	–56.91%
May	–.085 (.212)	–8.15%	.213 (.381)	23.78%	–.317 (.463)	–27.16%	.111 (.326)	11.73%
June	–.030 (.238)	–2.95%	.300 (.360)	34.95%	–.135 (.395)	–12.63%	–.506 (.435)	–39.70%
July	.072 (.210)	7.46%	.618 (.375)	65.52%	–.540 (.567)	–41.73%	–.255 (.421)	–22.50%
August	–.207 (.299)	–18.69%	.257 (.376)	29.30%	–1.233 (.661)	–70.85%	–.373 (.362)	–31.13%
September	–.273 (.272)	–23.89%	.117 (.424)	12.41%	–.828 (.521)	–56.30%	–.506 (.356)	–39.71%
October	–.239 (.282)	–21.25%	.166 (.377)	18.05%	–1.926* (.977)	–85.42%	–.054 (.454)	–5.26%
November	–.225 (.252)	–20.15%	.100 (.384)	10.51%	–1.924 (.977)	–85.39%	–.784 (.487)	–54.34%
December	–.159 (.253)	–14.70%	.240 (.429)	27.12%	–.272 (.534)	–23.81%	–.431 (.411)	–35.01%
Intercept	1.89** (.188)		1.06* (.341)		.136 (.317)		.715 (.224)	

\*  $p < .05$ . \*\*  $p < .01$ .

subject to misclassification). Table 7 shows that homicides involving Black male victims between the ages of 20 and 29 years old experienced a statistically significant decline of 26.7% ( $b = -.311$ ,  $SE = .104$ ). This observed intervention effect was found only for victims with this race and age classification. No significant decline occurred in homicides that involved older Black male victims (30 years old or older), and there were no significant changes in homicides that included pooled White and Hispanic male and female victims, or Black female victims (i.e., all other victims) between 20 and 29, as well as 30 years old or

older. Thus, the lone observed intervention effect among New Orleans homicides was for Black males between the ages of 20 and 29 years old.

Taken together, the analyses presented in this section consistently indicate that homicides experienced significant declines and that overall violence and overall property crimes experienced increases. Comparing trends among specific types of homicides, the observed significant reductions were specific only to GMI and firearm homicides, whereas non-GMI homicides (i.e., those that did not involve group and gang members) remained relatively stable during the period examined here. Finally, homicides that involved Black males between the ages of 20 and 29 years old were the homicides that experienced significant changes. These results indicate that targeted violent crime incidents had observed significant declines, whereas there was no evidence of a general reduction in overall crime or lethal violence that did not involve group or gang members.

***Sensitivity and supplemental time-series analyses.*** A series of sensitivity tests was examined to determine whether the estimated intervention point estimates were robust against rival explanations.<sup>13</sup> We conducted several placebo and sensitivity tests related to the intervention date. The placebo tests were designed to assess the relationship between the timing of the intervention estimate and the change in the targeted outcomes. We randomly selected four different placebo intervention dates for the preintervention time series available across each targeted outcome. As shown in Table 8, no placebo test resulted in a statistically significant intervention point estimate among the various homicide models (i.e., total homicides, GMI homicides, and firearms homicides), which indicates the observed significant intervention effect on the various types of homicides was found only when the *modeled postintervention period* paralleled the *true postintervention period* in New Orleans. This would suggest that the observed intervention effect on homicides occurred immediately after the initial notification session in New Orleans, which took place in late October 2012. In terms of firearm assaults, the placebo postintervention model (i.e., placebo model 4, or a postintervention period of January 2012 onward) was statistically significant

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13. We reestimated each model using negative binomial regression (to control for overdispersion), and the results mirrored those that were presented in this study in all meaningful ways. We also included a series of additional control variables such as linear and curvilinear trend measures, as well as annual dummy variables to absorb random annual fluctuations in violence. None of the linear, curvilinear, or use of dummy estimates changed the intervention estimates in any substantive way. We likewise examined each model for potential autoregressive processes because time-series data are likely to be influenced by closest proximity empirical values (McCleary and Hay, 1980). We did not find autoregressive processes to be an issue of concern based on the following results: First, we conducted Autoregressive Integrated Moving Average analyses on the preintervention time series, where we detected no statistically significant first- or second-order autoregressive parameters in the model identification stage. Second, we estimated first-order autoregressive parameters into each model that is presented in this article using the ARPOIS package available in STATA (Tobias and Campbell, 1998), where the results did not diverge substantively from those presented herein.

**T A B L E 8**

**Preintervention Time-Series Intervention Placebo Tests on Targeted Crime Outcomes (Total Homicides, GMI Homicides, Firearm Homicides, and Total Firearm Assaults)**

Variable	Total Homicides <sup>a</sup>		Firearm Homicides <sup>a</sup>		GMI Homicides <sup>b</sup>		Firearm Assaults <sup>b</sup>	
	Coefficient (St. Error)	p Value	Coefficient (St. Error)	p Value	Coefficient (St. Error)	p Value	Coefficient (St. Error)	p Value
Placebo Model 1	-.092 (.074)	.695	-.047 (.081)	.559	-.039 (.144)	.787	.002 (.081)	.976
Placebo Model 2	.012 (.072)	.859	.002 (.078)	.976	-.030 (.110)	.780	-.093 (.113)	.196
Placebo Model 3	-.103 (.074)	.167	-.101 (.080)	.210	-.057 (.109)	.599	-.124 (.067)	.088
Placebo Model 4	-.095 (.078)	.219	-.086 (.084)	.306	-.173 (.119)	.144	-.195 (.058)	.001

*Note.* Monthly dummy variables used to capture systematic seasonal fluctuations are estimated in each placebo model (4 models × 4 outcomes = 16 in total).

<sup>a</sup>Time series: January 1, 2008 to March 31, 2014, and random placebo preintervention and postintervention dates (1 = July 2009, 2 = February 2010, 3 = March 2012, 4 = May 2012).

<sup>b</sup>Time series: January 1, 2010 to March 31, 2014, and random placebo preintervention and postintervention dates (1 = March 2010, 2 = February 2011, 3 = August 2011, 4 = January 2012).

(IRR = .822), which indicates that the reduction in nonlethal firearms violence occurred in midyear 2013.<sup>14,15</sup>

14. There are two potential interpretations of this finding for firearm assaults. One possibility is that firearm assaults oscillated in an unsystematic way between January 2012 and March 2014 and that the intervention estimate in the time-series models (despite the use of relevant and suitable control variables) is influenced by such nonsystematic fluctuations. The second possibility, which seems more plausible given the visual display of the data in Figure 2, is that the statistically significant reduction in the average number of monthly firearm assaults was not precisely associated with the specific timing of the intervention onset in the time-series models. In this instance, the movement of the postintervention period to different dates does not influence the mean reduction in firearm assaults because the observed decline was less consistent with the timing of the true postintervention period; rather, it was influenced by the large shift in the magnitude of firearm assaults at some point in the true postintervention period. Figure 2 shows that beginning in early summer 2013, firearm assaults declined from reasonably stable 30–40 incidents per month to 15–25 incidents per month. In short, the timing of the intervention in the model becomes less important because of the very large decrease in firearm assaults that occurred in 2013.
15. As a second set of sensitivity tests, we followed procedures used by Cook and MacDonald (2011) and Piehl et al. (2003) to assess whether model fit parameters and the intervention point estimate coefficients had the largest estimated effect sizes (and optimal model fit indices) at the time of the intervention, or in the postintervention period (i.e., potential lagged effects) relative to potential preintervention effects (i.e., lead effects). The results presented in Appendix A illustrate that for total homicides, GMI homicides, and firearm homicides, the Wald chi-square statistic was smallest at the

Finally, it is important to isolate unique programmatic effects where possible. Although the combination of GVRS (focused deterrence) and street workers for conflict mediation (*CURE Violence*, which was implemented in Central City) has been shown to impact crime in previous settings (Engel et al., 2013; see also Webster, Whitehill, Vernick, and Curriero, 2013), we specifically partitioned the estimated impacts of GVRS and *CURE Violence* within Central City as well as the remainder of the city (i.e., excluding Central City).<sup>16</sup> The results are displayed in Appendix B and show that the overall city (minus Central City) experienced statistically significant declines in both total homicides (−17.5%,  $p < .05$ ) and GMI homicides (−32.9%,  $p < .05$ ). Comparatively, although both total homicides and GMI homicides were also reduced in Central City, this specific area of New Orleans did not have statistically significant declines that corresponded with November 2012 onset date. Thus, as will be discussed in more detail, there was no indication that the observed reduction in homicides that corresponded with the citywide focused GVRS was driven by the supplemental *CURE Violence* strategy within Central City.

## Discussion

The current study illustrates that focused deterrence strategies can have a significant impact even in the most challenging of contexts, which in the City of New Orleans included extremely high murder rates, political and police corruption, and a local culture seemingly more tolerant of violence. Furthermore, overall homicides in New Orleans significantly declined between 17% and 31% when compared with similar high-trajectory homicide cities. More refined interrupted time-series analyses within New Orleans show that significant reductions in violence were observed specifically for overall homicides (−17%), GMI homicides (−32%), homicides that involved young Black male victims (−26%), and both lethal and nonlethal firearms violence (−16%). A series of sensitivity tests and supplemental analyses provide more support that these observed intervention effects were robust, were unlikely to have been caused by extraneous circumstances (e.g., a general overall crime shift), and were consistent with the timing of the GVRS.

This study has two noteworthy limitations. First, the evaluation design was of secondary consideration to the implementation of the strategy given the widespread and persistent diffusion of violence within New Orleans. In an effort to overcome this limitation, we relied

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point of the true intervention (November 2011). There seemed to be a 2-month lagged effect (lag − 2) for firearm assaults. Thus, there is no evidence the intervention estimates were influenced in any discernible way by preexisting (preintervention) declines.

16. The City of Baltimore implemented a conflict mediation-based program (*Safe Streets*) within the McElderry Park community. However, police districts within Baltimore also conducted offender notification sessions, which were shown to have a significant and direct relationship with the observed reduction in homicides and nonfatal shootings associated with *Safe Streets* (see Webster et al., 2013: 36). Webster et al. controlled for this relationship when examining the impact of the conflict mediation strategy within McElderry Park.

on a series of analytical models to assess (and isolate) program impact; this methodology is of moderate strength and would have been enhanced considerably with a stronger quasi-experimental or pure experimental design, such as the use of a place-based focus at the onset to use a matching or randomization process (see Braga and Weisburd, 2014). However, prior criminological research has illustrated that geographic locations that have the highest levels of crime are also more likely to experience greater variability in crime. By modeling the change in homicides in New Orleans against a set of approximately matched control cities, it was apparent the decline was significantly sharper for New Orleans during the period examined here than all other sites. Second, the current study does not allow us to disentangle which of the observed aggregate crime effects are associated with deterrence (Braga and Weisburd, 2012), incapacitation effects (Levitt, 1998), changes in perceived legitimacy (Papachristos et al., 2007), or enhancements in social services—which can lead to greater institutional engagement (McCall et al., 2013). More precise measures (particularly at the individual and group levels) that capture these regulatory theoretical principles through surveys and narratives with notified offenders and groups would enhance the literature considerably.

Limitations notwithstanding, this study contributes to the literature in several important ways. Our findings illustrate that it might be possible to alter the mindset of gang and criminally active group members in settings where retaliatory violence has been a common occurrence. As found in a growing body of literature, *gang and groups* and *individuals* who receive the focused deterrence message participate in fewer documented cases of gun violence (as both victims and offenders) and less overall crime (across multiple outcomes) after call-in sessions relative to highly comparable gangs and groups as well as individuals who do not receive the deterrent-based message (Braga et al., 2013, 2014; Papachristos and Kirk, 2015; Wallace et al., in press). In short, it might be possible to alter in a tangible way persistent cultures of violence.

The findings presented in this study also suggest that outreach workers used to interrupt violence in one specific neighborhood had no independent effect that empirically corresponded with the reduction in violence in New Orleans. Such findings are similar with prior focused deterrence evaluations that have attempted to describe and examine the observable impact of street workers on violence (see Engel et al., 2013; Tillyer, Engel, and Lovins, 2012). Although not definitive, the results in this study add to the growing body of literature questioning the effectiveness of the use of street outreach workers rooted in the public-health model that are not connected to a larger criminal-justice-based violence-reduction initiative. As noted by Papachristos (2011), although the initial *Chicago CeaseFire* strategy showed mostly promising results, replications have shown less support for conflict mediators to combat serious lethal violence in alternative settings, such as in Pittsburgh (see Wilson and Chermak, 2011). Where conflict mediation has been implemented in settings that have shown changes in crime, it has corresponded with offender notification strategies that have been implemented in nearby geographic areas (e.g., *Baltimore Safe Streets*—see Webster et al., 2013) or occurred simultaneously with mediation strategies

(e.g., Cincinnati—see Engel et al., 2013, and now New Orleans). Thus, the literature has suggested that conflict mediation might complement focused deterrence, or its influence could be conditional in unique social settings, but there is little evidence to indicate that conflict mediation can function reliably as a stand-alone strategy to reduce violence. Future replications and evaluations are needed in this area.

Finally, it should be recognized that policy transfer was greatly enhanced by the persistence and commitment to the strategy by political and police officials in New Orleans. The research team, staff, and officers associated with GVRs received clear direction and unwavering political and police support from the highest levels of government, including Mayor Landrieu and NOPD Superintendent Serpas. This provided a clear mandate for the City of New Orleans, empowered those associated with the GVRs team, and ultimately led to successful implementation and reduced violence. However, members from the New Orleans GVRs working group (and future potential replicating agencies) would be well advised to understand the potential toward deterioration of treatment in strategies that approach their second, third, and fourth years (where applicable). Often, treatment deterioration is driven by staffing changes, changes in the characteristics of the target population, some decline in enthusiasm among working group officials, or other systematic factors (Kennedy, 2011; see also Land, McCall, and Williams, 1992). As noted by Brunson et al. (2013), the deepest collaborative relationships in focused deterrence working groups occur most often between individuals and not institutions, and thus staffing changes can undermine the collaborative capacity necessary for sustainability. Thoughtful efforts to rejuvenate the focused deterrence model among the key providers are critical to long-term sustainability (Tillyer et al., 2012). Additionally, maintaining a clear focus on problem identification (gangs and groups) is vital to sustained success.

Factors such as defining gang violence can potentially undermine such momentum. Even after recommending, measuring, and training NOPD supervisors multiple times to use an expanded definition of gang-member involved violent incidents, the agency routinely reverted back to its standard (conservative) definition of gang involvement in violence, requiring multiple recoding of the violent incidents by the research team and ongoing consultation and technical assistance to refocus the NOPD on the individuals and gangs most responsible for violence in the city. We conclude by reiterating that researcher–practitioner partnerships are imperative not only for problem identification, implementing effective strategic approaches, resource management, and evaluation purposes, but also for helping law enforcement keep track as to which individuals and groups are driving a city’s violent crime problems (see Kennedy, 2009b) and for maintaining programmatic sustainability. Each of these tasks is important for successful policy transfer and program implementation. Although researchers have long acknowledged that measurement matters for evaluation purposes, it should now be clear that measurement matters a great deal for programmatic implementation.

**Appendix A: Time-Series Intervention Estimate Sensitivity Tests on Targeted Crime Outcomes (Total Homicides, GMI Homicides, Firearm Homicides, and Total Firearm Assaults)**

Variable	Total Homicides <sup>a</sup>		Firearm Homicides <sup>a</sup>		GMI Homicides <sup>b</sup>		Firearm Assaults <sup>b</sup>	
	Coefficient (St. Error)	Model Wald Statistic	Coefficient (St. Error)	Model Wald Statistic	Coefficient (St. Error)	Model Wald Statistic	Coefficient (St. Error)	Model Wald Statistic
Lead + 2	-.166 (.086)	25.65	-.092 (.095)	29.24	-.251* (.124)	22.68	-.150* (.068)	35.99
Lead + 1	-.166 (.087)	25.65	-.153 (.092)	24.91	-.323** (.114)	29.02	-.157* (.067)	35.51
Intervention (T <sub>0</sub> )	-.191* (.085)	25.43	-.178* (.092)	24.05	-.387** (.115)	18.35	-.177** (.067)	37.06
Lag - 1	-.171* (.090)	25.52	-.160* (.098)	22.31	-.353** (.115)	29.15	-.188** (.073)	35.38
Lag - 2	-.219** (.086)	26.57	-.228** (.091)	26.42	-.398** (.119)	34.41	-.213** (.076)	37.70

Note. Monthly dummy variables used to capture systematic seasonal fluctuations are estimated in each sensitivity model.

<sup>a</sup> Time series: January 1, 2008 to March 31, 2014, and random placebo preintervention and postintervention dates (1 = July 2009, 2 = February 2010, 3 = March 2012, 4 = May 2012).

<sup>b</sup> Time series: January 1, 2010 to March 31, 2014, and random placebo preintervention and postintervention dates (1 = March 2010, 2 = February 2011, 3 = August 2011, 4 = January 2012).

\* $p < .05$ . \*\* $p < .01$ .

**Appendix B: Impact on Targeted Violence Within Geographic Settings with Competing Strategic Interventions (January 1, 2008 to March 31, 2014)**

Variable	Total Homicides				GMI Homicides			
	City Minus Central City		Central City Only		City Minus Central City		Central City Only	
	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100
Intervention	-.193** (.008)	-17.55%	-.131 (.432)	-12.27%	-.400* (.115)	-32.96%	-.161 (.414)	-14.87%
February	-.188 (.179)	-17.13%	-1.386 (1.055)	-74.99%	-.097 (.266)	-9.24%	-1.648 (1.030)	-80.75%
March	.158 (.179)	17.11%	.223 (.740)	24.98%	-.047 (.224)	-4.59%	.693 (1.081)	99.97%
April	.181 (.177)	19.84%	-.148 (.634)	-13.75%	-.192 (.195)	-17.46%	.893 (1.033)	144.24%
May	.048 (.159)	4.91%	-.554 (.755)	-42.53%	-.192 (.244)	-17.46%	.199 (1.253)	22.01%
June	.048 (.177)	4.91%	.139 (.664)	14.91%	-.261 (.198)	-22.97%	.199 (1.253)	22.01%

Variable	Total Homicides				GMI Homicides			
	City Minus Central City		Central City Only		City Minus Central City		Central City Only	
	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100
July	.162 (.176)	17.58%	-1.247 (1.036)	-71.26%	.017 (.238)	1.71%	-1.650 (1.043)	-80.79%
August	-.172** (.224)	-15.80%	.698 (.544)	100.97%	-.885** (.293)	-58.72%	1.991** (.924)	632.28%
September	-.158 (.170)	-14.62%	.362 (.712)	43.62%	-.192 (.209)	-17.47%	1.586 (1.089)	388.41%
October	-.158 (.231)	-14.62%	.139 (.666)	14.91%	-.261 (.285)	-22.97%	.892 (1.033)	144.00%
November	-.242 (.200)	-21.49%	-1.226 (1.048)	-70.65%	-.321 (.374)	-27.45%	.239 (1.292)	26.99%
December	-.025 (.194)	-2.46%	.160 (.759)	17.35%	.138 (.202)	14.79%	.932 (1.287)	153.95%
Intercept	2.70** (.147)		-.523 (.506)		2.29** (.187)		-1.548 (.899)	

\**p* < .05. \*\**p* < .01.

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# Focused Deterrence and Improved Police–Community Relations

## Unpacking the Proverbial “Black Box”

**Rod K. Brunson**

*Rutgers University*

Since Boston’s Operation Ceasefire was widely heralded as an effective youth violence-reduction strategy (Braga, Kennedy, Waring, and Piehl, 2001; Kennedy, 1997), numerous scholars have attempted to implement and evaluate subsequent deterrence-based models. This rapidly growing body of scholarship has resulted in improved understandings regarding how best to address a wide range of persistent crime and disorder problems (e.g., drug trafficking, gun violence, and youth gangs). We stand to learn considerably more, however, regarding exactly how notable crime reduction is achieved and whether focused deterrence strategies yield other important societal benefits.

Unfortunately, serious crime problems tend to cluster in places where people of color disproportionately reside—highly disadvantaged, urban settings. In these ecological contexts, police–community relations tend to be most strained and characterized by deeply rooted mutual suspicion. An emerging body of research reveals that seemingly indiscriminate and heavy-handed policing tactics erode minority citizen trust of and confidence in the police (Brunson, 2007; Carr, Napolitano, and Keating, 2007; Gau and Brunson, 2010). In contrast, focused deterrence policing efforts, consistent with the problem-oriented policing framework, rely on data-driven intelligence gathering to identify carefully and target repeat, high-risk offenders (for increased interagency law enforcement attention and individualized social service programming).

Directing intensive crime-prevention efforts at specific individuals and groups, who are disproportionately involved in problem behaviors, distinguishes focused deterrence from extremely controversial policing strategies (e.g., stop, question, and frisk). Furthermore, as a consequence of Tyler and colleagues’ (Sunshine and Tyler, 2003; Tyler, 1990, 2004;

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Tyler and Wakslak, 2004) innovative work, policing scholars better understand that officers have the potential to improve legitimacy by performing their duties in ways that reflect the guiding principles of procedural justice—fairness and respectfulness. Research has shown that citizens who view the police as being legitimate are more willing to support officers' crime-fighting mission (Brunson, Braga, Hureau, and Pegram, 2013; Decker, 1981). For instance, a recent randomized control trial of traffic stops in Australia found improved citizen views of the specific encounter and officers overall after motorists were exposed to procedurally just practices (Mazerolle, Antrobus, Bennett, and Tyler, 2013). Finally, although improving police–community relations might not be the primary goal of most focused deterrence models, these strategies seem to hold considerable promise for increasing police legitimacy in the eyes of community residents.

### **Procedural Justice and Police Legitimacy**

Offender notification meetings are a critical component of the focused deterrence framework. For example, research has demonstrated that the gangs called into the sessions were less likely to be involved in gun violence afterward, both as offenders and as victims, when compared with matched-control samples (Braga, Hureau, and Papachristos, 2014). This is also the case concerning recidivism among high-risk individuals (Wallace, Papachristos, Meares, and Fagan, in press). Corsaro and Engel (2015, this issue) note that “an emerging body of research has framed the use of offender notification meetings as a way to enhance the perceived legitimacy of the criminal justice system by providing an unbiased and procedurally just response to violence.” However, the empirical evidence is limited about whether offenders, members of their criminal networks, family, and communities view the police as having the moral, not just legal, authority to promote and enforce the deterrence message.<sup>1</sup>

Call-in meetings are open to the public, held in familiar neighborhood settings (e.g., community centers, churches, and recreational facilities), and typically receive substantial local media attention. Thus, news coverage depicting multiple arrests and intensified law enforcement activities could help assure citizens that the police are genuinely working to curtail neighborhood crime. Moreover, learning of concentrated crime-prevention efforts might improve citizen willingness to cooperate with the police, providing crucial information about active criminal investigations.

During call-ins, suspects are confronted with stacks of evidence that authorities have compiled in connection with their current criminal cases. Next, a cast of trusted community leaders (e.g., antiviolence activists, crime victims, and members of the clergy) explains to alleged offenders that the community honestly cares about them and their futures. Local

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1. Research teams examining various focused deterrence strategies have administered short questionnaires to prior call-in participants regarding their impressions of the announcement and whether they had shared the antiviolence message with others. Most of these findings have not been published, however.



spokespersons express strong support for giving arrestees another chance to reform their lives versus incarceration; they also are adamant that the community will no longer tolerate their wrongdoing and that future law violations (by them or fellow group members) will result in swift, certain, and severe punishment for all.<sup>2</sup> Alternatively, individuals with the most serious criminal histories are not summoned to call-ins but are referred for immediate prosecution.<sup>3</sup> Finally, before leaving the call-in, arrestees meet with a team of social service providers, who determine their individual programmatic needs (e.g., job training, education, and substance abuse treatment).

It stands to reason that legal authorities' public displays of procedural justice and compassion for call-in participants might lead to greater police legitimacy and community support (see Papachristos, Meares, and Fagan, 2007). For example, academic studies of police–minority community relations have underscored the importance of learning of family members' and friends' police interactions for collectively shaping citizen perceptions of police legitimacy (Brunson, 2007; Gallagher, Maguire, Mastrofski, and Reisig, 2001; Rosenbaum, Schuck, Costello, Hawkins, and Ring, 2005; Weitzer and Tuch, 2005). These indirect police encounters have been described in the literature as vicarious experiences. The perceived longstanding unfairness of the criminal justice system toward people of color is a frequent and spirited topic of conversation within minority communities (Boyles, 2015; Solis, Portillos, and Brunson, 2009; Stewart, 2007; Wood and Brunson, 2011). In fact, Black parents consistently report sternly cautioning youth about how to negotiate unwelcome police encounters successfully (Brunson and Weitzer, 2011). Thus, it is essential that crime-reduction strategies not only are successful but also inspire citizen confidence—especially among those who have historically been on the receiving end of racially discriminatory policing practices (Brunson and Miller, 2006; Holmes and Smith, 2008).

Given that observed fairness and program effectiveness are equally important for strengthening police–community relations, Corsaro, Brunson, and McGarrell (2010, 2013) used mixed methodological approaches in their evaluations of two separate “pulling levers” focused deterrence strategies (aimed at disrupting persistent, open-air drug markets). Specifically, in addition to collecting various pre- and post-intervention official crime data, research team members conducted 44 face-to-face interviews in Nashville, Tennessee, and 34 in Rockford, Illinois, in an attempt to examine local residents' perceptions of the respective initiatives. Most study participants across both research settings expressed having considerable confidence in the police and a renewed interest in partnering with officers to address local crime problems. Furthermore, most respondents in each city articulated a strong preference for policing strategies after program implementation when compared

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2. Call-in attendees are encouraged to circulate the warning of increased surveillance and heightened legal sanctions throughout their networks.
  3. It is common for their images and potential lengthy prison sentences to be displayed at notification meetings in the hope of deterring attendees from engaging in future criminal activity.

with previously unsuccessful approaches to combat street-level drug dealing (e.g., arrests, crackdowns, and sweeps). Although the qualitative findings from these two distinct evaluations may not be widely generalizable, they suggest that beyond being effective at reducing crime, which was the case in both studies, focused deterrence models also have the capacity to enhance police–community relations in high-crime, disadvantaged areas.

### Concluding Remarks

A string of recent, well-publicized deaths suffered by Black males at the hands of police, followed by episodes of civil unrest across several U.S. cities, are stark reminders of the sizeable rift between police departments and many communities of color. Although undeniably tragic, these events have also cast light on an issue that minority citizens have vigorously complained about for generations—perceived over-policing of their neighborhoods. Thus, there is growing recognition among police leaders that damaged police–minority community relations seriously threaten their ability to launch effective crime prevention strategies. Furthermore, several scholars have asserted that improved police legitimacy is a consequence of citizens holding favorable views of police after encounters where officers demonstrate procedural justice principles (Tyler, 2004).

An examination of Boston's Operation Ceasefire provides community leaders, policy-makers, and police administrators with keen insights regarding how to enhance police legitimacy and informal social control in high-crime, minority communities (Berrien and Winship, 2002; Winship, 2005; Winship and Berrien, 1999). Specifically, in the early 1990s, a group of activist Black clergy, frustrated by high rates of youth gang violence, founded the TenPoint Coalition (TPC).<sup>4</sup> The ministers later joined Ceasefire, quickly emerging as its principal community-based partner. Although TPC clergy did not play a role in establishing Ceasefire, they were integral in executing the overall gang violence-reduction initiative.

The extent to which community policing efforts are meaningful depends heavily on police executives' commitment to establishing and supporting mutually beneficial partnerships with local groups, especially those that can successfully broker trust between officers and neighborhood residents. Clearly, this is an enormous undertaking. For instance, the TenPoint–Boston Police Department (BPD) collaboration has endured several weighty challenges throughout its tenure. Furthermore, hardly any members of either organization define the relationship as continuously harmonious (see Brunson et al., 2013). In fact, early on, TPC ministers were antagonists of the BPD and routinely used local media outlets to denounce their crime control strategies (Winship and Berrien, 1999). As a result of faith leaders' efforts to engage at-risk youth on the streets, however, they repeatedly came into contact and established rapport with BPD officers assigned to the Youth Violence Strike Force. Interestingly, individuals from two vastly different organizations reached a shared

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4. Originally, the TPC comprised approximately 40 local churches.

perspective of Boston's youth violence problem. Specifically, officers and ministers became acutely aware of three key features:

- (1) Most disadvantaged neighborhood youth were not engaged in violence.
- (2) Youth gang members were in need of targeted intervention and prevention programming.
- (3) Only a handful of gang-involved youths warranted confinement.

The TPC's pivotal role in Ceasefire and considerable visibility within high-crime neighborhoods enabled them to deliver an antiviolence message to at-risk youth that reflected the communities' moral voice and consciousness.

Moreover, TPC leaders served as important liaisons to the BPD and worked tirelessly to enhance community residents' trust of them in the hope of extending it to police officers. During several potentially racially explosive incidents involving BPD officers (e.g., dubious use of force against unarmed minority suspects), ministers' public support provided the police department with an "umbrella of legitimacy" (Winship and Berrien, 1999) or a "moment of pause" (Brunson et al., 2013) to conduct appropriate investigations. Furthermore, BPD command staff frequently sought clergy leaders' advice prior to implementing crime control strategies that residents might perceive as needlessly aggressive. In their investigation of plural policing in England and Wales, Crawford, Lister, Blackburn, and Burnet (2005) observed that, "good community consultation at both strategic and operational levels was identified as important in establishing and maintaining community engagement and helping to build constructive and informed relationships" (p. x).

Although Ceasefire involved faith-based leaders, other organizations enjoying extraordinary levels of community support might also be capable of effectively partnering with the police. When forming collaborations, however, police commanders should resist the urge to dismiss hastily unconventional groups from consideration (e.g., those composed of prior offenders) and organizations that have previously openly challenged the department's crime control policies. In fact, groups causing traditional criminal justice organizations trepidation also might have huge credibility among neighborhood residents. Local activists who occasionally take on adversarial positions to public officials also understand that police officers play essential roles in maintaining public safety. Furthermore, most informal community leaders' ultimate goal is the implementation of policing innovations that are both effective and evenhanded. Corsaro and Engel's (2015, this issue) study joins an expanding body of research demonstrating the promise of focused deterrence strategies and is especially noteworthy given the multitude of disconcerting social problems plaguing New Orleans. Furthermore, Corsaro and Engel's findings contribute to current policy-relevant discussions centered on encouraging police contacts that improve citizens' sense of fairness and procedural justice (Tyler, 2004). This area of research has consistently demonstrated that effective crime control efforts do not have to result in weakened police legitimacy.

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# Something That Works in Violent Crime Control

## Let the Focused Deterrence and Pulling Levers Programs Roll with Eternal Vigilance

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**D**uring the past two decades, criminologists have teamed up with police departments in several U.S. cities to initiate and evaluate various forms of community and police interventions for increasing both general and specific deterrence against crime. The general objective of these interventions is crime reduction, that is, a reduced number of crime incidents, often, but not always violent crime incidents, within a specific area or citywide. The deterrence principle on which the interventions are based is that an increased visibility of the police by hiring more officers and by allocating officers to heighten the perceived risk of apprehension can have marginal deterrent effects (Durlauf and Nagin, 2011).

A particular form of several interventions is the “focused deterrence” framework, which is also known as “pulling-levers policing” (Braga, 2012; Braga, Kennedy, Waring, and Piehl, 2001; Kennedy, 1998, 2008). The key ideas of focused deterrence intervention strategies are to increase the risks faced by offenders and to find new and effective ways to use traditional and nontraditional law enforcement tools to communicate incentives and disincentives directly to targeted chronic offenders. Many careful studies of focused deterrence interventions have been conducted; these are cited and reviewed by Corsaro and Engel (2015, this issue) and will not be repeated. Suffice it to reiterate that the review and meta-analysis of 10 quasi-experimental evaluations and one randomized trial of focused deterrence strategies to prevent and control gang and group-involved violence, overt drug

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markets, and individual repeat offenders by Braga and Weisburd (2012) concluded that the interventions were associated with medium or moderate overall effect size reductions on such crime outcomes as total homicides, homicides involving gang members, gun homicides, and gun assaults.

Into this research arena now comes Corsaro and Engel's (2015) evaluation of the impact of the New Orleans Group Violence Reduction Strategy (GVRS) on serious violence. As they note, New Orleans not only suffered from the devastation of Hurricane Katrina in 2005, but also the city has endured decades of high homicide rates, high levels of gang crime and violence, poverty, police misconduct, and political corruption. Indeed, it is a challenging urban context and test case for the effectiveness of a violence-focused deterrence program. What could be expected from the GVRS program? Although the estimated reductions in violent incidents associated with focused deterrence interventions vary from study to study and from outcome to outcome, based on prior interventions focused on gang or group involvement, a reasonable broadband *a priori* expectation is a reduction in the range of 15% to 35% in total homicides and 25% to 40% in gang- or group-involved homicides.

Some key parameters of violent crime in New Orleans are as follows: an average of 55 homicides per 100,000 residents in the years 2008–2012 (approximately four times the national average for large U.S. cities with a population of greater than 250,000), 59 potential street gangs with 600–700 individual members in six of the seven police districts identified prior to initiation of the GVRS program, and approximately 54% of lethal incidents during the period prior to and during the study (2010–2014) identified as group or gang member involved (GMI).

Corsaro and Engel's (2015) assessment of impact of the GVRS consists of two parts. The first part compares the change trajectory of the homicide rate of New Orleans for the years 2008 through 2013 with the trajectories of other high homicide rate cities identified in a previous study. Compared with the other cities, this analysis estimates a 17% to 31% reduction in the New Orleans annual homicides associated with the initiation of the GVRS in late 2012. The second part of the evaluation is an interrupted time-series analysis for monthly incident data on overall homicides, GMI homicides, firearm-related homicides, and firearm assaults. This analysis estimates a mean monthly decrease of 19% for total homicides and 30% for GMI homicides. In brief, these estimated reductions are within the ranges of expected reductions, based on prior GMI-focused interventions, cited previously. The various sensitivity and supplemental analyses reported by Corsaro and Engel support the inference that the estimated reductions are robust.

### **What Works? Lessons Learned**

In brief, the New Orleans GVRS intervention was associated with lethal violence reductions of the order of magnitude that might be expected of an effective pulling-levers strategy. Corsaro and Engel (2015) provide additional empirical evidence that focused gang- or group-related violence reduction interventions work. Why? To me, there are four takeaways.



First, as Corsaro and Engel (2015) state, consistent with the growing body of focused deterrence/pulling-levers intervention evaluation studies, gangs/groups and individuals who participate therein who receive the focused deterrence message “participate in fewer documented cases of gun violence (as both victims and offenders) as well as less overall crime (across multiple outcomes) after call-in sessions relative to highly comparable gangs/groups and individuals who do not receive the deterrent-based message.” The inference from these intervention studies is that communicating directly to those who are likely to be chronic offenders affects their perceptions of sanction risk and certainty (Braga, 2012; Kennedy, 1998), and, thereby, alters the persistent cultures of violence, including gang retribution violence, that go therewith.

Second, the focused deterrence approach recognizes that pulling levers does not occur in a vacuum. That is, as articulated by Braga (2012: 205–206), in addition to increasing the risk of offending by direct communications with gang leaders and members, the focused deterrence approach emphasizes the following points:

- (1) The opportunity structures for crime and violence need to be decreased simultaneously (e.g., through more extensive guardianship, increased formal and informal surveillance, and reduced anonymity of offenders).
- (2) Offenders need to be deflected away from crime (Corsaro and Engel, 2015, report that 25 of the 158 individuals who attended the offender notification sessions both signed up and actively participated in or received various social services extended to session participants).
- (3) Local communities can control crime effectively (e.g., through reducing social and physical disorder and increasing the sense of residents that unrestrained violations of social order will be attended to if brought to the attention of authorities).
- (4) The legitimacy of police actions can be increased (e.g., by increasing the likelihood that offenders will “buy in” and comply voluntarily with prosocial, antiviolence norms).

Of these four pathways, the evidence provided by Corsaro and Engel (2015) indicates the operative presence of the first and second in the New Orleans GVRS. It can be surmised that the third and fourth pathways were activated as well, but there is no direct evidence thereof. As Corsaro and Engel note, more direct and precise measures are called for in future focused deterrence intervention studies.

Third, Corsaro and Engel (2015) report that, simultaneous with the implementation of the GVRS, New Orleans also supported the *CURE Violence* model murder reduction strategy, which used violence interrupters and outreach workers to mediate between conflicting groups within the Central City area. However, the empirical analyses do not support an independent contribution of conflict mediation to the reduction of total and GMI homicides in the Central City area. This does not mean that there are no circumstances in which conflict mediation can be effective. Rather, it is consistent with the proposition that chronic

violent felony offenders may be less responsive to efforts that do not increase perceived risks of sanctions for offending.

Fourth, Corsaro and Engel (2015) remark on the persistence and commitment of the New Orleans political and police officials to the GVRS intervention. I agree fully that this essential and key element is necessary to ensure that focused deterrence programs are effective. Indeed, as my colleagues and I found in our evaluation of an intensive protective supervision program for juvenile status offenders, an intervention program is only as good and effective as the personnel who manage the program and carry on its daily tasks (Land, McCall, and Williams, 1992). The key point is that focused deterrence intervention programs are human programs staffed by humans who have limited capacities for extended periods of emotional and physical commitment and enthusiasm for the day-to-day work of the programs. When enthusiasm and physical commitment decline, that is, when the implementation, promotion, and monitoring tasks become bureaucratized and just part of one's daily work routine, the program treatment may deteriorate, with the effectiveness of the program correspondingly declining. Corsaro and Engel (2015) note that gang violence focused deterrence programs may be critically dependent on deep (i.e., emotionally charged) personal relationships that develop in the focused deterrence working groups and offender notification sessions. Routinization and bureaucratization of the work of these groups and sessions almost surely will be associated with declines in their effectiveness in reducing GMI lethal violence. The takeaway message is that the institutionalization of focused deterrence interventions as part of the violent reduction strategies of police departments must be accompanied with strategies to rejuvenate the commitments and energies of those involved. In other words, eternal vigilance is required to maintain the effectiveness of the interventions.

### **Concluding Remarks**

In sum, an accumulating body of evaluation studies supports the proposition that focused deterrence programs can work to reduce group and gang violence in large U.S. cities. Does this mean that all police departments in such urban contexts should have ongoing programs of this type? The answer is a qualified "yes"; such programs should be in the programmatic array of such departments, with the proviso being that the four takeaways noted previously are attended to. In addition, recognizing that the declines in U.S. homicide and other crime rates since the early 1990s have produced stronger associations of structural covariates of cities (and neighborhoods within cities) with the rates (McCall, Land, and Parker, 2010), it is natural to ask whether the strategies of focused violence deterrence programs such as merging crime analysis with criminal intelligence and the proactive targeting of problem areas and hot spots as well as repeat offenders can reduce violent crime even more. Recent research (Groff et al., 2015) has suggested that this promising strategy should be studied more extensively. So, as an echo of the Cajun expression *Laissez les bon temps roulez* associated with New Orleans, let the focused deterrence programs and related policing strategies roll, with eternal vigilance to maintain their effectiveness.

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# EDITORIAL INTRODUCTION

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## CHICAGO'S GROUP VIOLENCE REDUCTION STRATEGY

### To Shoot or Not to Shoot; Gang Decisions, Decisions

**James C. Howell**

*National Gang Center*

Papachristos and Kirk (2015, this issue) report that the Violence Reduction Strategy (VRS) implemented in Chicago reduced gang violence. The two prominent features of the VRS are a fine-grained gang problem assessment leading to targeting of gangs with small co-offending networks and enhanced therapeutic ingredients in the intervention. Although neither of these innovations represents modifications of the recognized effective parent Boston Ceasefire strategy (Braga and Weisburd, 2012), the original model is advanced, particularly in these two important components, and remarkably, it has had positive outcomes in Chicago, one of our nation's two gang capitals (along with Los Angeles).

The "gang audit" method Papachristos and Kirk (2015) use has its foundation in social network analysis (SNA), dating back to the classic work of Suttles (1968), *The Social Order of the Slum*, and continued in Boston (Kennedy, Braga, and Piehl, 1997; Papachristos, Braga, and Hureau, 2012). In the refined gang audit that builds on prior successful applications of SNA in Boston Ceasefire, Papachristos and Kirk identify disputes among smaller factions, as opposed to simply active offenders in large battling gangs, by using available data to determine *which* groups and *which* individuals were involved in current and ongoing shootings (who share a common experience: gunshot victimization) (Papachristos et al., 2012; Papachristos and Wildeman, 2014; Papachristos, Wildeman, and Roberto, 2015). For example, in one targeted police district, the audit process uncovered 35 active gang factions and 50 active disputes or feuds between factions. Next, individual members or associates of each faction, judged to possess sufficient standing in the group to carry the "don't shoot" message back to other group or network members, were selected through police gang intelligence as "influentials" to serve as the groups' representatives at the call-in.

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In the evaluation, Papachristos and Kirk (2015) use group propensity scores to match gang factions that attended a call-in to similar gang factions that did not attend a call-in. Gang factions participating in VRS were significantly less likely to be involved in shootings in the 12 months after call-in attendance compared with otherwise similar factions in the same general section of the city that did not participate in VRS. In sum, Papachristos and Kirk find that the Chicago VRS produced significant reductions in both shooting behavior and gunshot victimization in Chicago—despite the challenge in intervening with co-offending networks, which is a first in gang programming. In addition, the VRS placed greater emphasis on service access and matching these to offenders' needs on a case-by-case basis. These services included health, mental health, housing, drug treatment, education, and employment services, all of which are made available to those in attendance free of charge.

In their insightful policy essay, Gravel and Tita (2015, this issue) make several important observations with future replications of the Boston Ceasefire model in mind, two of which are highlighted here. First, they remind us that program evaluations should fully describe the contents of the “black box”—the program's theory of change. They underscore that the Boston Ceasefire model expects “[t]hose who attend the meeting to carry back to their gangs a message that law enforcement is serious about taking advantage of these vulnerabilities against the entire gang if the violence does not stop.” This diffusion process was not documented across gangs in this evaluation. Second, viewed through the lens of high scientific standards, Gravel and Tita note that Papachristos and Kirk's (2015) propensity score matching did not produce precisely equivalent control groups. Indeed, this is a common issue when scientific research designs are taken into the real world of everyday practice; adjustments are common, although the group differences did not obviate program effectiveness in this case.

In their provocative policy essay, Griffiths and Christian (2015, this issue) remind us that, “[i]f law enforcement tactics are perceived as aggressive, unwarranted, or contrary to civil liberties by persons in these licit networks, then larger concerns about the use of threat-based initiatives may diffuse into the broader community” and undermine procedural justice. Griffiths and Christian ponder this real possibility under the current not-so-rhetorical question: “Why do they run?” The critical issue they raise is as follows: How much do running proclivities discourage positive responses to call-in invitations, interrupt diffusion of the “don't shoot” message, and possibly create fissures in police–community relations?

Maximizing benefits for participating offenders is important—the “carrot” component of the focused deterrence program. VRS staff not only coordinated a variety of services for members of gang factions who wanted them, but also case workers helped tailor service plans to offenders who came forward at the conclusion of the call-ins or later—totaling approximately half of the participants. This service component is a recognized strength of the Comprehensive Gang Program Model (CGPM). In CGPM sites that reduced gang violence (including Chicago), a balance of services and socioeconomic opportunities along

with sanctions was delivered (Spergel, Wa, and Sosa, 2006). Having observed the importance of opportunities provision in Boston, Braga and Hureau (2012) made a strong case for integrating the CGPM and Operation Ceasefire. This model and several other programs have demonstrated evidence of effectiveness in reducing gang crime and, thus, are candidates for inclusion, as appropriate, in cities and counties that wish to build a continuum of prevention, intervention, and suppression services and sanctions (Howell and Griffiths, 2016). Given the chronic serious gang problems in nearly 200 large U.S. cities (Howell, 2015), accounting for one in four total homicides therein (Howell, Egley, Tita, and Griffiths, 2011), and with no signs of abating (Egley, Howell, and Harris, 2014), a study showing the effectiveness of the VRS version of Operation Ceasefire in Chicago, where street gangs have been institutionalized for more than a half-century, is very good news.

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# Changing the Street Dynamic

## Evaluating Chicago's Group Violence Reduction Strategy

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### Research Summary

*This study uses a quasi-experimental design to evaluate the efficacy of Chicago's Group Violence Reduction Strategy (VRS), a gun violence reduction program that delivers a focused-deterrence and legitimacy-based message to gang factions through a series of hour-long "call-ins." The results suggest that those gang factions who attend a VRS call-in experience a 23% reduction in overall shooting behavior and a 32% reduction in gunshot victimization in the year after treatment compared with similar factions.*

### Policy Implications

*Gun violence in U.S. cities often is concentrated in small geographic areas and in small networks of group or gang-involved individuals. The results of this study suggest that focused intervention efforts such as VRS can produce significant reductions in gun violence, but especially gunshot victimization, among gangs. Focused programs such as*

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*these offer an important alternative to broad-sweeping practices or policies that might otherwise expand the use of the criminal justice system.*

In August 2010, local and national press slammed Chicago Police Superintendent Jody Weis for a meeting he held with approximately six gang members at a park field house on the city's west side. At this "secret gang summit," as one newspaper branded it (Bryne and Ford, 2010), Weis and a group of law enforcement representatives, community members, and service providers met with gang members in an effort to quell escalating gang violence. One side of the political spectrum denounced Weis for "negotiating with terrorists" (The Huffington Post, 2010). Police, some said, were molycoddling gang members when they should be locking them up. "I can't believe we're sitting down and negotiating with urban terrorists who are killing our kids with guns and drugs on the streets," remarked one City Councilperson (Robinson, 2010). Meanwhile, gang members and other street activists hosted their own press conference, charging that police were unconstitutionally targeting gang members as well as threatening to charge members of a gang with the crimes of their associates. "The police aren't playing fair," leveled one activist, asking "how gang leaders could be asked to take responsibility for their subordinates when city government leaders don't take responsibility for alleged misdeeds by their employees" (Allen, 2010; theGrio, 2010).

The meeting in question was not, in fact, some secret backroom parlay between police and gangs; instead, it was the first Violence Reduction Strategy (VRS) "call-in," Chicago's incarnation of an increasingly popular gun violence reduction strategy that gained popularity in Boston, Massachusetts, and has since been replicated in other cities across the country (Braga, Hureau, and Papachristos, 2013; Engel, Tillyer, and Corsaro, 2013; Kennedy, 2011). The Chicago call-in brought together a group of individuals known to be members or associates of street gangs currently involved in violent disputes to meet with representatives from law enforcement, the community (including the families of victims), and social service providers. The objective of a VRS call-in is simple: deliver a message to gang-involved individuals about the present gun violence situation and tell them, in no uncertain terms, to put down the guns. There were no negotiations, deals, or breaks. The hour-long meeting took place not at a police station or courtroom but at Garfield Park Observatory, one of the city's most stunning public spaces. Everyone went home at the end of the day. No one was arrested or detained against their will.

Attendees of the meeting, nearly all of whom tend to be on probation or parole, were told that police are aware of the ongoing disputes and of their group's current role in such violence. A focused deterrence message is conveyed to attendees that stresses that the next shooting by their group will elicit the full attention of the criminal justice system to use every available legal means to go after the entire group, including arresting members, pulling warrants, revoking parole/probation, and increasing overall pressure on the group (Braga,

Kennedy, Waring, and Piehl, 2001). Representatives from the community are also present, who express their desire to help the attendees and stress their love for them. “You’re part of this community. Our community. Our families. And, we love you,” one mother of a murder victim told the room, showing pictures of her fallen son while fighting back tears. Service providers in the room urge attendees to take advantage of the offer for help—immediately.

In contrast to media reports, the VRS “call-ins” were not entirely new in Chicago. Meetings with a somewhat different focus and target population have been ongoing in Chicago since 2002 as part of Project Safe Neighborhoods (PSN). In fact, a quasi-experimental evaluation of PSN found that the initiative yielded double-digit reductions in homicide in targeted geographic areas (Papachristos, Meares, and Fagan, 2007). The novelty of the VRS call-in was its specific focus on *groups* and its use of new analytic tools to guide the intervention. Specifically, VRS sought to use new data-driven methods—including social network analysis—that fostered a more precise focus of intervention efforts on those groups actively involved in shootings.

The hope of VRS was that these new analytic tools coupled with a novel intervention would go far in reducing gun violence in the Windy City. Proponents of the VRS approach argued that the dynamic of gun violence in most American cities was driven by interpersonal and intergroup disputes that were settled through gunplay; although the specific contexts of such disputes vary by locale, the central street dynamics were the same (Kennedy, 2011). If the street dynamic changes, then gun violence will decrease. Opponents of this approach argued that Chicago is too unique: what worked in Boston, Massachusetts; High Point, North Carolina; or Cincinnati, Ohio will not work in Al Capone’s city. Chicago gangs have been around nearly a half-century; they are too entrenched in the city, too involved in large-scale drug dealing, and simply too violent and unpredictable to be amenable to such an intervention. Chicago’s slight rise in homicide in 2012 seemed to illustrate this point.

This study evaluates the efficacy of Chicago VRS at reducing gun violence by using a quasi-experiment to determine whether those gangs attending a call-in experienced the hypothesized reduction in shooting behavior. Put another way, did VRS change the street dynamics among gangs in Chicago? VRS call-ins have been in continuous operation since the initial August 2010 meeting. Through 2013, 18 call-ins reached 149 gang factions and 438 individual gang members. To analyze changes in both victimization and offending, we use a propensity score matching procedure to match gang factions that attended a call-in to up to three otherwise similar gang factions that did not attend a call-in. Our analyses find that gang factions participating in VRS were significantly less likely to be involved in shootings in the 12 months after call-in attendance than otherwise similar factions that did not participate in VRS.

### **Homicide, Gangs, and Guns in Chicago**

Regardless of its actual violent crime rate, the media, political pundits, popular culture, and at times even academics frequently portray Chicago as one of the country’s most violent

cities. Statistically, crime trends in Chicago mirror the overall national crime decline of the past two decades. In fact, rates of violent crime and homicide in present-day Chicago are currently at the lowest recorded levels in nearly five decades (Papachristos, 2013). To be sure, Chicago tallies a greater *number* of total murders than other cities of comparable size (e.g., Los Angeles, California, and Houston, Texas) and more than New York, which has a population three times its size. The city's overall rates of both violent crime and homicide surpass national averages. But when controlling for population, Chicago's homicide rate does not breach the top 10 most violent cities in the United States. In 2012, the year many branded Chicago the country's "murder capital," Chicago's violent crime rate ranked 19th among law enforcement agencies serving jurisdictions of 250,000 or more—rates similar to those of Houston or Minneapolis, Minnesota, and far lower than Detroit, Michigan; Oakland, California; or St. Louis, Missouri (see Table A1).

Such declining crime rates and city-level comparisons, however, mask more severe disparities in crime and violence *across* Chicago communities. In Chicago, as in most other cities across the country, crime rates vary tremendously by neighborhoods (for a review, see Peterson and Krivo, 2010). Also like most cities, homicide and violent crime in Chicago concentrate in a small number of neighborhoods and geographic microplaces (Kirk and Papachristos, 2011; Morenoff, Sampson, and Raudenbush, 2001; Sampson, 2012). For instance, Garfield Park, on the city's west side, had a 2012 homicide rate of 55 per 100,000, more than three times higher than the city average (approximately 16 per 100,000) and more than 10 times higher than the national average (approximately 5 per 100,000). Meanwhile, Jefferson Park, on the city's northwest side, had a homicide rate of effectively zero. Research since the work of the early Chicago School sociologists documents the remarkable stability of the high crime parts of the city over long periods of time (for a review, see Sampson, 2012). Although nearly all of the high-crime communities also experienced significant declines in crime over recent years, the rates in some high-crime communities—like Garfield Park—remain stubbornly high, generating what some have called a "crime gap" between the safest and most dangerous neighborhoods of the city (Papachristos, 2014).

Homicide and violent crime in Chicago concentrate not just spatially but also socially. Criminological research since Wolfgang's (1958) classic study of offenders in Philadelphia has revealed that a large portion of crime is committed by a small number of offenders—a finding that seems to be as true today as it was nearly five decades ago and applies to cities across the country. Recent research employing social network analysis has extended this logic by examining the exact contours of co-offending networks and the placement of shooting victims within them. A study of one high-crime Boston community, for instance, found that 85% of all fatal and nonfatal gunshot injuries occurred in a single network of individuals who had been arrested that comprised less than 5% of the community's total population (Papachristos, Braga, and Hureau, 2012). Likewise, Papachristos, Wildeman, and Roberto (2015) found that 70% of all nonfatal shootings in the entire city of Chicago occurred in a co-offending network composed of less than 6% of the city's population.

FIGURE 1

**Number of Total Homicides, Homicides Involving a Gang Member, and Homicides Not Involving a Gang Member in Chicago, 1994 to 2013**



Most strikingly, this line of research found that simply *being in* such networks exponentially increases the likelihood that one becomes a victim of a gunshot injury; in the Chicago study, for instance, being in a network with another gunshot victim increases the probability of being a victim a staggering 900% (Papachristos et al., 2015).

Although the exact estimates vary, there is mounting consensus that a large portion of gun violence and homicide in Chicago is driven by street gangs, either by gang-motivated behavior (such as turf disputes) or the involvement of gang members in group and non-group-related interpersonal disputes (Block and Block, 1995; Papachristos and Kirk, 2006). Figure 1, for instance, displays homicides in Chicago since 1994 disaggregated by whether it was “gang member involved,” meaning that a member of gang was involved as either a victim or an offender. As just described, total homicides in Chicago have declined steadily since 1994 with a few smaller peaks in 2002, 2008, and most recently in 2012. Disaggregating by whether the homicide involved at least one gang member shows that non-gang-involved homicides more closely followed the citywide trend, whereas gang-involved homicides trended upward in 2000 and have remained relatively stable. So, for instance, since the spike in 2002, the yearly number of gang homicides has only declined by 16%, whereas non-gang homicides have declined by nearly 36%. This decrease has a significant impact on the

percentage of total homicides that currently involve a gang member—today, compared with the 1990s, gang-involved homicides constitute a greater percentage of the total homicides in Chicago, roughly 50% to 60%. As such, changes in gang homicide can generate spikes in the overall homicide rate, as observed most recently in 2012. Hence, to stem the tide of violence in Chicago, interventions need to be directed toward altering the dynamics leading to group violence.

Part of Chicago's image as one of the most violent cities in the nation stems precisely from the reputation of its gangs. Gangs in Chicago have been consistently reported as being more organized and more heavily involved in organized levels of drug dealing than gangs in most other cities (Fagan, 1989; Howell, 2012; Spergel, 1995). Many modern-day Chicago gangs—like the Vice Lords, the Black P. Stone Nation, the Latin Kings, and the Gangster Disciples—trace their origins to the late 1950s and have been involved in a variety of prosocial, political, and criminal activities across the decades (Dawley, 1973; Hagedorn, 2008; Moore and Williams, 2011). In the late 1980s and early 1990s, many of these gangs entered the drug game by orchestrating sophisticated drug-dealing enterprises complete with complex distribution practices, rules and regulations, and violent methods of dispute resolution (Levitt and Venkatesh, 2000; Venkatesh and Levitt, 2000). In some ways, Chicago gangs represent the “worst” of what gangs could become and not, in fact, what the typical American street gang looks like.

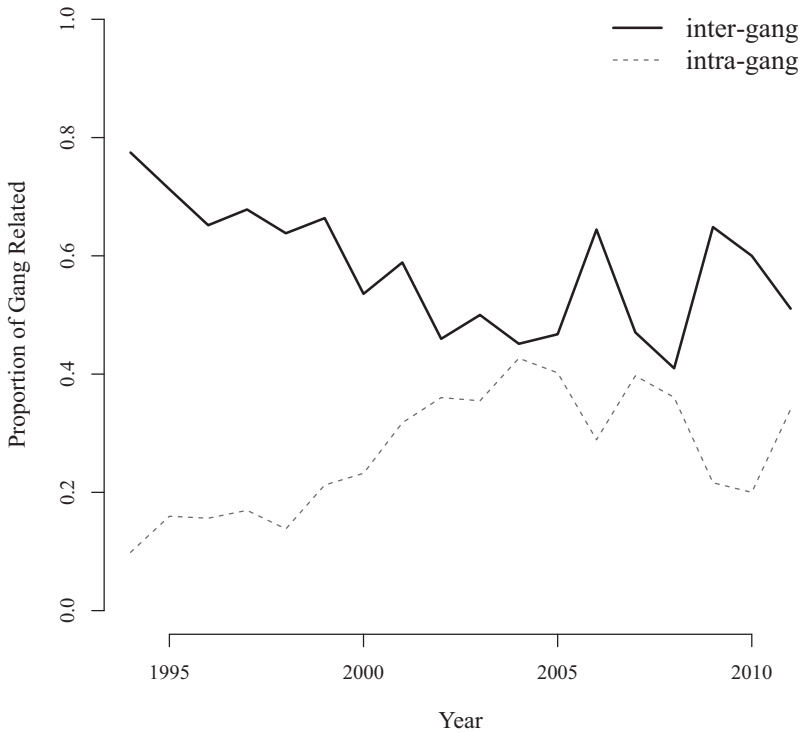
However, in recent years, Chicago has witnessed important changes in the nature of its gangs and gang-involved violence. One trend noted by police officials is the splintering of once large gang entities into smaller “factions” or geographically bounded crews. During the height of the crack cocaine era, many Chicago gangs operated under a “corporate” style of operation or, at least, with more formal hierarchical structures—leaders, subgroups, line workers, and so on (Venkatesh and Levitt, 2000). Power was concentrated in the hands of a small number of older gang members—some of whom were incarcerated during their reigns—whereas often younger members assumed the risky “on the street” drug-dealing and violence-related activities. These hierarchical structures seem to have receded during the past decade; many of the larger groups have splintered into smaller factions that operate, for the most part, independently. For example, in the 1990s, the Gangster Disciples prided themselves on their “Board of Directors” and system of “Governors” and “street taxes” that coordinated thousands of members across the city. Today, however, the Gangster Disciples name is more of a “brand” than a functioning organizational structure. Factions still use the Disciple moniker, to be sure. But the main identity has become the local or small group—e.g., The Guttaville Disciples, the 80s Babies Disciples, and so on.<sup>1</sup>

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1. The causes of this gang splintering seem to be diverse and include (a) long-term effects of gang prosecutions and enforcement actions, (b) changes in local and global drug markets, (c) internal conflicts among gang leadership, and (d) the general fading of large gang alliances over time. In many

FIGURE 2

## Intergang versus Intragang Homicides in Chicago, 1994 to 2010



*Note.* A homicide was defined “inter-gang” when the victim and the offender were from distinct (nonaffiliated) gang groups or factions and “intra-gang” when the victim and the offender were from either (a) the same gang or faction or (b) affiliated gangs or factions.

This splintering of gangs has had a profound effect on the dynamics driving violence on the street. Today, compared with 20 years ago, gang violence is more likely to occur *within* gangs or gang divisions (or between gangs with some affiliation) than it is between distinct gangs. Figure 2 plots inter-gang versus intra-gang homicides in Chicago from 1994 to 2010. In this figure, “intra-gang” refers to any homicide in which the victim and the offender belonged to the *same* gang faction or *related* gang factions (gang factions that share some common ancestry of past alliance—i.e., members of the same gang “nation” such as the Gangster Disciples). “Inter-gang” homicide refers to a homicide in which the victim and the offender belonged to gang factions with no shared alliance or ancestry. This figure

ways, gang factions in Chicago today are beginning to resemble gangs in other cities in that they are increasingly becoming smaller in size and the locus of control.

shows that since the mid-1990s, the number of inter-gang homicides has declined steadily as the number of intra-gang homicides has increased. The two almost converge circa 2004 and have meandered up and down since.

Recent fluctuations notwithstanding, Figure 2 has two important implications for understanding gangs, gang violence, and the street dynamic among gangs. First, the unit of analysis of what constitutes a meaningful point of intervention has changed. Since the 1960s, police in Chicago often have considered “the gang” the largest meaningful unit. Gang nations—like the Gangster Disciples—represent, essentially, federations of gangs. Gang members and their groups were lumped into nation units: A member of the Disciples was considered by police (and, importantly, police data systems) to be a Disciple. But the splintering of gangs has shifted the focus to smaller, often neighborhood-bounded factions that themselves have unique identities, names, and behaviors. Thus, it matters more whether a member is of the Guttavilla Disciples or the 80s Babies Disciples, as the nation as a whole seems no longer to direct organizational behavior in the same way.

Second, understanding faction-level behavior means rethinking group dynamics in Chicago. For decades, gang violence in Chicago has been characterized along first categorical gang nation distinctions: the Disciples versus the Stones, the Latin Kings versus The Latin Saints, and so on. Enforcement and prevention efforts directed resources accordingly, focusing on large organizational behaviors. In contrast, faction-level disputes more closely resemble “family feuds” that tend to be more personal and localized. History still matters, to be sure, but what is happening on the street *today* often provides the spark for feuds and violence. If such types of faction-level disputes are increasingly drivers of gang homicides in Chicago, as Figure 2 suggests, then understanding the proximal motivators for gang disputes means rethinking how we conceive of gang disputes. We must move away from 1980s and 1990s notions of gang disputes in Chicago being motivated purely around the crack trade and age-old vendettas and toward an understanding of the microdynamics of small group conflicts.

Taken together, these trends broadly summarize the current homicide and gun violence problem in Chicago. Despite impressive declines in homicide and violent crime since the 1990s, crime and violence (a) concentrate in a small number of communities and in small social networks, (b) involve a large number of gangs and gang members, and (c) are increasingly driven by disputes among smaller gang groups and factions as opposed to large battling gang nations. Therefore, changing the street dynamics driving gun violence requires engaging these issues in programmatic design and implementation.

### **Program Intent (and Its Effectiveness)**

VRS, like many violence prevention and policing efforts, prides itself on being “data driven.” This buzzword translates into many different forms, often with an eye toward appeasing funding agencies that understand this phrase to mean that practitioners will use data in the planning, implementation, and evaluation of their programs. A successful program is



evidenced by a decline in the targeted crime type or of crime rates in a specified location. The extent to which any specific program is data driven derives, in part, from how much data are available, whether data are analyzed thoroughly or cursorily, and whether participants engage with said data and analytics.

For Chicago VRS, the idea of being “data driven” meant using all available data to identify specific individuals and groups who are actively involved in gun-related disputes and violence in as close to “real time” as possible. VRS did not seek to analyze a series of blanket risk factors for its intervention; it has long been well established that young minority males in specific parts of the city and belonging to street gangs were the most likely victims and perpetrators of violence (Block and Block, 1995; Morenoff et al., 2001; Papachristos and Kirk, 2006). From VRS’s perspective, going to the city’s disadvantaged and high-crime communities to look for street gangs was not a focused strategy. Rather, VRS sought to use the available data to determine *which* individuals and *which* groups were involved in current and ongoing shootings to provide precise and strategic points of interventions. Thus, knowing that “gangs in Englewood” were fighting was insufficient. VRS wanted to know whether a dispute between the Disciples on 67th Street and a “renegade” set of Disciples from 71st Street was responsible for the violence. The entire premise of changing the street dynamics behind gun violence in Chicago is first to use data to determine the actors and disputes of said violence and then to bring the VRS message directly to those involved groups.

This idea of bringing the program and its message directly to those involved in gun violence is based on the principle of focused deterrence (for a review, see Braga and Weisburd, 2012). Unlike general deterrence, which aims to dissuade the general population from engaging in particular criminal behaviors by increasing the severity, certainty, and swiftness of punishments associated with said crime, *focused deterrence* posits that crime reduction is best achieved by concentrating deterrence efforts on those groups or individuals involved directly in the targeted type of crime. Rather than enact broad-sweeping policies that indiscriminately apply across populations and places, focused deterrence efforts honor traditional deterrence principles while leveraging existing policies and practices in innovative ways directly toward small offending populations. The Chicago VRS program based its deterrence principles on those pioneered in the Boston Operation Ceasefire efforts of the 1990s, which was designed to reach out directly to gangs involved in ongoing shootings, saying that gun violence would no longer be tolerated, and then following through on such actions by “pulling every lever” legally available when gun violence occurred (Braga et al., 2001; Kennedy, 2011).

Chicago (Papachristos et al., 2007); Los Angeles (Tita, Riley, and Greenwood, 2003); Indianapolis, Indiana (Corsaro and McGarrell, 2009; McGarrell and Chermak, 2003); High Point (Corsaro, Hunt, Hipple, and McGarrell, 2012); and other cities (Braga, McDevitt, and Pierce, 2006; Engel et al., 2013) that have replicated some version of the original Boston Ceasefire approach typically deliver a deterrence message to individuals or groups

through “call-in” or notification meetings. To summarize, these meetings are the vehicle for transmitting the message. Although specifics vary within each program, usually a brief meeting is held between a group of targeted offenders and a collective of law enforcement officials, community representatives, and social service providers. Some programs stress the enforcement side of the message, whereas others balance the deterrence message with a strong “moral voice” and service provider element that hopes to provide choices and options that might help steer offenders along more prosocial paths (Crandall and Wong, 2012). Thus, in addition to “pulling every lever,” programs are also trying to provide possible alternatives that might aid the desistance process.

In addition to the message itself, specific attention is given to *how* the message is delivered. In particular, the Project Safe Neighborhood (PSN) initiative in Chicago tried to balance the focused deterrence message with principles of procedural justice and legitimacy under the guiding principle that a deterrence message will be better received if the *process* of delivering the message is fair and the actors delivering the message are perceived as acting justly (Papachristos et al., 2007). Chicago PSN designed the architecture of its focused deterrence-style meetings explicitly to embody such principles by (a) holding the meetings in a place of civic importance, such as a park, school, or local community institution, as opposed to a criminal justice facility; (b) organizing the meeting room in either a round-table format or a small classroom, as opposed to a court room or large lecture hall; and (c) scripting the actual language of the meeting to balance the enforcement, community, and service aspects.

A report issued by the National Academy of Sciences found the accumulation of evaluation evidence on the focused deterrence approach “compelling” (Wellford, Pepper, and Petrie, 2005: 10); moreover, this evidence seems to exert “very positive” effects in reducing gun-related crime and violence (Braga and Weisburd, 2012: 347). Recently, Braga and Weisburd (2012) conducted a meta-analysis of all focused deterrence programs using a quasi-experimental evaluation design and found demonstrable program effects in 10 of the 11 programs. Although additional evaluation research is clearly needed—especially those with more fully developed experimental and quasi-experimental designs—many of the programs cited in Braga and Weisburd’s meta-analysis posted double-digit declines in crime. For example, the original Boston Ceasefire calculated a 65% overall reduction in youth homicides, 25% reduction in gun assaults, and 32% reduction in 9-1-1 calls for shots fired during the observation period (Braga et al., 2001). An Indianapolis program witnessed a 34% reduction in citywide homicide rates compared with six other Midwestern cities (McGarrell, Chermak, Wilson, and Corsaro, 2006). Operation “Peacekeeper” in Stockton, California, experienced a 42% reduction in gun homicides compared with eight other cities in California with similar populations (Braga, 2008). An evaluation of Cincinnati’s Initiative to Reduce Violence was credited with a 35% reduction in gang member involved shootings compared with trends in non-gang-member-involved shootings (Engel et al., 2013). Total homicides in Chicago’s PSN target communities, in which repeat gun offenders

returning from prison were selected randomly to attend a notification meeting, decreased 37% compared with a set of comparison neighborhoods in Chicago (Papachristos et al., 2007). A quasi-experimental evaluation of the retooled Boston Ceasefire by Braga and colleagues (2013) found a 31% reduction in total shooting involvement of those gangs that were the focus of the program compared with a matched control group.<sup>2</sup>

### Intervention

Guided by the principle that reducing gun violence in Chicago entails bringing a “don’t shoot” message to those involved in the street dynamics currently driving shootings, the Chicago VRS team faced the daunting questions raised by (some) opponents of the program: How would these principles translate into the vast gang situation in present-day Chicago?

The intervention would remain true to the form embodied in Boston and other locations, but the content (and context) of the message was tailored to Chicago’s unique gang landscape. Given the sheer size of both the city of Chicago and its gang problem, the intervention would have to be even more focused than previous efforts. The intervention itself would take the form of a call-in in which a collaborative group of law enforcement officials from the state, local, and federal levels; community stakeholders; and service providers would convey the message.<sup>3</sup> The VRS effort was to be led by a group of non-law-enforcement professionals affiliated with the John Jay College of Criminal Justice’s National Network for Safe Communities, whose job it was to (a) work with police, researchers, and other gang experts to analyze current shooting patterns; (b) organize call-ins in a timely fashion, including inviting individuals as attendees; (c) follow up with various service providers and police; and (d) coordinate the various stakeholders in their VRS-related activities, including participation at the call-ins.

The call-ins followed the model described previously: one-hour-long meetings with groups of approximately 15 to 20 individuals affiliated with gang factions currently involved in (as either victim or offender) shootings. True to the legitimacy and procedural justice elements of the program, the VRS team elected to hold such meetings in a place of civic importance, such as a park, library, nonprofit organization, or school, rather than in a police station or courthouse. The VRS team strongly believes that the *setting* of the message relays important information: Despite any individual’s label or status as a gang member, the VRS team acknowledges that the attendees are members of the community and will be treated as such unless they choose to pick up a gun to settle a dispute. Part of the design of the

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2. Several of these evaluations suggested that the observed effects of such strategies vary by the timing of the intervention as well as by the dosage of treatment calling for the heightened importance of quasi-experimental designs such as the one used in the present study (Braga et al., 2013; Corsaro et al., 2012).

3. See Crandall and Wong (2012) for a discussion of the structure of such call-ins and the process for such coalition building.

program was to enhance the legitimacy of such programs precisely by changing where the meetings were held and how the message was delivered.

The structure of the message followed past programs and is divided into three explicit sections: the enforcement component, the community moral voice, and the social service component. The meeting is moderated by a VRS staff member who acts as a sort of coach or moderator of the event. As a non-enforcement member, the moderator makes the transitions between the segments; this individual repeats and stresses key points, and he or she ensures that everyone stays on task. The VRS moderator begins the meeting with a call to order, ensuring everyone they will go home at the end of the hour. For example, at the beginning of one call-in, the moderator said: "This isn't a trick. Everyone gets to go home tonight. So relax a bit. We're here to talk to you about one thing: gun violence. No tricks. Just some straight talk, and an offer to help." The moderator goes on to explain why those specific people are in their respective seats—i.e., that they are somehow connected to a network of factions (or affiliated with such factions) that have been involved in recent shootings. They are at the call-in meeting as representatives of those factions or because they are "influential" in the faction networks. The moderator then shows attendees a picture of such networks (such as in Figure 3) to give the attendees a sense of just how much those in the room know of the ongoing disputes.

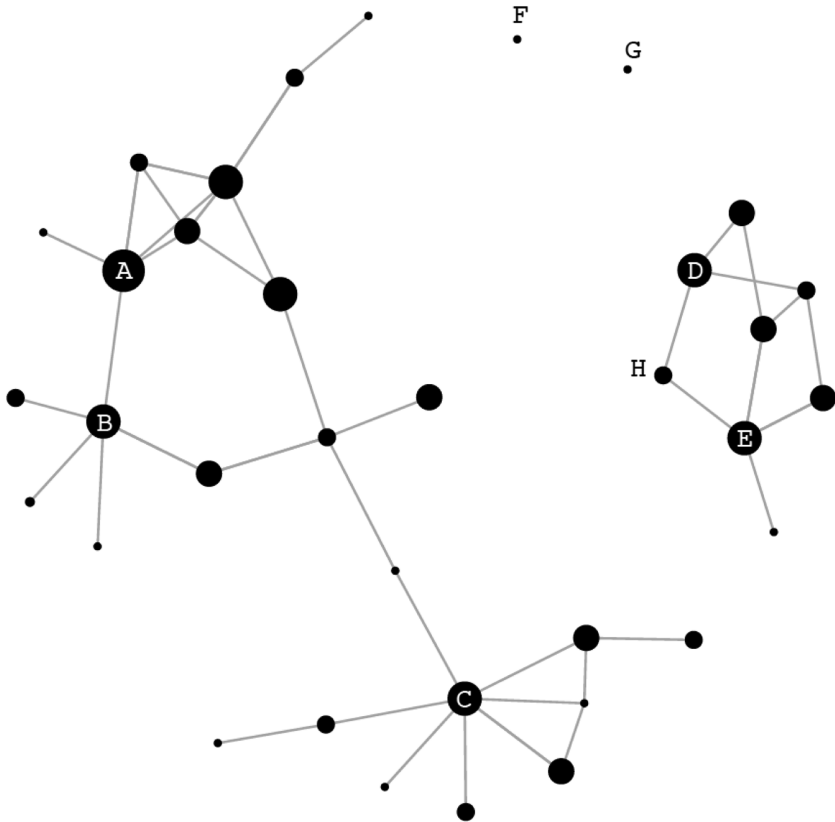
At this point, the call-in switches to the enforcement component and representatives from local, state, and federal law enforcement each take a turn explaining how their respective agency might be deployed against the various factions in the event of the next shooting. A federal partner, typically from the U.S. Attorney's Office, explains how federal statutes might be leveraged against the faction, including continued criminal enterprise and armed career criminal statutes. The point of this message stresses the deterrent aspect of the program. Representatives from local police and prosecutors provide examples of recent cases and shootings to underscore the reach of the current violence and how they are working in a coordinated fashion with others in the room. All stress that gun cases in the area are getting their full attention.

After the enforcement component, the VRS moderator introduces representatives from the community, often family members of those killed or harmed by gun violence or other local activists and community members or organizations. This segment is typically the most emotional; community actors retell their experiences around the loss of a loved one and the damage gun violence causes families and the community. A mixture of anger, pain, and frustration are bundled with a sense of hope as the community members close their segment with a notion of forgiveness, understanding, and love. At one call-in in 2010, a mother who had lost her 17-year-old son concluded by saying:

I'm angry as hell. But you know what? I love you. I love all you. You are the men of our community. We want you here, not in jail or in the ground. We know what's inside of you. You might not always think about what you're doing, but

FIGURE 3

## Example Faction Conflict Network Derived from Gang Audit



we know you. These people [pointing to others in the room]. They here to help you. Didn't no one try to help [my boy.] You better listen up: Because they here trying to save you too. We all are.

After the community representative finishes, the VRS moderator again takes the lead and stresses the “don't shoot” message. The final segment of the call-in centers on making good choices as the moderator reinforces that people—ex-offenders and gang members like those sitting in attendance—have put down their guns and turned things around. “If you want help, it's here for you” is a reoccurring theme as the moderator introduces a series of service providers in the room who go on to discuss their respective organization's goals and services. Services include a range of health, mental health, housing, drug treatment, education, and employment services, all of which are made available to those in attendance free of charge. VRS staff offer to coordinate these services for anyone in the room who

wants them, and case workers are available to help individuals tailor service plans to their needs and follow up with them after the meeting.

At the end of the forum, the moderator thanks everyone in the room, stresses that people “really think on what they heard,” and insists that they “spread the word” among those in their respective factions. Importantly, those present are continually reminded to “take the message back” to their groups and factions. When the meeting ends, many in the room make a beeline for the exit. But some participants linger, staring at their feet and waiting to make contact with a call-in speaker or service provider. According to VRS staff members, more than 50% of attendees take advantage of services in one way or another.

### *Looking for Gang Factions*

The basic structure of the VRS meeting, its message, and its general architecture have much in common with many of the prior focused deterrence- and legitimacy-based efforts reviewed in a previous section. One of the most striking innovations of Chicago's VRS, however, was its desire to leverage data on gang factions and current episodes of violence to select attendees for the call-in. The overarching goal was to leverage all possible data to understand the current street dynamics of Chicago gangs described in the previous section in order to identify those factions actively involved in shootings. For this purpose, the VRS teamed turned to a process referred to as a *gang audit*.

During the past decade, a technique known as a *group or gang audit* has been developed as part of focused deterrence-style programs with the explicit goal of extracting on-the-ground or experiential knowledge out of the heads of gang experts (such as case workers, police, and program officers) to analyze current shooting patterns; specifically, Which groups are involved in current shootings? Where do they hang out? What are the motives behind the shootings (Kennedy, Braga, and Piehl, 1997; Sierra-Arevalo and Papachristos, 2015)? The audit process is, essentially, a focus-group-style process led by the VRS team and researchers. The typical audit process begins with a large map of a specified geographic area. The researchers lead the group through an exercise with the following goals: (a) identifying all gang factions that exist or operate in the specified geographic area; (b) gathering information on the membership of said factions and their (illegal) activities; (c) locating important gang-related locations, pieces of turf, or activity centers; and (d) mapping interfaction relationships—i.e., alliances, disputes, mergers, fracturing, and so on. The researchers record and code the responses for subsequent analysis but allow the experts to work out details of specific gangs as a group. The VRS team and researchers probe with clarifying questions, asking about specific relationships and events to complete a series of preidentified questions aimed at gathering information in the four previously mentioned domains.

One key objective of the audit process is to create a social network map of the “gang landscape”—the patterns of conflict and violence among gangs in the specified geographic area (Kennedy et al., 1997; Sierra-Arevalo and Papachristos, 2015). An example of such a

map of gang conflict for one Chicago community is shown in Figure 3, where each node represents a unique gang faction and each tie represents a unique dispute or conflict as identified in the audit process. The size of the node reflects each faction's nodal degree; in this case, it is the total number of current conflicts in which the gang is involved.

Figure 3 displays the patterns of conflict among the population of gangs for one of the city's 25 police districts (estimated population of 105,000 residents in 2010). The audit process uncovered 35 active gang factions in the district, where "active" means a faction was involved in some kind of illegal activity. The audit process uncovered 50 active disputes or feuds between factions represented by the edges or lines in Figure 3. Many of the officers and experts involved in the audit are familiar with specific factions and feuds—indeed, many are tasked with the precise goal of knowing everything there is to know about a particular faction. What the audit process reveals is how the *population* of gangs is connected. For instance, most individuals in the audit identified the dispute between factions A and B, but they might have been pressed to determine how that single conflict is, in fact, nested in a much larger network of faction disputes. Second, the audit process also reveals how gangs can be *indirectly* connected. Gangs D and E, for instance, share a common enemy in gang H. This sort of shared animosity drives alliance formation under the old adage of "the enemy of my enemy is my friend"—known in network terms as "transitivity" (Chase, 1980).

Gang audits were conducted citywide starting in fall 2009. The initial VRS program, however, began slowly in one police district, expanding only thereafter to other high-crime districts. The VRS program uses such audit data to focus its gaze—and its message—on those groups most active in violence within the targeted districts. Although no precise algorithm or computational method is used to select target factions, the VRS team chooses to direct its efforts at groups actively involved in conflicts as opposed to those who are not actively involved in gun violence. The underlying principle is to reach those factions that are involved in shootings, rather than simply reaching out to gang members writ large. The audit provides an initial step toward sharpening the program's focus: by identifying those factions currently involved in shootings.

Importantly, the audit process does not end with such network maps. Rather, the process is *iterative* with information going back and forth among analyst, police, and program staff. For instance, after identifying potential factions who might be part of the intervention, the VRS team crosschecks its information with police detectives, line staff, and even community contacts to ensure their portrait of the current street dynamic is as accurate as possible.

Once the identity of the participating factions has been established, the VRS team must identify individual members or associates of each faction who will serve as the group's representatives at the call-in. This, too, is done in an iterative manner that begins with names of members derived from the audit process that is then cross-checked against additional police data and intelligence. The goal is to select influential individuals, by which the VRS team means those faction members or affiliates who have some standing in the group

and are likely to bring the call-in message back to other group or network members. Many individuals in the gang network are well known to police, parole, and probation officers, and the VRS team goes through a vetting process to ensure they are generating potential candidates who fit this criteria. Once the VRS team has whittled down the list to approximately 40 individuals, the names are again cross-checked to make sure candidates are not currently in prison, under investigation, deceased, or acting as a confidential informant. Finally, the VRS team reaches out to probation and parole officers to help recruit candidates to participate in the meeting. Each selected individual receives a customized letter explaining the goals of VRS and a visit or call from their probation or parole officer inviting them to a call-in on a specified date and time. Probation and parole officers, as well as VRS staff, follow up with each invitee prior to the call-in to maximize participation.

### Research Design

Between August 17, 2010 and December 31, 2013, a total of 18 call-in meetings were held in Chicago; 149 gang factions (of 858 recognized factions in the city) had at least one member attend one of these call-ins with a total of 438 unique individuals having ever attended a call-in during this period. Because the program focused on specific gang factions and began in a limited number of police districts, it is not intended to decrease shooting behavior among *all* gang factions in the city—only those targeted by the intervention. This targeted nature of VRS affords a unique opportunity to test the efficacy of the VRS strategy and, perhaps, the larger theory behind it. The fact that VRS focused on only 17% of all gang factions leaves a large pool of potential comparison and control groups, especially in nontreatment districts. Thus, it affords a unique opportunity to develop a quasi-experimental research design.<sup>4</sup> Our study uses propensity score matching to compare the shooting behaviors of those gang factions who were part of the VRS program with factions that are similar on important characteristics but that were not part of the VRS program. In the current study, we compare the shooting behaviors of “treated” gang factions (as either victim or perpetrator) in the 12 months after call-in attendance with the shooting behavior of matched controls during the same 12-month time period. A programmatic effect would be attributable to a decrease in shooting behavior of the VRS target gang factions relative to the comparison or control gang factions. A null finding or an increase in shooting behavior would suggest evidence against a VRS program effect.

### Data

Data in this study came from three sources made available by the Chicago Police Department:

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4. In this way, the greater number of gang factions in Chicago allowed us to overcome one limitation experienced by Braga et al. (2013) in matching gangs in Boston. While both studies achieve comparable matching of groups, our design was able to match based on a larger number of possible groups.



TABLE 1

## Covariates Used in Propensity Score Matching

Aggravated Assault Arrests	Number of Faction Arrests for Aggravated Assault	N
Amount of turf controlled	Turf area in square feet	140
Average degree	Average degree (co-arrest) among faction members	149
Average path length	Average path length of all present paths	149
Chicago Police Department area 1	Gang faction located in central police area	141
Chicago Police Department area 2	Gang faction located in south police area	141
Chicago Police Department area 3	Gang faction located in north police area	141
Diameter of network	Longest geodesic distance	149
Drug arrests	Number of faction arrests for drug offenses	149
Faction—level of organization	Level of faction organization (low, medium, high), as determined by CPD	141
Faction—level of violence	Qualitative level of violence estimated by CPD (low, medium, high)	141
Faction size	The estimated number of members of the faction, as determined by CPD	130
Median age of faction members	Median age of members	149
Number of components in network	Number of components in faction-level co-arrest network	149
Other felony arrests	Number of faction arrests for other felonies	149
Racial composition of faction	Racial composition where "race" = > 66% of a given race	149
Robbery arrests	Number of faction arrests for robberies	149
Shootings - 2006 to 2010	Number of faction shootings (victim or suspect) between 2006 and 2010	149
Size of largest component	Size of largest component	149
Total number of active alliances	Total number of "active" alliances	141
Total number of active conflicts	Total number of "active" conflicts	141

Note. CPD = Chicago Police Department.

- (1) Incident-level records of all arrests in Chicago
- (2) Homicide and nonfatal shooting records
- (3) Additional faction information collected during gang audits

All of these data cover the period from January 1, 2006 to March 31, 2014. The unit of analysis is the *gang faction*; as described previously, the gang faction is believed to be the smallest and most meaningful action unit for gang members. Between 2009 and 2010, the VRS team and the Chicago Police Department completed citywide audits of gangs in each of the 22 (at the time 25) police districts covering the population of gang factions within each district, information on conflicts and alliances among these factions, and qualitative and quantitative information on each faction. These data were updated on a regular basis by each local police district, and factions were selected for VRS based on the most recently available data.

After identifying all unique factions, we created a faction-level database containing as much information about each faction as possible from the available data, including demographic, organizational, network, and crime involvement information (see Table 1). From the gang audit information, we created variables on each faction's overall "level of

organization" (low, medium, and high) and perceived "level of violence" (low, medium, and high). *Membership size* was also estimated during the audit process and included in our models by asking audit participants of the approximate size of the group and then arriving at some general consensus. Finally, and directly related to Figure 3 and one of the core selection mechanisms for VRS treatment, we created variables for the total number of conflicts and alliances of any faction with other factions in the data (Braga et al., 2013). Conflicts were coded when there were identified or known tensions, but especially shootings, among groups; alliances indicate relations marked by consensual criminal ventures or else had a formal alliance.

In addition to gang information from the audits, we calculated the general criminal activity of factions as aggregated from arrest records. For each faction, we aggregated the total number of arrests for aggravated assault, drug-related crimes, robberies, and all other felonies committed by members of each faction. Likewise, we created a *shooting* variable that counts the number of fatal and nonfatal shootings of each faction's members (as either victim or offender) in the 5 years preceding VRS—in essence, the "pretest" level of shooting involvement.<sup>5</sup> Matching on this measure is crucial as our goal is to compare factions otherwise equal to each other on prior levels of shooting involvement to determine whether VRS participation yielded significant posttreatment differences in shooting involvement.

Given the specific use of network analysis to determine treatment factions, we also include several variables about each gang's internal network. For each faction, we created unique gang networks based on the two-degree ego-network for members of each faction. This process began with all of the known members (defined by the police) for each faction and, in essence, snowballed out from these seeds extracting all co-offenders listed in all available police records. We then repeated this to get all the associates' associates. Although by no means a perfect means of determining a gang's true network structure, a growing body of research has found that such co-offending networks provide important insight into the criminal activity of gangs (Grund and Densley, 2014), especially gun violence more broadly (Papachristos et al., 2015). Importantly, these faction co-offending networks represent the co-offending patterns of the faction as opposed to some larger organizational or leadership structure.

To account for the extent that gang network and organizational structure—as well as the variability in said structures—might affect shooting behaviors (Decker, 1996; Decker, Katz, and Webb, 2007), we created several variables pertaining to the organizational or network structure for each gang. In particular, we selected several important structural variables that describe the extent to which each faction is connected and how its patterns of connection might potentially influence the diffusion of the VRS message. These variables are as follows:

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5. For the matched controls, the 5-year window of shooting behavior encompasses the 5 years leading up to the call-in attendance date of the treated faction to which it is matched.

- (1) The *average degree*, or number of, co-offending ties among all known network members. In network analysis, nodal degree can measure (and be interpreted as) many things (Wasserman and Faust, 1994). In our analysis, degree measures each node's number of unique co-offenders. The average degree, then, measures the distribution of unique co-offenders across each faction-network—i.e., on average, how many co-offenders any member of a faction has (whether the co-offender is a member of the faction). A high average degree suggests that members of a faction are tied to a greater number of unique offenders than a faction with a lower average degree.
- (2) The *average shortest path length* (or mean geodesic) among all faction-network members. The geodesic is the shortest distance between any two nodes in a network (Wasserman and Faust, 1994). In networks with more than two people and more than two ties, there are multiple paths between pairs of nodes and the geodesic is the shortest of these paths. Members in faction networks with shorter path distances are “closer” to each other, on average; as such, information—like the VRS message—might diffuse more quickly within factions with low geodesics.
- (3) The *diameter* of a network refers to the *longest* path between any two nodes in a faction network. Broadly, diameter is a definition of network *size*: A larger diameter means that there is a greater distance between the two nodes on that diameter (Wasserman and Faust, 1994).
- (4) The *number of components*. A component is a completely connected subgraph within a network—a graph in which members of one component can all reach each other but cannot reach nodes outside of the component. In Figure 3, for instance, there are four components: the largest component, which includes members A and B, and a smaller component with members D and E. Components F and G are each (technically) their own component. Faction networks with multiple components might be indicative of more splintering within each faction, the presence of smaller operational groups, or greater network size and variability.

The importance of geographic space for gangs—especially gang turf or set-space—is well known (Brantingham, Tita, Short, and Reid 2012; Papachristos, Hureau, and Braga, 2013; Tita, Cohen, and Engberg, 2005). As such, our propensity score models included two geographic variables. First, based on Chicago Police Department maps of gang turf, we calculated the total gang turf controlled by a faction (in square feet). Multiple pieces of turf or larger pieces of turf might be indicative of larger organizational capacity, not to mention potentially more geographic points of conflicts (Brantingham et al., 2012). Second, we include three dummy variables for the general “police area” in the city, with the idea that we want to match treated factions to control factions from the same general section of the city. Broadly, police areas cover wide swaths of geopolitical districts where area 1 represents (roughly) the city's west side (home to gangs like the Vice Lords) and communities near the central business district, area 2 represents the south side (birthplace of gangs like the

Gangster Disciples and the Black P. Stone Nation), and area 3 represents the north side (a more diverse part of gangland).

Finally, our dependent variable is the frequency of shooting involvement of each faction in the 12 months *after* a call-in, where faction involvement is defined by the known gang affiliation of the victim or perpetrator. We calculated separate variables for total shooting involvement, victimization, and offending. Whereas our main interest is in the frequency of shooting involvement, we also conducted a supplementary analysis of the time to the first shooting using a survival model. For this analysis, our dependent variable is the number of weeks from the call-in date until the first shooting involvement of a faction (or the last date of our data collection—March 31, 2014—for those factions that were not involved in a shooting).

### Propensity Score Matching

To summarize, we use propensity score matching to create a quasi-experimental condition to estimate the effect of call-in attendance on the frequency of shootings in which a gang faction was involved during the 12 months immediately after the call-in. One prime source of lack of comparability and equivalence between treatment and control groups—in the case here, between gang factions that had one or more members attend a call-in (i.e., the treatment) and those factions not represented at any call-in—is imbalance. *Imbalance* between the treatment and control groups occurs if there are differences in the pretreatment characteristics of each group. It becomes a problem if there are differences in confounding factors—i.e., characteristics of gang factions that are related to both the likelihood of call-in attendance *and* shooting behaviors. If treatment and control groups are imbalanced, then a comparison of the prevalence of shootings across groups will not yield a valid estimate of the effect of call-in attendance—some other difference between the gang factions *besides call-in attendance* may account for outcome differences.

To resolve any issues of imbalance, we statistically adjust for differences between factions through propensity score matching (Morgan and Harding, 2006; Morgan and Winship, 2007). The propensity score is defined as the probability that a certain faction receives the treatment (i.e., attends a call-in) given all that we observe about the faction. It is a summary measure of the characteristics could confound our ability to estimate the effect of call-in attendance on subsequent shootings. In the present study, we estimate the propensity of call-in attendance for each gang faction in Chicago using a logit model. We use 23 different covariates, which are described and summarized in Table 2, as predictors of call-in attendance. Covariates include prior involvement in violence.

As noted in Table 1, we have missing data on several of our predictors. Accordingly, before creating propensity scores, we used the *mi* commands in Stata (Stata Corp, College Station, TX) to implement the multiple imputation by chained equation algorithm to create five imputed data sets. We then followed Hill's (2004: 13) multiple-imputation matching strategy and calculated a propensity score for each observation in each of the imputed data

**T A B L E 2**

**Covariate Balance Before and After Matching**

Covariates	Raw Means		Differences in Means		Percentage Reduction in Absolute Bias	Postmatch Hypothesis Test	
	VRS	Non-VRS	Unadjusted	Postmatch		t Statistic	p Value
Amount of Turf Controlled (sq. ft.)	3,300,000	2,800,000	500,000	0.00	98.3	-0.01	.989
Arrests							
Aggravated assault arrests	1.34	0.85	0.49	-0.30	37.3	-1.32	.189
Drug arrests	6.30	2.08	4.21	-0.99	76.6	-0.86	.391
Robbery arrests	0.49	0.30	0.19	-0.03	83.7	-0.23	.817
Other felony arrests	5.18	2.22	2.96	-0.36	87.9	-0.44	.659
Network Characteristics							
Average degree	2.13	1.95	0.19	-0.01	95.8	-0.18	.856
Average path length	4.09	2.96	1.13	-0.17	85.4	-0.72	.470
Diameter of network	9.66	6.08	3.59	-0.68	81.0	-1.05	.295
Number of components in network	10.06	5.36	4.70	-1.27	73.0	-1.06	.289
Chicago Police Department Area							
Area 2 (vs. 1)	0.35	0.31	0.04	0.02	55.2	0.28	.777
Area 3 (vs. 1)	0.51	0.23	0.29	0.09	67.9	1.40	.164

(Continued)

T A B L E 2

Continued

Covariates	Raw Means		Differences in Means		Percentage Reduction in Absolute Bias	Postmatch Hypothesis Test	
	VRS	Non-VRS	Unadjusted	Postmatch		f Statistic	p Value
Faction—Level of Organization							
Low (vs. high)	0.15	0.24	-0.09	0.02	79.1	0.39	.696
Medium (vs. high)	0.37	0.38	-0.01	0.00	47.1	0.08	.940
Faction—Level of Violence							
Low (vs. high)	0.21	0.23	-0.02	0.01	56.3	0.18	.858
Medium (vs. high)	0.30	0.31	-0.01	0.09	-878.4	1.61	.108
Faction size	44.85	35.77	9.07	8.71	4.0	1.16	.246
Median age of faction members	21.34	22.23	-0.89	-0.10	89.1	-0.22	.828
Racial Composition of Faction							
Black	0.76	0.47	0.29	0.00	98.5	-0.09	.928
Hispanic	0.13	0.21	-0.08	-0.01	91.1	-0.17	.867
Mixed race	0.05	0.25	-0.19	-0.01	96.5	-0.25	.804
Shootings—2006 to 2010	3.85	2.11	1.74	-0.96	45.2	-1.63	.105
Total number of active alliances	2.39	4.24	-1.85	-0.10	94.7	-0.22	.822
Total number of active conflicts	3.02	5.72	-2.70	-0.16	94.0	-0.30	.767

Note: N = 576. Model specifications: three-to-one matching with a caliper of 0.05.

sets by using the *mi estimate* and *mi predict* commands in Stata. We then averaged the propensity scores for each respondent across the five imputed data sets.<sup>6</sup>

After estimating the propensity score, we matched each treated faction (i.e., attended a call-in) with up to three control factions (i.e., did not attend a call-in) with very similar propensity scores to produce treatment and control groups that are indistinguishable except for the receipt of treatment conditioning on propensity scores.<sup>7</sup> In this case, we used a caliper of 0.05, where caliper refers to a maximum tolerance of distances between propensity scores of the treated and control factions. In our matching procedure, we use matching with replacement—that is, each control faction can be matched to more than one treated faction. Matching with replacement generally increases the quality of matches (i.e., reduces bias), and it increases the variance of the estimate because fewer unique control observations are used to construct counterfactuals (Morgan and Winship, 2007; Smith and Todd, 2005). Matched observations will not necessarily be similar on each covariate, but they will be similar, on average, across all the covariates used to estimate the propensity of call-in attendance.

In total, we matched 148 of the 149 treated factions to at least one control observation. One faction had a propensity of call-in attendance that was not within a .05 probability of any of the nontreated factions, and therefore it was not matched with any control cases. In total we use 428 matched controls; because we matched with replacement and some controls were matched to more than one treated faction, the 428 control matches include 211 unique control factions.

After matching treated and control cases, we determined whether our matching procedure produced balance across the groups on observed covariates. This was done by assessing the percentage reduction in absolute bias and the mean differences across groups for each covariate after adjusting for propensity scores. Bias represents the mean differences across groups as a percentage of the square root of the average of the sample variances:

$$100 \times (\bar{x}_T - \bar{x}_C) / (s_T^2 + s_C^2)^{\frac{1}{2}}$$

- 
6. We also estimated our analyses using listwise deletion for cases with missing values on any of the 23 covariates and include results based on this sample in Figure A1. We used the same matching specification for the analysis in the main body of the article with the imputed data and in the Appendix with the listwise deletion data (i.e., three-to-one matching with replacement and a caliper of 0.05). The results based on the imputed data presented in the main body are more conservative than the results in the Appendix; yet both analyses reveal that call-in attendance yielded at least a marginally significant reduction in the likelihood of all shootings (victimization and offending), and a highly significant reduction in victimizations.
  7. We explored the robustness of our results to the specification of our propensity score model by varying the size of the caliper (0.01 to 0.05, by an increment of 0.005), the number of matches (one versus three), and estimation method (nearest neighbor versus kernel matching with bandwidths of 0.02, 0.06, and 0.10). Our chosen specification of nearest-neighbor matching with a caliper of 0.05 and up to three matches per treated case achieved the lowest level of median bias relative to other model specifications.

where  $\bar{x}_T$  and  $\bar{x}_C$  are the sample means in the treated group and the control group, respectively, and  $s_T^2$  and  $s_C^2$  are the respective sample variances (Rosenbaum and Rubin, 1985).

## Results

Table 2 provides a comparison of treated and control factions across a variety of characteristics before and after matching on propensity score. Focusing on the unadjusted prematch differences, the comparison reveals that members of VRS factions were more frequently arrested for aggravated assaults, drug crimes, robberies, and other types of felonies than non-VRS faction members. Compared with control factions, the VRS factions are also characterized by a greater degree, path length, diameter, and number of components than non-VRS factions. Taken together, this means that the VRS factions were, on average, larger networks with a greater number of components relative to non-VRS factions. This was not necessarily an intention of selection, but it might be the fact that groups with larger networks or greater network diversity (i.e., greater number of components) are more involved in shootings. VRS factions also tend to be located in Chicago Police Department police area 3 (north) with few VRS factions located in the central police area.<sup>8</sup> In contrast, non-VRS factions are much more likely to be located centrally (area 1). In part, this is a function of program design: The program began in high-crime districts in one police area and then expanded slowly from that point. In terms of racial and ethnic composition, VRS factions were much more likely to be factions with predominately Black members and less likely to be predominately Latino factions; again, this is a result of initial program design given that the program began in predominantly Black communities. VRS factions tend to have significantly fewer alliances with other factions but also fewer conflicts.<sup>9</sup> However, VRS factions were involved in significantly more shootings in the 5-year period from 2006 to 2010.

Table 2 thus reveals that treated and control factions differ on numerous characteristics. Ultimately, these differences could account for any observed differences in shootings across factions. Our objective is to ensure that the treated and control factions are statistically similar, on average, across all observable covariates. We do so by matching on the propensity score. After matching, the postmatch *t* statistics and corresponding *p* values in Table 2

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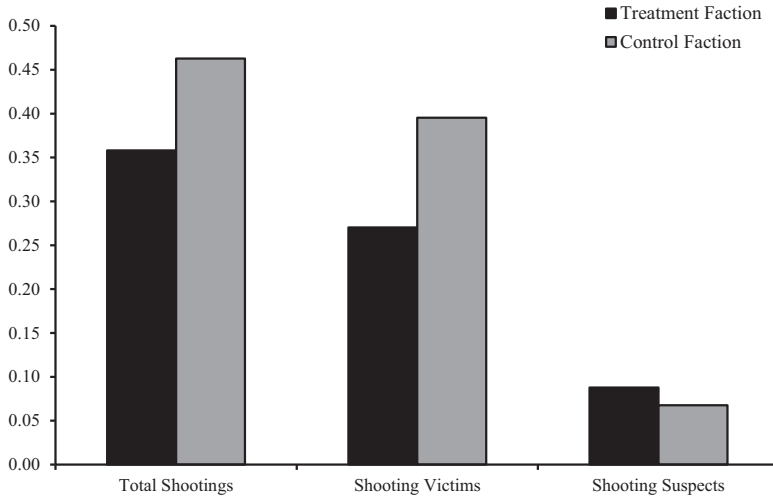
8. For a map of Chicago Police Department police areas, districts, and beats, see the map from the Office of Emergency Management and Communications (2012).

9. This finding seems odd given the audit process's intention of identifying those gangs most involved in violence. Descriptively, this finding stems from two issues: (a) several Latino gangs that are involved in a large number of conflicts but are involved in a small number of shootings—i.e., these gangs have plenty of conflict, but they less often morph into episodes of gun violence—and (b) several large Black gangs on the south side of Chicago that became part of the program in 2014 after our observation period. The current analyses does not include gangs who attended call-in in 2014 as data were not yet available at the time of this writing.



FIGURE 4

**Predicted Number of Fatal and Nonfatal Shootings in the Year After VRS Call-in Attendance, Propensity Matched Gang Factions**



demonstrate that among the 23 covariates used to estimate the propensity score, not one significant difference emerged between the treated and controls in our final matched sample. Matching on propensity score reduced absolute bias across all covariates by 77% from a mean of 36.4 down to 8.4, as well as from a median of 33.1 to 3.8. Equivalence on observed characteristics is critical to our design, as it allows us to compare “apples to apples” when examining the effect of VRS attendance on subsequent shooting involvement.

Having established the effectiveness of our propensity score matching to produce statistically equivalent treatment and control groups, we turn now to the results of the effect of call-in attendance on shootings. Recall that our outcome variable, shootings, is a measure of the number of separate shootings faction members were involved in as either a victim or a known suspect in the 12 months after the date of the call-in. For matched control cases (i.e., that did not attend a call-in), we simply counted the number of shootings for a given faction between the call-in date and the 12 months after. The treated factions and the matched controls were involved in a total of 254 shootings in the 12 months after the call-in date.

The results in Figure 4 display the difference in mean number of shootings between the treatment and control factions 1 year after call-in attendance. The results show that call-in attendance yields a marginally significant reduction in the likelihood of subsequent faction shootings. On average, factions attending a call-in were involved in 0.36 shootings in the year after the call-in, whereas control factions (i.e., those that did not attend a call-in) were

involved in 0.46 shootings. This difference of 0.10 shootings equates to a 23% reduction in shootings after attending a call-in ( $Z = -1.28$ ;  $p$  value = .100, one-tailed test). Put differently, if at least one faction member attends a VRS call-in, then that faction will be involved in 23% fewer shootings in the year after the call-in than if no faction member had attended a call-in.<sup>10</sup>

Of the 254 shootings involving a treated or control faction, in 211, a faction member was the victim, and in 43, a faction member was a known suspect. The relative imbalance in victimization versus offending reflects the fact that the perpetrators of many shootings are unknown.<sup>11</sup> As such, although we may be limited in the extent to which we can conclusively tell whether call-in attendance led to a decline in offending, we can be more confident in victimizations.

Figure 4 also presents analyses disaggregated by victimizations and offending. These results indicate that call-in attendance significantly and substantially reduced the likelihood of shooting victimizations ( $Z = -1.78$ ;  $p$  value = .038, one-tailed test). VRS attendance equates to a 0.13 reduction in the number of shooting victimizations in the year after call-in attendance. In percentage terms, call-in attendance yielded a 32% reduction in the likelihood of nonfatal and fatal victimization in the year after the call-in date. VRS had no observable effect on known offending ( $Z = 0.60$ ;  $p$  value = .274, one-tailed test). Again, many perpetrators of gun violence are unknown to the police. Moreover, given heightened scrutiny of those gang factions participating in VRS, we might even expect that for treatment and control factions committing the same number of shootings (as an offender), that the treatment (i.e., VRS) faction would be more likely to be arrested for the involvement. Hence, the fact that there is no statistical difference in the perpetration of gun violence (i.e., offending) between the VRS and non-VRS factions suggests that, at a minimum, VRS factions are no more likely to be perpetrators of shootings and may in fact be less likely.

## Conclusion and Discussion

Four years after the first VRS call-in raised concern in the media and drew the ire of some politicians, ex-gang leaders, and community activists, our study finds evidence of a promising gun violence reduction effect among those gang factions who participated in the program. Our quasi-experimental analyses that matched treatment and control

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10. As a supplementary analysis, we also analyzed the time to failure (i.e., time until a faction-involved shooting) using propensity score weights in a Cox proportional hazards model. The results from this survival analysis reveal a significant, negative relationship between VRS attendance and the hazard of shooting involvement (coefficient =  $-0.393$ ;  $Z = -2.04$ ; hazard ratio = 0.675). Consistent with our other results, those factions that participated in VRS were significantly less likely to be involved in a shooting than otherwise similar control factions and go a longer period of time until a shooting incident. Survival curves are shown in Figure A2.

11. For 2010, the Chicago Police Department reported a clearance rate of 33.9% for murders and 33.3% for all violent index crimes (Chicago Police Department, 2011). The percentages were comparable for 2009.

factions using propensity score matching techniques found a 23% reduction in total shooting behavior in treatment factions and a 32% reduction in gunshot victimization among members of treatment factions. No statistically significant effect was observed on offending patterns. Overall, our results provide evidence that the call-in style intervention of VRS that focuses its efforts on specific gang factions provides a promising strategy for targeted gun violence reduction strategies.

These findings build on prior research in at least three important ways. First, most prior research on call-in style programs has focused on *aggregate* neighborhood or city-level crime rates (for an exception, see Braga et al., 2013). In contrast, our study analyzed the actual unit of intervention—gang factions—and, more importantly, created a set of matched comparison groups. Second, our study is one of the first to differentiate between gun victimization and gun offending among the treated population (again, with the exception of Braga et al., 2013). Although our findings on offending are stymied by missing data on unknown offenders, the current results suggest that important differences may exist between faction-level victimization and offending patterns—something future research should consider. Finally, studying such programs in Chicago, one of the country's gang epicenters, represents one of the first attempts of applying and evaluating such a program in a city of such size, with a large population of gangs, and with a long and embedded history of gangs and gang violence.

Our study is not without limitations. First and foremost, despite our best efforts, it is possible that our propensity score modeling fails to capture unobserved nonrandom selection processes, especially the political processes for selecting the initial program areas and subsequent program expansion. Although Table 2 suggests that our models do an adequate job in eliminating faction-level differences between treatment and control groups, unobserved differences might influence our findings. Second, the lack of complete data on offending patterns might suggest that we are underestimating the overall shooting behavior of factions, although this would be true of both treatment and control factions.

A third limitation is the concurrency of VRS with other gun violence reduction strategies in Chicago. In particular, two other high-profile gun violence prevention programs—PSN and CureViolence—were in operation during our study period. Some overlap did exist between PSN and VRS treatment areas; however, PSN and VRS staff worked together to minimize the cross-contamination between the individuals involved in the respective programs.

CureViolence has operated in Chicago since 1999, and during this time, it has worked in more than a dozen high-crime communities (see Skogan, Hartnett, Bump, and Dubois, 2009). VRS and CureViolence share a common theoretical guiding principle in directing resources toward those gangs actively involved in gun violence. VRS does so through call-ins, whereas CureViolence uses outreach workers called “violence interrupters” (Skogan et al., 2009). The exact procedure through which CureViolence directs its violence interrupters is unknown; therefore, it is not possible to know whether individuals

were part of both CureViolence and VRS. In terms of geographic treatment area, CureViolence's programmatic area ebbed and flowed during our study period, making it difficult to ascertain programmatic cross-contamination. However, a recent evaluation of CureViolence suggests that VRS and CureViolence were not operating in the same areas during our VRS study period (Henry, Knoblauch, and Sigurvinsdottir, 2014). Thus, although programmatic overlap is still a possibility, we believe the effects would be minimal.

Limitations notwithstanding, our study provides consistent evidence that getting the right message to the right groups in a way that is timely, just, and fair can successfully reduce gun violence among the targeted factions. Programs such as VRS are by no means a cure-all for gun violence: They do not, for instance, improve schools, create jobs, reduce inequalities, or address other macrolevel community factors at the heart of gun violence. Yet, VRS-style programs just might provide a way to intervene in the street dynamics that drive gun violence in American cities. Furthermore, in stark contrast to policies and policing efforts such as "stop and frisk" or gang loitering laws that cast their nets broadly, VRS-style interventions achieve a dramatic crime reduction effect while subjecting smaller numbers of people and groups to criminal justice intervention. Taken together, the design of the program and its demonstrated efficacy might lend itself to similar focused efforts in the realm of educational, social work, violence interruption, and public health interventions.

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**Appendix**

The results in Figure A1 are based on the listwise deletion of cases with missing values. In this case, we delete a total of 20 treated gang factions of 149 total from the analysis. The results indicate that call-in attendance significantly and substantially reduced the likelihood of total shootings ( $Z = -1.87$ ;  $p$  value = .031, one-tailed test) and victimizations specifically ( $Z = -2.61$ ;  $p$  value = .005, one-tailed test). Call-in attendance had no apparent effect on known offending ( $Z = 0.45$ ;  $p$  value = .326, one-tailed test). VRS attendance yielded a 0.16 reduction in the total number of shootings and a 0.18 reduction in victimizations in the year after call-in attendance. In percentage terms, these numbers equate to a 31% reduction in the likelihood of nonfatal and fatal shootings (victimizations or offending) and a 40% reduction in victimizations specifically.

**FIGURE A 1**

**Predicted Number of Fatal and Nonfatal Shootings in the Year After VRS Call-in Attendance, Propensity Matched Gang Factions (Nonimputed Data)**

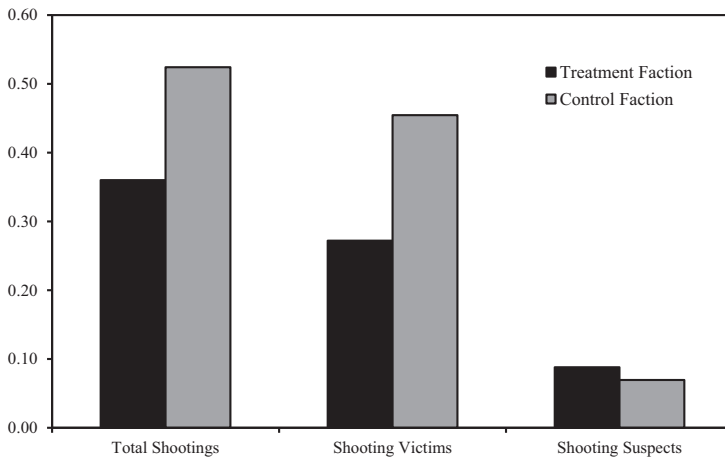




TABLE A 1

## Top 30 Violent Crime Rates Across Major Metropolitan Areas (2012)

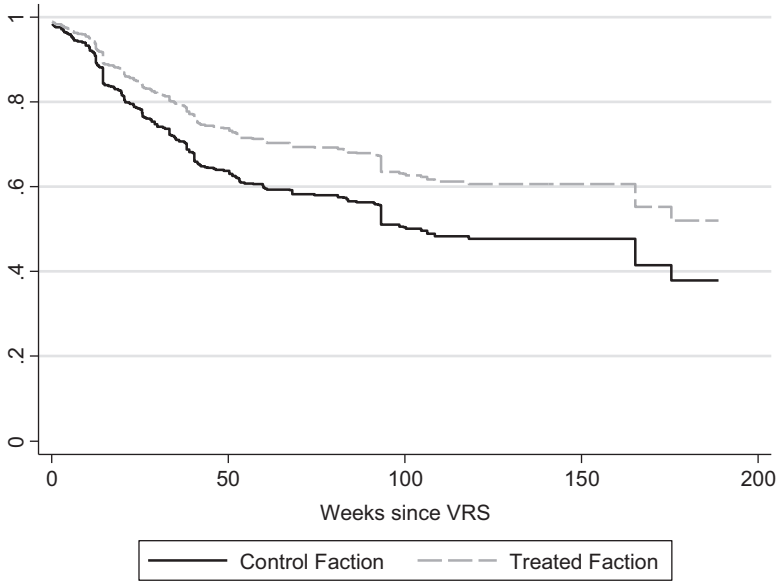
Ranking	Agency	State	2012
1	Detroit Police Department	MI	2,122.9
2	Oakland Police Department	CA	1,993.3
3	St. Louis Police Department	MO	1,776.5
4	Memphis Police Department	TN	1,750.3
5	Stockton Police Department	CA	1,548.0
6	Baltimore City Police Department	MD	1,405.2
7	Cleveland Police Department	OH	1,383.8
8	Atlanta Police Department	GA	1,379.0
9	Milwaukee Police Department	WI	1,294.5
10	Buffalo Police Department	NY	1,288.7
11	Kansas City Police Department	MO	1,263.2
12	Nashville-Davidson Metro Police Department	TN	1,216.0
13	Indianapolis Police Department	IN	1,185.5
14	Washington Metropolitan Police Department	DC	1,177.9
15	Miami Police Department	FL	1,172.0
16	Toledo Police Department	OH	1,171.9
17	Philadelphia Police Department	PA	1,160.1
18	Newark Police Department	NJ	1,154.5
19	<b>Chicago Police Department</b>	<b>IL</b>	<b>1,045.2</b>
20	Houston Police Department	TX	992.5
21	Minneapolis Police Department	MN	992.2
22	Tulsa Police Department	OK	990.0
23	Cincinnati Police Department	OH	974.7
24	Oklahoma City Police Department	OK	919.1
25	Boston Police Department	MA	835.0
26	Anchorage Police Department	AK	828.7
27	New Orleans Police Department	LA	815.2
28	Las Vegas Metropolitan Police Department	NV	784.0
29	Pittsburgh Police Department	PA	752.0
30	Albuquerque Police Department	NM	749.7

Note. These data were taken from the FBI Unified Crime Reporting Statistics ([ucrdataool.gov/index.cfm](http://ucrdatatool.gov/index.cfm)) data portal, listing the crime rate for Index Part 1 violent crimes per 100,000 residents for law enforcement agencies serving 250,000 people or more. As the Chicago Police Department does not report forcible rape according to UCR guidelines, we impute the violent crime rate for 2012 from our data at hand.

**Andrew V. Papachristos** is an associate professor of sociology at Yale University. His research applies the growing field of network science to understanding patterns of gunshot victimization in U.S. cities. Much of this work has focused on street gangs, illegal gun markets, and interpersonal violence. His recent writing has appeared in *The American Journal*

FIGURE A 2

**Estimated Survival Time Until a Fatal or Nonfatal Shooting, Cox Proportional Hazards Regression with Weighting by Propensity Score**



of *Sociology*, *The American Sociological Review*, *The American Journal of Public Health*, *Social Science & Medicine*, and *The Journal of Quantitative Criminology*, among other outlets.

**David S. Kirk** is an associate professor of sociology and a professorial fellow of Nuffield College at the University of Oxford. His current research interests include neighborhood effects, prisoner reentry, and crime and the life course. One ongoing project involves an experimental housing mobility program for ex-prisoners. Kirk's recent research has appeared in *American Journal of Sociology*, *Sociology of Education*, and *Criminology*.

# POLICY ESSAY

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## CHICAGO'S GROUP VIOLENCE REDUCTION STRATEGY

# With Great Methods Come Great Responsibilities

## Social Network Analysis in the Implementation and Evaluation of Gang Programs

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“I’ve had it with gangs . . . There comes a time when the new seems familiar and the familiar, soporific” (Klein, 1971: 1)

Our initial reaction to Papachristos and Kirk’s (2015, this issue) article eerily resembled the above quote regarding Malcolm Klein’s exasperation with gang research. “Here we go again, yet another replication of ‘pulling-levers’ policing. What more can we learn?” By now we know that these initiatives are generally effective at reducing gun violence (see Braga and Weisburd, 2012). We have been told that the reason these programs work is that they are “getting deterrence right” (Braga, 2012; Kennedy, 1998). The simplicity and intuitiveness of the approach makes it all the more attractive: Information about the consequences of continuing to commit acts of violence is delivered to individuals, most frequently gang members, who are the most likely to engage in and be victims of firearm violence. Meetings are held where gang members are told that they are vulnerable to a wide variety of sanctions including any pending felony investigations, breach of conditions of parole or probation, and outstanding warrants. Those who attend the meeting carry back to their gangs a message that law enforcement is serious about taking advantage of these vulnerabilities against the entire gang if the violence does not

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stop. The hope is to co-opt the group's social cohesion and create incentives to hold members accountable to their fellow gang members for their actions.

It is implied, but hardly ever made truly explicit, that the effectiveness of the program depends on the presence of two key elements. First, the social contract among gang members must be strong enough that each individual cares about the vulnerability of his or her fellow gang members and elicits behavioral changes for the good of the group. That is, the decision of an individual gang member as to whether to commit a violent act has to be a function not only of the consequences that could befall the actor but also of the consequences for other members of the gang. Second, the structure of the networks that link individuals must be sufficient to propagate the diffusion of the "don't shoot" message.

The theory behind focused-deterrence programs has generally been accepted at face value and for good reasons. Gang members, like members of all groups, are embedded in networks of social relationships, and although the strength or "cohesiveness" of the typical urban street gang varies from one setting to another, any structure can facilitate to some degree the diffusion of information. However, there is a rich literature on the effect of different structural features of social networks on the diffusion of information, ideas, and behaviors (e.g., Albert and Barabási, 2002; Burt, 1992; Granovetter, 1973; Watts, 2002) that has generally been left untapped. Such research should help identify areas of inquiry that may allow researchers to engage the theory behind these programs. Those evaluating gang control strategies, including most evaluations of pulling-levers strategies, have generally failed to provide the logic model of their programs and properly test the hypothesized theory of change (Gravel, Bouchard, Descormiers, Wong, and Morselli, 2013).

To test the theory behind these interventions, one would imagine that we are most interested in capturing changes in individual behavior and in examining how the deterrent message spreads throughout social networks. However, the methods used for evaluating these programs have usually been limited to examinations of trends in aggregate-level crime statistics (see Braga and Weisburd, 2012, for a review; see Rosenfeld, Fornango, and Baumer, 2005, for a critique of past evaluations). Although the use of aggregate data might provide a good measure of the impact on overall levels of gun violence, analyses that rely on aggregate level data generate little insight about *how* a program works and which facets of a program are responsible for the change (Papachristos, 2011). We know most focused-deterrence strategies are effective, but what we know about *why* they work is limited to theoretical ideas (Kennedy, 1997) and simulations (e.g., Kleiman and Kilmer, 2009). Yet researchers seem to have been generally satisfied with hypotheses, citing program effectiveness as evidence supporting the theory. Like Klein, we have "had it" with pulling-levers evaluations that seek to answer the *does it work* question without empirically demonstrating *how it works*. But also much like it was the case for the venerable gang researcher, novel approaches to the problem are likely to renew our interest in the topic of focused deterrence.

Then comes Papachristos and Kirk's (2015) research. Their article provides one of the most thorough descriptions of the logic behind a central component of "pulling-levers"

programs: the deterrent message delivery system or the “call-ins.” Consistent with this logic, Papachristos and Kirk employ social network analysis (SNA) to target and evaluate the effectiveness of the approach. There has been no shortage of programs that have used SNA explicitly to target their intervention (e.g., Braga, Kennedy, Waring, and Piehl, 2001; Engel, Tillyer, and Corsaro, 2013; Kennedy and Braga, 1998), and a few have also used it in the evaluation (Braga, Hureau, and Papachristos, 2014; Tita, Riley, Ridgeway, et al., 2003). But we believe that Papachristos and Kirk’s use of SNA is innovative and holds great promise to move toward a better understanding of how programs like Chicago’s Group Violence Reduction Strategy lead to a reduction in gang violence.

In this essay, we first describe the program and its evaluation described by Papachristos and Kirk (2015). Second, we draw from the social network literature to highlight the potential of SNA for research on focused-deterrence strategies. Finally, we provide concrete recommendations to guide policy makers and researchers for the design, implementation, and evaluation of future strategies.

### **Implementation and Evaluation of Chicago’s Group Violence Reduction Strategy**

Papachristos and Kirk (2015) describe the implementation and program evaluation of Chicago’s Group Violence Reduction Strategy (VRS). The strategy behind VRS is based on the widely replicated Boston Ceasefire program (see Braga et al., 2001), a focused-deterrence strategy that aims at delivering a “don’t shoot” message to those most likely to engage in gun violence. VRS was designed to reduce gun and gang violence by reaching out to gang members from gangs involved in gun violence through meetings—or “call-ins”—attended by program staff, community members, social program agencies, and law enforcement. The objectives of these “call-ins” were to demonstrate to gang members that their role in local violence is not a mystery to the police or other participants and to “tell them, in no uncertain terms, to put down the guns” (Papachristos and Kirk, 2015). The target audience was informed that failure to cooperate would lead law enforcement to “pull all levers” available to them (i.e., executing warrants, revocation of parole or probation, arrests, etc.), not only against individuals directly involved in violence but also against the gang as a whole.

The VRS has much in common with prior iterations of a data-driven analysis that leads to a pulling-levers strategy. Working group meetings were held on an ongoing basis with a variety of stakeholders to gain an understanding of the factors driving violence. Official police data were analyzed, and a social network analysis was used to identify the relationships between gangs involved in the violence. By using propensity score matching, Papachristos and Kirk (2015) compared the shooting behavior (i.e., shootings and victimization) of treatment and control groups in the year after the “call-in” sessions. Consistent with prior evaluations of similar programs, Papachristos and Kirk found that groups who attended the “call-ins” experienced a 23% reduction in shooting behavior (not statistically significant  $p = .10$ ) and a 32% reduction in victimization ( $p = .04$ ).

What sets Papachristos and Kirk's (2015) research apart from a prior evaluation of similar programs, however, are not the findings regarding the program's effectiveness. Rather, the evaluation of VRS departs from its predecessors in considering the internal structure of the gang as an important feature in determining the success of the intervention. Even though propensity score matching has been used to create geographically defined matched/treatment areas (e.g., Tita, Riley, and Greenwood, 2003), Papachristos and Kirk use the social ties among gang members (in this case co-arrest data) to create matched samples of gangs. This contribution is important because structural features are likely to be critical to the effectiveness of the intervention. Therefore, a better understanding of how these structures influence the program's effectiveness can go a long way toward unpacking the program's theory of change.

Papachristos and Kirk's (2015) study is an important step forward in the use of SNA in the evaluation of these types of programs (and the relevance of SNA to criminology more broadly.) However, we believe that the study falls short in terms of positing a more complete theoretical explanation of why internal gang structures matter. Furthermore, as we will discuss in this policy essay, the use of social network concepts relevant for gang violence prevention are somewhat contradictory. Social network analysis is especially well suited to study diffusion of information and adoption of behaviors (e.g., Centola, 2010; Rogers, 1976; Valente, 1996; Watts, 2002), which are theorized as key mechanisms behind these programs, and yet Papachristos and Kirk do not fully explain why considering the internal structure of the gang is relevant to the effectiveness of the program.

In the following sections, we attempt to build on the insight provided by Papachristos and Kirk's (2015) analysis and examine ways in which social network analysis can improve both the delivery and the evaluation of focused-deterrence programs.

### **Why Should Focused Deterrence Work, and How Can We Make Sure it Does?**

The premise of focused-deterrence strategies is that they produce an incentive for gang members not only to change their own behavior but also to regulate and induce behavioral changes in others (Kennedy, 1997). If the goal is to ensure that the "don't shoot" message is propagated through networks, then SNA can help define the most efficient ways to propagate the message. Network analysis was in fact used as part of the Boston Gun Project to identify how best to identify ways to diffuse the deterrence message (Kennedy, Braga, and Piehl, 1997). Kennedy et al. (1997) explained that SNA was used to map the network of conflict and alliances between gangs in Boston in order to select which gangs to target and identify clusters of gangs within the network. They argued that identifying such clusters is useful to ensure that the message is efficiently diffused throughout the network: "[T]alking' to one member [in a cluster] would effectively be talking to all members" (Kennedy et al., 1997: 240).

Other than the original implementation of Operation Ceasefire, SNA has been used to identify the most relevant gangs for intervention in other sites as well. For example, Tita,

Riley, and Greenwood (2003) mapped the rivalry network of gangs in the Hollenbeck area of Los Angeles to identify gangs most involved in violence. Although other studies also have mentioned the use of SNA or the relevance of networks generally (e.g., Braga, McDevitt, and Pierce, 2006; McGarrell and Chermak, 2003), how these networks were constructed or how the analysis was carried out has rarely been described in as much depth as it is in the Operation Ceasefire evaluations or in the current study of VRS by Papachristos and Kirk (2015). Moreover, SNA has been used to inform the evaluation design (e.g., Braga, Apel, and Welsh, 2013) or to study the spillover effect of the program (e.g., Braga et al., 2014; Tita, Riley, and Greenwood, 2003). To our knowledge, Papachristos and Kirk's analysis is the first study to consider the *internal structure* of gangs as part of their modeling strategy to assess the program's effectiveness.

This innovation is important. To borrow from Morselli (2009), when it comes to gangs, we should not assume but rather seek structure. Programs targeting gangs (rather than individuals) operate under the assumption that these groups can efficiently diffuse the deterrent message. In fact, the hope is to use the gang's social cohesion to produce informal social control to reduce violence. However, it is a foregone conclusion in gang research that highly cohesive, organized, and structured gangs are the exception, not the rule, and such features vary greatly from one gang to another (e.g., Bouchard and Spindler, 2010; Decker, 1996; Decker and van Winkle, 1996; Decker, Katz, and Webb, 2008; Klein and Maxson, 2006).

Because much of the strategy depends on certain assumptions regarding gang structure and cohesiveness, a failure to understand the social structure of gangs can derail intervention efforts. Past research has shown that police-collected data on gangs is unreliable (e.g., Katz, 2003; Spergel, 1995) and that police have a tendency to overemphasize gang organization (e.g., Klein and Maxson, 2006). Moreover, as Felson (2006) argued, law enforcement officials have a tendency to group many small gangs into larger groups and focus their attention on these few large groups. Large monolithic gangs, such as the Bloods and the Crips, are often a very loose association of different smaller gangs with little relationships between them. As Papachristos (2005) pointed out, SNA can help identify relevant groups within larger conglomerates by examining structures emerging from the data.

An example of such a use of SNA was provided by McGloin (2005), who analyzed the internal networks of gangs in Newark, New Jersey, and found little evidence that these groups were "socially unified" (p. 624). Indeed, the gangs she studied were generally formed of multiple small, unconnected cliques. Given the analysis undertaken in the VRS evaluation by Papachristos and Kirk (2015), this also seems to be the case for gang factions in Chicago (more on this next). The implication of McGloin's analysis is that an uninformed group-level targeting strategy is inefficient for programs that attempt to use "the social cohesion of the gang as strategic leverage" (p. 624). At the gang level, she argued, "there is simply not enough social cohesion to sustain this strategy" (p. 624). McGloin (2005) also wondered whether perhaps using smaller groups—or sets—much like what was done for the VRS would be a more efficient strategy. McGloin (2005) found that cohesive subgroups that emerged from

individual-level network analysis did not reflect smaller sets of gangs identified by police. Instead, connections between individuals often bridged different sets. This analysis reflects a point made by Papachristos (2005). He argued that SNA holds promises, most notably, to address a problem that has been a thorn in the side of both academics and law enforcement: the notoriously difficult task of gang definition.

Recently, researchers using SNA have stressed the limitations posed by treating such “gangs” as Bloods, Crips, Gangster Disciples, and Latin Kings as meaningful units of analysis, especially when labeling violence as “intragang and intergang” violence (Decker and Curry, 2002; Descormiers and Morselli, 2011; Papachristos, 2009). One of the more surprising findings (and we believe unintended consequence) of Papachristos’s (2009) seminal work *Murder by Structure* is the notion that a lot of gang violence occurs to members of the *same gang*. In Los Angeles, the Crips and Bloods are enemies. It is also true that more Crips have been killed by members of other Crip sets than by all other Blood sets combined. No one studying gangs would lump the members of the various Crip sets in South Los Angeles into the same gang or suggest a homicide committed by a member of the Eight-Trey Gangsters against a member of the “Rolling 60s” and “intragang” homicide. They may both claim “Crip,” but they are far from members of the same “gang.” Thus, targeting factions rather than gangs, as Papachristos and Kirk (2015) did, is an important improvement from analyzing the larger gang “nations” of Chicago. However, until we can better understand the structure of the factions, it is still not clear that simply targeting these smaller elements will promote the efficient diffusion of information among members. Research on social networks and Papachristos and Kirk’s (2015) analysis suggest that using SNA to identify key individuals or cliques within larger groups could enhance the efficiency of the approach, as well as perhaps even lead to larger effects of the program.

From Papachristos and Kirk’s (2015) study, it is unclear whether information about the internal structure of factions was used to identify the most efficient points of intervention. The focus on factions rather than on gangs in the VRS suggests that Papachristos and Kirk are aware of the internal structure of large gangs in Chicago. However, their results show that the factions targeted by the program are not necessarily “socially unified,” to borrow McGloin’s (2005) words. To understand more fully why social networks matter so much to the theoretical reasoning of focused deterrence, we take a moment to unpack some of the measures used by Papachristos and Kirk. We provide a basic introduction to the social network literature to support what we think should be the “next steps” in terms of using SNA in the design, implementation, and evaluation of focused-deterrence programs.

### **Strengths of Strong and Weak Ties**

Research on social networks has provided a theoretical basis for the logic of focused deterrence for two reasons. First, the most efficient ways to diffuse information in a network have been studied extensively. Second, network theory can help identify structural features that may lead to information to elicit behavioral changes.



*Diffusion of Information and the Strength of Weak Ties*

Research has found that most social networks are “scale free,” which means that a few individuals have a high number of ties and most individuals have few (Barabási and Albert, 1999). Moreover, social networks often exhibit “small-world” characteristics—local clustering combined with short average social distance (Watts, 1999; Watts and Strogatz, 1998). The speed and extensiveness of diffusion is directly related to the structure of the regions of networks where information is introduced; given the topology of networks, information is much more likely to spread through the network when it enters through the most connected individuals (i.e., “central” individuals) and those who bridge local clusters (e.g., Albert and Barabási, 2002; Centola, 2010; Watts, 2002). In other words, when social networks are concerned, a “focused” approach will generally outperform a random approach when the objective is to “infect” the network with an idea.

Social networks are generally organized in such a way that key individuals, because of their structural position, control the flow of information or simply have more influence on others. The combination of local clustering and short average distance typical of small-world networks is possible because of the presence of “weak ties” (Granovetter, 1973). Weak ties bridge different social groups, and therefore, individuals who are positioned between social groups act as gatekeepers for novel information (Granovetter, 1973). Networks that are not fully connected—that is, where not every individual knows every other individual—have “structural holes” (Burt, 1992). For a message or an innovation to reach different parts of a network, individuals positioned in these structural holes must not only relegate the message to others but also convince others that the message is important enough that it should be dispelled to others in the more densely connected substructures (Burt, 2004; Lin, 2001). Therefore the “strength of weak ties” is that they are crucial to the generalized diffusion of information in all different substructures of a network (Granovetter, 1973).

The absence of weak ties will result in a network that is more likely to be fractured into multiple components. Components are smaller disconnected portions of the larger structure in which each individual can directly or indirectly reach every other individual in the component. Theoretically, information cannot flow directly from one component to any other. The analysis of the internal structure of factions in the VRS evaluation revealed that on average, the factions targeted by the program were composed of 10 components (see Table 2 in Papachristos and Kirk, 2015). This implies that for the deterrent message to flow throughout the entire faction, at least one member of each faction should be targeted. Comparatively, factions not targeted by the program seem to be more connected: These factions have on average five components. Therefore, whereas at least 10 gang members would need to attend the call-ins, a similar effort directed at the nontargeted gangs would require only five attendees. From these structural differences inherent between the two groups, we surmise that these groups are clearly “different.” This underscores the importance of controlling for these characteristics as part of the propensity score matching techniques.

The structure of the component also matters in terms of the flow of information (Burt, 1992). Individuals that link different parts of the component together (what Burt labeled “brokers”) are often in a position of power because they have access to information from different parts of the network (Lin, 2001). These “brokers” often play an important role in the coordination of activities among the otherwise disconnected components of a group (Morselli, 2009). They are in a position to influence the opinions of others in the network. For example, Nash, Bouchard, and Malm (2013) found that brokers were crucial in the network diffusion of fraud as they were able to recruit friends and family to invest in the Ponzi scheme. Similarly, brokers in a gang network may occupy important positions between different subgroups such as age-graded groups or between different “sets.” More importantly, brokers are crucial for the cohesion of groups; brokers may reassure members that all members are willing to change their behaviors for the good of the group. Without the broker binding the larger group together, it is difficult to enforce behavioral changes at the group level.

### *Adoption of Behaviors and the Strength of Strong Ties*

The efficiency of diffusing information is of little consequence if the message is not taken seriously and does not lead to behavioral change. This is where “the strength of strong ties” is important (Krackhardt, 1992). The social contract that emerges between strongly connected individuals has great implications for focus-deterrence strategies. Network components often include substructures of closely connected individuals called cliques. In SNA, a clique is a part of a component that exhibits maximal connectivity—all members of a clique are directly connected to all other members of the clique (Wasserman and Faust, 1994).

Highly interconnected subgroups in networks foster trust between members: If everybody in the network knows everybody else, then they are much more likely to become aware of others’ behaviors or intentions (Burt, 2005). This notion is what Burt (2005) called bandwidth: Dense networks ensure the rapid spread of information and the salience of this information for individuals whose ties have a redundant access to the information. The information gets repeated multiple times in the network because a single piece of information can reach every individual through as many conduits as there are other individuals in the clique. Bandwidth therefore implies greater social control in closed networks. As Burt (2005) explained:

[T]wo people affiliated with the same contacts have structurally equivalent identities. . . . They are, to a degree, the same kind of person. Undercutting or otherwise failing to cooperate with your own kind is a betrayal viewed with suspicion by observers debating trust. (p. 107)

Because of the trust that emerges in dense networks, these ties are often called “strong ties.” (Granovetter, 1973)

Cliques are more likely to ensure the rapid and redundant repetition of the deterrence message, but even more importantly, if it is taken seriously by the carriers of the message to that clique, then the message is much more likely to yield behavioral changes in highly connected or cohesive subgroups. The factions targeted by VRS do not seem to be particularly cohesive. On average, targeted factions had a geodesic path length of 4 and a diameter of almost 10. Theoretically, an average geodesic path of 4 implies that if you select a gang member at random and give him a message, then the message will reach the furthest person in the network through three other individuals before reaching the furthest parts of the network. These measures are not necessarily indicative of the extent to which cliques are present, but they highlight the sparse nature of the networks. It should be noted that the use of co-offending data is likely to create sparse networks and is likely to be only a superficial representation of a faction's connectivity. The existence of cliques, even in these sparse networks, and the diffusion of the deterrent message within them may increase the likelihood of behavioral changes.

We have highlighted in the preceding discussion that the factions targeted by VRS were perhaps less efficient for diffusion and behavioral change compared with nontargeted factions. Why is that important given that propensity scores control for the differences? It matters because, whatever effect is found after matching, it is inconsistent with the program's theory of change. If the VRA factions' networks are theoretically inefficient at propagating the effect, then why do we find an impact of the program? The focused-deterrence logic is that if you reach out to the *right* people (as opposed to a wide-net approach), then deterrence will work and lead to more dramatic effects. It is certainly promising that the VRS group showed a decrease in victimization, but the inefficiency of the networks to propagate the message in VRS raises a doubt that the propagation of the message is really the mechanism that drives the observed reductions in gang violence.

In this essay, we have discussed how research on social networks might support the program's theory of change. The evaluation of VRS puts SNA to good use by informing the matching technique. However, we argue that data on the internal structure of factions could have been used to move beyond answering the "does it work?" questions and move to the more challenging "how does it work?" questions. We believe that this is the next step in understanding how focused deterrence works.

Nevertheless, the fact that significant changes are observed despite the inefficient targeting of factions is encouraging. If the theory behind the program is supported, then better targeting of subgroups and key individuals should lead to larger programmatic effects.

## Conclusion

When research is carried out with the intention to find an effect (if any) rather than explaining the effect (or lack thereof), replications and program expansions are little more than shots in the dark. Evidence of effectiveness is not evidence that a program works how

we think it works; a program's theory of change remains a theory if the mechanisms that lead to a program's effect are not tested.

In this essay, we have discussed the relevance of the use of SNA in the implementation and evaluation of focused-deterrence programs and other similar gang control strategies. This discussion has allowed us to make several recommendations for policy makers and researchers designing and evaluating these programs.

First, we believe that local law enforcement and research partners should use SNA to understand the structural features of gangs more fully. Prior studies have shown that combining SNA techniques with police knowledge can be much more efficient than relying on police intelligence alone. Examining complex issues such as the structure of gangs requires the kind of "big picture" approach SNA is designed to tackle. Unverified assumptions relating to the organizational structure of gangs can hinder efforts to identify key players to target. For example, Morselli (2009) demonstrated through SNA that contrary to conventional wisdom, nongang members played an important role in the control of a local drug distribution network. Bouchard and Konarski (2013) also showed that SNA proved to be a better technique than simply relying on police judgment in identifying "core" gang members.

Second, future research should take into account the interconnected nature of street gangs when designing their evaluations. Evaluations using quasi-experiments can be informative regarding the causal effect of a program so long as the design does not violate the Stable-Unit-Treatment-Value-Assumption or SUTVA (Rubin, 1990). Simply stated, SUTVA is not violated when (a) there is no interference between the treatment units and (b) treatment given to each treatment unit is identical. The unique design of pulling-levers strategies makes evaluations of these programs especially vulnerable to violation of SUTVA.

The first condition can be easily addressed by a technique used by Braga et al. (2014) in their evaluation of the more recent iteration of Boston's Ceasefire program. In this study, the authors used SNA to remove from the comparison group any gangs that are socially connected to the treated groups. Papachristos and Kirk (2015) compare gang factions in areas where VRS was active to factions active in areas where VRS was not implemented. The rationale behind this decision was that, "Because the program focused on specific gang factions and began in a limited number of police districts, it is not intended to decrease shooting behavior among *all* gang factions in the city—only those targeted by the intervention" (Papachristos and Kirk, 2015). It is logically inconsistent to argue that a program's theory of change is based on the diffusion of information in the networks and not to consider that gangs, even if geographically distant, may be socially connected and therefore be indirectly exposed to the treatment. In fact, Braga et al. (2013), in reanalyzing results from the Ceasefire evaluations (see Braga et al., 2014), showed that groups socially connected to the treated groups also showed a decrease in violence.

Papachristos and Kirk (2015) readily address the second condition. The variability in the internal structure factions suggests that not all factions received equal treatment. Highly

fractured factions or factions with low density are unlikely to propagate the deterrent message as efficiently as highly cohesive groups. Future studies should follow Papachristos and Kirk's lead and include measures about the internal structure of groups targeted in order to control for this variability. In fact, future studies should go beyond and explore how a different network topology is associated with behavioral changes. In a way, network measures can serve as a proxy for treatment dosage because less connected networks are likely to be associated with less exposure to the deterrent message as well as with a reduced likelihood of the development of a social control mechanism between gang members. Moving research in this direction will enable us to address *why* focused deterrence seems to work and guide future policy on how to make these programs more effective.

Papachristos and Kirk's (2015) use of what we believe to be the "right" techniques to implement and evaluate focused-deterrence strategies brings to light many issues that might have gone unnoticed had Papachristos and Kirk chosen to use a more traditional methodology. We believe that many of our observations apply to almost every prior study on focused deterrence. However, Papachristos and Kirk's (2015) study is easier than most prior studies to criticize, not because it is methodologically weaker but precisely because it is methodologically stronger. Our critical discussion was fuelled by data they provide on the internal structure of factions. Because such data are absent from prior evaluations of focused-deterrence programs, Papachristos and Kirk (2015) receive the brunt of our criticism for something that is likely to be a common failing of past research on the topic. Introducing a novel methodology is a difficult but important task because it highlights problems—old and new—and opens the door for more productive discussions. But remember: With great methods come great responsibilities.

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# POLICY ESSAY

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## CHICAGO'S GROUP VIOLENCE REDUCTION STRATEGY

# Considering Focused Deterrence in the Age of Ferguson, Baltimore, North Charleston, and Beyond

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**W***hy do they run?* Such is the mantra of news anchors and media pundits alike, who recognize the injustice of the consequences for victims of police violence while insinuating the suspiciousness of the victim. Critical questions about (a) the appropriate and just model for policing and (b) acknowledgment of the intense frictions that comprise the police–community relationship in many urban neighborhoods have been driven to the fore in recent months as institutional violence has sparked substantial unrest, nationwide protests, and the seeds of the major social movement “#blacklivesmatter.” This movement arose after well-publicized incidents of police action resulted in the fatalities of several African American men over a short period of time. In this historical moment, the question of “what works” may be rightly viewed as less pressing than the question of “what is just.” Consequently, we must begin to evaluate not only the effectiveness of various policing practices, distinguishing broad-based policies like zero tolerance from targeted policies like focused deterrence, but also the collateral consequences of various violence-reduction strategies and their larger implications for public policy.

There is little doubt that the assessments of focused deterrence initiatives in Chicago and elsewhere (Papachristos and Kirk, 2015, this issue; Braga and Weisburd, 2015) have shown sizeable reductions in gun violence among groups who experience the intervention relative to those who do not. Although the strength of the effect varies by locale, time frame, dosage, and other idiosyncratic features of the interventions as they are implemented on the ground, the direction is clear—moderate-to-large statistically significant reductions in gun violence are almost always identified. Papachristos and Kirk (2015) likewise show that, in

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the year after the implementation of Chicago's Group Violence Reduction Strategy (VRS), gang factions subject to the intervention evidenced a 23% reduction in "shooting behavior" relative to controls.

The Chicago VRS, which involved hour-long "call-ins" among carefully selected members of violent gang factions, local police, federal prosecutors, community members, and social service providers, generally mirrors similar focused deterrence initiatives based on the concept of "pulling (all available law enforcement) levers" when any member of the targeted gang faction is involved in a shooting (Braga, Kennedy, Waring, and Piehl, 2001). Gang members are put on notice that law enforcement will use all legal sanctions at their disposal to deal with gun violence. In other words, gang members can expect an increase in both the certainty and the severity of punishment. Focused deterrence is thus premised, at least in part, on the heightened scrutiny associated with police surveillance and the state's willingness to sanction aggressively; in Foucault's (1977) terms, gang factions are expected to self-regulate as they are increasingly transformed into the objects of "panoptic gaze" (see also Feeley and Simon, 1992; Garland, 2001). Focused deterrence is also premised on a second foundation seemingly at odds with the first; these initiatives incorporate a service-based, protective emphasis on harm reduction for those criminally active persons or groups most vulnerable to violence. Specific gang factions are selected for intervention by using an evidence-based, "data-driven" approach to identify those groups that are expected to be imminent targets or users of gun violence. To bolster procedural justice and legitimacy, Chicago's VRS held call-ins in local parks and included social service providers to offer community-based resources and members of the community to lend moral voices against gun violence. Papachristos and Kirk's (2015) findings contribute to a growing body of research showing promising reductions in gun violence as a consequence of focused deterrence programs.

In this policy essay, we do not explore whether focused deterrence works or how broadly it should or could be implemented. If reducing shootings in the immediate aftermath of the intervention is the yardstick by which we should evaluate focused deterrence, then it seems to be effective. Instead, we step back to raise critical questions about the contemporary model of policing in America. In doing so, we draw attention first to differences in the theoretical underpinnings of focused deterrence vis-à-vis more recent and situationally grounded theories that determine the causes of violence in immediate provocations. Second, we draw attention to the potential collateral consequences of even targeted surveillance initiatives that encompass a threat-based component. Finally, we explore how the framing of crime problems and the range of crime policies available to lawmakers has both been expanded in accordance with scholarly knowledge about violence contagion and increasingly recognizes the importance of enlarged roles for nonstate actors.

## Understanding the Theoretical Mechanisms of Violence

The findings of the Chicago VRS described by Papachristos and Kirk (2015) suggest that success hinges not on reducing all shootings but on reducing the likelihood that members of gang factions who participate in the call-in will be *shot at* by others (refer to Figure 4 in Papachristos and Kirk, 2015). In other words, although a 23% reduction in “total shootings” is identified between the treatment and control groups, virtually all of the effect is driven by a reduction in gang faction members being the *target* of shootings (i.e., shooting victimization is 32% lower for the treatment group than for controls) rather than the *shooting suspects* (n.s.). This is noteworthy as these findings complicate the implied rational cost–benefit mechanism that ostensibly drives violence reduction in focused deterrence initiatives.

The logic of specific deterrence-based strategies dictates that increases in the certainty and severity of punishment act on the decision calculus of active offenders as they confront future opportunities for crime (Clarke and Cornish, 1985; Nagin, 1998). Said differently, when sanction threats work, they operate to reduce *offending* by elevating the costs; there is little reason to expect that the victimization risks *of treatment groups* should be directly impacted (even if generalized reductions in gun violence are realized across the board). Yet, curiously, the effects detected in Papachristos and Kirk’s (2015) evaluation are apparently driven by variations in the treatment group’s risk of shooting *victimization* rather than by reductions in the treatment group’s willingness to shoot others. Indeed, gang factions experiencing the intervention and their matched controls are equally likely to be suspected of a shooting in the year after call-in attendance. What, then, can we make of the mechanism driving changes in victimization risk as a consequence of focused deterrence initiatives? Being told that every legal sanction will be levied, and that social services are available to those who put down their guns, apparently modifies the actions of others toward treatment gang factions but has little influence on their own willingness to pull the trigger.

Focused deterrence programs theorize the violent actions of gang factions by viewing members as rational actors who are responsive to cost–benefit contingencies, in that these programs both increase the legal costs of involvement in gun violence and offer alternative prosocial benefits (in the form of social service provision) to discourage it. Yet scholars have increasingly been attentive to how situational motivations emerging in the moments before violence erupts can act as immediate precursors to and causes of action (Collins, 2009; Wikström, Oberwittler, Treiber, and Hardie, 2012). In the absence of greater attention to the microdynamics of violent interactions, in terms of situational inducements, moral filters (Wikström et al., 2012), and the specific content of conflicts (Griffiths, Yule, and Gartner, 2011; Harding, 2010), it may be challenging to disrupt violence as it begins to unfold (Athens, 2005). Moreover, without attending to the content of longstanding disputes among gangs, turfs, or neighborhoods, the underlying conflicts will continue to simmer. Harding (2010: 33) argued, for example, that “beefs between neighborhoods often go back years, before today’s teens were even born, and their exact origins are almost always

unknown by the current participants.” The same can be said of certain longstanding and retaliatory intergang conflicts (Howell and Griffiths, 2016). Approaching violence reduction from a perspective that draws from the immediate situations and contexts in which violence manifests recognizes the situational inducements to violence and the importance of the content of conflicts. Programs that act primarily on the perceived legal aftermath of violent actions (which can be somewhat removed from the immediate decision calculus of actors in conflict) may have difficulty accomplishing these objectives in the long term.

### **Collateral Consequences of Increasing the Costs**

Recent research has suggested that young men who are the subject of intensive surveillance by the criminal justice system are apt to avoid what Brayne (2014) called “surveilling institutions,” including educational, financial, labor market, and medical institutions. Goffman (2014) likewise argued that young African American men in Philadelphia eschew visiting hospitals for the birth of their children, avoid visiting sick friends or family, and even forgo vital medical treatment for fear of being picked up on outstanding warrants. These accounts have drawn attention to the reality that criminally active young men, who are prime targets of focused deterrence and other types of chronic law enforcement gaze in American cities, are motivated to evade authorities and, in so doing, may avoid seeking medical treatment even in the face of serious nonfatal injuries.

How does this relate to Chicago's VRS and other focused deterrence initiatives? It is possible that the call-in warnings to deploy continuing criminal enterprise or armed career criminal statutes “in the event of the next shooting” (Papachristos and Kirk, 2015) are ambiguous or confusing for gang members. Thus, part of the dramatic reduction in risk of being *shot at* in the 12 months after call-in attendance (according to police records on arrests, homicides, and nonfatal shootings) may be related to system avoidance. If gang members in the treatment factions fear prospective punitive sanctions at being “involved” in gun violence, even as victims, then they may be less willing to seek treatment for injuries. Although this explanation is not likely to account for a large proportion of the variation in shooting victimization shown by Papachristos and Kirk (2015), the possibility that a law enforcement strategy could inhibit even some victims from reporting injuries is troublesome on two fronts. Empirically, focused deterrence initiatives would not be adequately equipped to capture all gun violence in the assessed outcome measures if treatment seeking is actually discouraged as a consequence of call-in attendance. Practically, equitable access to medical care could be further circumscribed by the intervention.

Sanction threats that elevate both the certainty and the perceived severity of punishment may also have the unintended consequence of encouraging gang members to “run” when they find themselves in dealings with police. In response to larger concerns about law enforcement overreach and misconduct, and especially in light of direct messages that becoming entangled in criminal or illicit activities will invite a barrage of penalties, gang members in the treatment faction might be incentivized to evade police contact at all costs. If

so, their prospective courses of action become effectively reduced to “running.” The instinct to run is so widespread that recently, for example, a public safety campaign in Atlanta led to the erection of “Don’t Run from Cops” billboards (although community resistance encouraged their swift removal; Visser, 2015). The deaths of Freddie Gray in Baltimore and Walter Scott in North Charleston, both of which occurred after brief police pursuits, serve as poignant (and very public) examples of the precarious risks associated with elevating the perceived costs of interactions with police. Indeed, fostering an instinct to run may, in the long term, jeopardize the safety and lives of young men of color, further undermining the perceived legitimacy of law enforcement in affected communities and beyond.

Advocates of focused deterrence would rightly argue that, by its very nature, targeting specific influential offenders in actively violent gangs, rather than instituting the broadly discriminatory practice of racial profiling that emerges in certain zero tolerance programs, for example, should heighten perceptions of procedural justice and legitimacy. However, even if members of violent gang factions are accurately identified as the persons most likely to be involved in the next shooting, missing from this narrative is recognition that those offenders are embedded in licit networks (Pattillo-McCoy, 2000). If law enforcement tactics are perceived as aggressive, unwarranted, or contrary to civil liberties by persons in these licit networks, then larger concerns about the use of threat-based initiatives may diffuse into the broader community. In an age of marked disquiet about police action, such concerns can quickly manifest in reductions in police–community cooperation, a decrease in perceptions of police and government legitimacy, and an undermining of procedural justice (Gau and Brunson, 2010; Tyler, 2006).

### **Policy Prescriptions for Violence Reduction**

Focused deterrence initiatives are applauded for but also highlight longstanding tensions in our conceptualization of the very work of the criminal justice system. Balancing the objectives of due process and crime control (Packer, 1968); satisfying shifting public opinion and the need for political expediency (Mears, 2010); and managing the on-the-ground, day-to-day functions of complex bureaucracies with deeply embedded cultures and practices is extraordinarily complicated. Focused deterrence strategies can push police organizations out of their comfort zone in that they require surveillance and crime control functions in tandem with social support and community capacity-building activities that are still anathema to many police organizations (Garland, 2001). Police are not unique in their discomfort with doing business differently even when they are equipped with programmatic resources to do so. Probation and parole, for example, have explicit mandates to provide rehabilitation and support; yet they struggle to balance such functions with risk management and surveillance (Taxman, 2008). Focused deterrence policing therefore asks police to broaden the scope and nature of their work as well as their very definition of the “problem” of crime and violence.

When considering the policy implications of Papachristos and Kirk’s (2015) work, we observe a portal for crucial discussions about the framing of crime problems in the

United States and, subsequently, the range of responses our society is willing to consider. For example, the CureViolence program in Chicago and elsewhere (briefly discussed by Papachristos and Kirk, 2015) is a public health-based violence reduction strategy developed by a medical doctor with extensive experience in the prevention of infectious disease (Slutkin, 2012). CureViolence seeks attitudinal and behavioral change by determining the most effective “message” for altering behavioral norms and delivering that message from multiple sources. Peer influence is a hallmark of the approach wherein “violence interrupters” are dispatched to diffuse potentially violent conflicts as the dispute unfolds, and outreach workers are dispatched to offer social services in the long term. Violence interrupters and outreach workers, who are typically former gang members or formerly incarcerated individuals, are carefully selected for their credibility (and legitimacy) with young people. State agents, such as the police, do not play a role in the initiative. Although empirical support for the effectiveness of CureViolence is mixed (Butts, Roman, Bostwick, and Porter, 2015), approaching crime and violence as a contagious disease rather than as a personal and moral failing shifts our attention from surveillance and punishment to prevention, harm reduction, and cure. As Butts et al. (2015) noted, enforcement-centered strategies are more readily embraced by public officials because such approaches are familiar. Broadening the range of possibilities in addressing violence is critical to innovation, as well as to deep and lasting change.

There are some indications of a shift from decades of punitive, control-oriented criminal justice policies on the horizon (Clear and Frost, 2013). Drug courts, restorative justice community conferences, and prevention programs aimed at early intervention and community capacity building have gained support but remain the exception rather than the rule. Such approaches have the potential to address the underlying causes of violence and to enhance legitimacy and procedural justice, making people less compelled to evade police. Thus focused deterrence policing potentially stands at the juncture between the best and the worst of criminal justice practices. It is undoubtedly a bold innovation. Yet, moving forward, it will become increasingly important to evaluate directly the perceptions and responses of those who are the subjects of focused deterrence interventions and to consider the broader collateral consequences of any initiative with a threat-based component for police–community relations.

## Conclusions

The literature is reasonably well established; generally, scientific evidence shows that focused deterrence programs are successful at reducing a host of criminogenic outcomes, including gang conflict, domestic violence, drug market activity, robberies, and especially gun violence (Braga and Weisburd, 2015). Scholars have raised legitimate questions about the possibility of temporal decay in the strength of effects, the appropriate emphasis on service provision carrots versus law enforcement sticks, and issues of external validity (see Engel, Tillyer, and Corsaro, 2013); yet quasi-experimental research and rigorous evaluations have generated

a growing body of evidence that (at least short-term) reductions in gun violence can be realized among the treatment groups. With this evidence, we do not argue. Rather, we view this policy essay as an occasion to step back from the finer points of intervention and evaluation to assess whether, in this moment, even more comprehensive overhauls to the contemporary model of control-based policing are needed.

The U.S. Department of Justice's (2015) recent report on the Ferguson Police Department has articulated the many and varied ways that all citizens, but African Americans and young men in particular, have suffered routine harassment in the day-to-day standard operations of policing in America's cities. Widespread use of surveillance, control, fines, and threats amount to what some might reasonably call "institutional bullying." To the extent that these practices are targeted toward specific high-rate offenders or "influential" members of violent gang factions (and their peers), proponents of focused deterrence correctly contend that these programs promote less biased and discriminatory policing practices. But among those who are targeted in interventions involving a threat-based component, and among their family, friends, and acquaintances who may view the intensive gaze as more evidence of the regular violation of civil liberties, we can predict increasingly evasive action on the part of young men in high-crime areas. We might also anticipate broader and more pervasive disenchantment with a system in which targeted surveillance primes young men to run from law enforcement, running leads to more police-involved fatalities, and the whole machinery of threat and control is called into question time and again.

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