Community-driven violence reduction programs

Examining Pittsburgh's One Vision One Life

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Despite some evidence of reductions (FBI, 2009a), violent crime remains among the most important social problems affecting the quality of life in communities throughout the United States. Aggregate reductions also mask the variability in violence among and within communities. The total number of persons annually victimized by violence remains high. In 2008, more than 9,000 persons were killed with guns (FBI, 2009b). In 2006, 71,000 persons suffered nonfatal gunshot wounds, and 2.1 million persons sustained an injury requiring emergency-room treatment as a result of a violent incident (CDC, 2010). Overall, more than six million individuals were victimized by crimes of violence in 2006 (BJS, 2007). One comprehensive review of gun research indicated that firearms play a significant role in violence and that young persons are particularly vulnerable to violence and death from firearms (Wellford, Pepper, and Petrie, 2005).

The impact of violent crime on individuals, families, and communities is substantial. Some estimates indicate that the annual costs of gun violence are approximately $100 billion (Cook and Ludwig, 2000). The annual costs of all personal victimization by violence, including intangible losses such as pain, suffering, and reduced quality of life, are more than $450 billion (NIJ, 1996). This figure is dated and likely to be significantly higher today.

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Indeed, Cook and Ludwig (2000: 138) suggested that "the costs of violence are so great that effective interventions essentially pay for themselves."

The extent of violence and its impact highlight a critical need to develop and implement effective programs to reduce it. Many communities have initiated a wide range of responses to violent crime, firearm-related violence, and drug crimes. These interventions cover a wide range of approaches, including public health, media publicity, technology, community-driven, and criminal justice initiatives. Scholars have produced an overwhelming number of studies on these initiatives using data and methods of evaluation that range greatly in quality. Although previous evaluations indicate that there are certain types of strategies and specific programs that are promising, there is still a great need for additional critical evaluations. As the National Institute of Justice (NIJ) (2002: 19) noted, after compiling and analyzing a representative selection of NIJ research on gangs, there remains "a need to know 'what works'. . . too little is known about the relative merits of comprehensive, broad-based interventions." More recently, Weisburd and Neyroud (2011: 11) reiterated that, "what is most striking about policing is that we know little about what works, in what contexts, and at what costs." Moreover, most evaluations of gang interventions examine enforcement strategies that are primarily implemented by law enforcement organizations. In short, a critical need remains for researchers to evaluate promising strategies rigorously, to broaden understanding of promising strategies by replicating them and their evaluations at other sites, to identify why and what about such programs work, and to assess the impact of nonenforcement-related strategies.

In this article, we assess a Pittsburgh, Pennsylvania–based violence-prevention strategy known as One Vision One Life (or One Vision). In 2003, Pittsburgh had a record-setting 70 homicides, a 49% increase over 2002, with the homicide rate that year increasing from 14 per 100,000 to 22. The homicide rate in Pittsburgh in recent years has been higher than that elsewhere in the nation and, since 2001, than in other cities with 250,000 to 500,000 residents. This increase in violence rallied a coalition of community leaders who formed the Allegheny County Violence Prevention Initiative, which became One Vision One Life. Real increases in certain types of crime, as observed in Pittsburgh, as well as perceptions that a type of crime is "getting out of control," can often lead communities and their leaders to adopt well-meaning but not always well-considered responses. One Vision staff, however, planned their response carefully by examining systematically the nature of violence, considering best practices from other communities across the nation, coordinating with key community partners, communicating with law enforcement, and adopting a strategy they felt was appropriate for responding to the problem and consistent with the goals of the initiative.

Borrowing aspects from several promising evidence-based models, One Vision seeks to prevent violence using a problem-solving, data-driven model to inform how community organizations and outreach teams respond to homicide incidents. It also uses street-level intelligence to intervene in escalating disputes and seeks to place youth in appropriate social

programs. One Vision shares information with law-enforcement officials, but it is truly a grassroots effort. Its evaluation has practical and theoretical value.

This assessment of One Vision builds on prior research, and policy makers and scholars should be interested in the findings for several reasons. First, although there is a rich literature evaluating various types of violence-reduction strategies, there have been few quality studies of community-initiated actions that could be thought of as an alternative to strictly an enforcement strategy. Most evaluations have focused on interventions led by the criminal justice community, but the initiative discussed in this article was designed to be representative of evidence-based practices that have been shown to work from public health, social services, and criminal justice disciplines. Second, a critical element of this strategy is to involve non–criminal-justice personnel, usually former gang members, in mediating potential violent conflicts. Although the involvement of "street workers" has been part of other well-known violence reduction strategies like Boston's Lever Pulling initiative, few studies are available and some raise concerns about their effectiveness (see Klein, 1971). We discuss these studies in the context of our results. Third, the intervention is modeled after (but does not mirror) a similar strategy that has been implemented in Chicago, Baltimore, and several other cities. In fact, personnel involved in the Pittsburgh program visited Chicago in late 2004 and early 2005 and attempted to model the intervention and their data collection after CeaseFire in Chicago. Fourth, this type of intervention has been evaluated carefully in Chicago and Baltimore (see Skogan, Hartnett, Bump, and Dubois, 2008; Webster, Vernick, and Mendel, 2009), but an additional evaluation of this type of intervention can yield new lessons about the promise and possible pitfalls of such a strategy. Exploring the program's effectiveness relative to variation in implementation, local dynamics, and community characteristics is helpful for assessing the likelihood that this program could succeed elsewhere. Such lessons would be a useful resource for policy makers, practitioners, communities, and researchers. Finally, the results in this article are not only different that what was observed in the other studies, but it seems that this program led to an increase in violence in the target neighborhoods. We discuss the potential reasons for these increases and the implications for these types of strategies.

Literature Review

In this literature review, we review first the literature relevant to understanding the potential impacts of the model. Specifically, we examine research on problem-solving, street workers, and community outreach initiatives. Second, we review the small number of studies that examined the impacts of programs designed similarly.

Problem Solving, Homicide Incident Reviews, and Collaborative Partnerships

One of the most significant developments for initiating change within criminal justice organizations is the application and adoption of problem-solving approaches. The theory behind the approach has been adopted widely and used successfully in multiagency

collaborative partnerships (Dalton, 2003). There are many examples of criminal justice officials systematically collecting data to examine a crime problem more completely, to develop and implement innovative responses, and to assess the impact of these responses. New York City's CompStat program is probably the best-known example of formulating this process into everyday organizational decision making (Silverman, 1999), and the Boston Gun Project is often used as a program that demonstrates the potential of systematic data analysis (Kennedy, 1997, 1998; Kennedy, Piehl, and Braga, 1996; NIJ, 2001). Analyses of the Boston Gun Project found several benefits (see Wellford, Pepper, and Petrie, 2005). Violent gang offending slowed dramatically, and youth homicide in Boston fell by two thirds after the strategy was put into place (Kennedy, 1998: 3). The intervention also led to a 63% decrease in the monthly number of youth homicides, a 25% decrease in assaults with firearms, and a 32% decrease in shots fired. Boston experienced a greater (statistically significant) decrease in youth homicide than did 39 other comparison cities (Braga, Kennedy, Waring, and Piehl, 2001; see also Braga and Pierce, 2005). Minneapolis also experienced sharp reductions in homicide after implementing a similar strategy (Kennedy, 1998; Kennedy and Braga, 1998).

This success led NIJ to support efforts to replicate similar Strategic Approaches to Community Safety Initiatives (SACSIs) in ten other cities, ultimately leading to national deployment of the Project Safe Neighborhoods (PSN) initiative by the Department of Justice (Coleman, Holton, Olson, Robinson, and Stewart, 1999; PSN, n.d.). Recently, the principles of problem-oriented policing generally and PSN have been extended to a drug market initiative (see Corsaro, Brunson, and McGarrell, 2009). Although the deployment of this model elsewhere has not been examined as closely as it was in Boston, there is some evidence of similar promise. For example, the Indianapolis Violence Reduction Partnership helped reduce homicides from 155 in 1997 to 101 in 2000, making Indianapolis the only city among six comparison cities to experience a statistically significant change in homicide frequency (Chermak and McGarrell, 2004; Corsaro and McGarrell, 2009; McGarrell, Chermak, Wilson, and Corsaro, 2006). A national evaluation of ten SACSI sites concluded that, when the SACSI approach is implemented effectively, it "is associated with reduction in targeted violent crime in a community, sometimes as much as 50%" (Roehl et al., 2006: 2). Similar positive results are emerging from select PSN sites that have implemented the problem-solving model (McGarrell, Hipple, and Corsaro, 2007; McDevitt, Braga, and Cronin, 2007; Papachristos, Meares, and Fagan, 2007) and from a national assessment of the PSN initiative (McGarrell, Corsaro, Hipple, and Bynum, 2010).

One intriguing element of the Pittsburgh One Vision approach to violent crime is that, although it is only loosely linked to law enforcement, it embraced the problem-solving model. Concerned officials and community leaders completed a systematic review to improve their understanding of the nature of the problem before acting. They discovered an important and familiar pattern: A small group of chronic offenders in just a few neighborhoods accounted for a large share of all homicides. They also found that young

Black males living in several high-crime neighborhoods were significantly more likely to be homicide victims and that more than 60% of the homicides in Pittsburgh occurred in just four neighborhoods. The homicide rate for Black males living in just a few neighborhoods was 423 per 100,000—more than 50 times the U.S. rate (One Vision One Life, 2005). These neighborhoods became some of the target neighborhoods chosen for a strategic response. Violence data continue to guide the program's intervention strategies, as they did when One Vision expanded its Pittsburgh Southside target area when it became clear that incidents in its original target neighborhood were spilling into adjacent neighborhoods.

Conflict Intervention and Mediation: Street Workers and Street Intelligence

One Vision community coordinators use street-level intelligence to become aware of and then intervene in potentially violent altercations. The coordinators, who are selected because of their familiarity with and connections to the targeted neighborhoods and knowledge about rival groups, are trained in dispute resolution, conflict mediation, and culturally sensitive outreach. They work to prevent violence in three direct ways:

1. They attempt to defuse disputes, such as a petty argument or turf battle, before they escalate.
2. They coordinate public and behind-the-scenes responses to every homicide (and shooting, when awareness of the incident is timely) that occurs in the targeted neighborhoods.
3. They connect individuals and specifically youths to critical services.

Responses to homicides include gathering intelligence about the situation and talking with key actors (e.g., the victim's family, the perpetrator, or others who might be involved in any ongoing dispute) to mediate or minimize the violence and disseminating a general antiviolence message by providing resources, materials, and information to residents.

This is similar to the underexamined role that street workers and community organizations played in contributing to the success of the Boston Gun Project. Boston street workers identified at-risk youth and worked to provide them with critical services, such as job training and substance-abuse counseling. They mediated disputes between rival gangs and worked with law enforcement to prevent violent outbreaks (Braga and Kennedy, 2002). These street workers also worked closely with the Boston TenPoint Coalition—a group of activist Black clergy that also tried to link youths with social services and worked with law enforcement to resolve disputes. Few data exist on the work of street workers and community organizations, which was not measured in any substantive way. This is unfortunate especially given contentions that the TenPoint Coalition was critical to the decreases in youth violence through its creation of an "umbrella of legitimacy," providing balance to the inner-city community and law enforcement that did not exist (Winship and Berrien, 1999). Other cities, such as Indianapolis and Rochester, New York, also have implemented a clergy or street-worker coalition as part of a larger violence-reduction

strategy. Yet we have little understanding of whether or how these are effective and how they might be transferred to other cities and programs. Importantly, scholars have identified several potential problems and weaknesses in the delivery of service by street workers. For example, Klein's (1971) important study of programs in Los Angeles highlights potential weaknesses, including lack of supervision, lack of focus, and goal confusion. Moreover, he found that gang workers spent only approximately 20% of their time monitoring gang members, concluding "it may be like squeezing blood out of a turnip to think that an average of five minutes per week per boy could somehow result in a reduction of delinquent behavior" (p. 163). Klein (1971: 151) raised the possibility of street workers contributing to a "paradox of programming," whereby meeting with gang members might actually increase delinquency by increasing the potential cohesiveness of the gang. An evaluation of the Pittsburgh program can expand understanding of the impact of street work in that the program uses primarily former gang and other individuals with criminal justice histories.

Community Mobilization and Outreach

One Vision coordinates broadly and to varying degrees with other community and social service agencies, businesses, and law enforcement. Much of the violence in the areas it targeted stemmed from the illicit drug trade. In its broad approach, it is similar to effective programs that addressed neighborhood drug problems from multiple perspectives with a diverse array of resources and that were connected to broader neighborhood quality-of-life issues (Corsaro et al., 2009; Weingart, Hartmann, and Osborne, 1994). A better understanding is needed of how broader efforts, such as that in Pittsburgh, can harness community capacity to combat both relatively narrow problems, such as the drug trade, and broader problems, such as crime.

Macrolevel variables, such as economic inequality, politics, racism, and demographics, certainly have a greater impact on neighborhood crime, disorder, and quality of life than anything law enforcement or community organizations do (see Duffee, Renauer, Scott, Chermak, and McGarrell, 2006; Skogan, 1990; Spergel, 1976; Wilson, 1987). Yet community organizations or law enforcement can still mediate the impact of these broad social forces on residents (Byrum, 1992; Cortes, 1993; Grogan and Proscio, 2000; Sampson, Raudenbush, and Earls, 1997; Spergel, 1976). As Duffee et al. (2006: 2.7) noted, "[t]here are numerous actions that can be and are taken within neighborhoods and between neighborhoods and outsiders that are an effective component of a larger, more encompassing community improvement strategy." For One Vision, these actions include working in the community to build broad-based sustainable partnerships, significantly increasing the community's commitment to its most troubled neighborhoods, reducing the isolation of the residents living in these neighborhoods, and linking residents to social service organizations as well as organizations to each other.

Research on Similar Initiatives

There have been two other evaluations of programs like One Vision. These evaluations are discussed subsequently. Although the results show generally positive effects for such strategies, it is important to test the effectiveness of the model in other cities with different types of offense and program challenges.

CeaseFire Chicago. As noted previously, the individuals involved in the creation of One Vision were significantly influenced by a program administered by the Chicago Project for Violence Prevention called CeaseFire Chicago (Skogan et al., 2008). CeaseFire Chicago began in 1999 and underwent a rigorous NIJ evaluation, led by Wesley G. Skogan, in 2005. The process evaluation included surveys of staff, interviews with clients and collaborators (e.g., community, clergy, business, police, and school representatives), and observation of meetings. The impact assessment compared changes in violent crime, hot spots, and gang-related changes that occurred in seven CeaseFire sites with those that occurred in other matched areas.

The researchers found that the program contributed to statistically significant decreases in shootings and attempted shootings, the size and intensity of hot spots, gang homicide density, reciprocal killings, and gang homicides in many of the research areas evaluated relative to the comparison sites (Skogan et al., 2008). The researchers examined the impact of the program in seven of 25 program areas, comparing the results with matched areas. Although violence in Chicago was generally down in all areas during the evaluation period, the study indicates that the program pushed key violence indicators down even more. Specifically, shootings and attempted shootings decreased in four of the seven areas between 17% and 24%. An analysis of hot spots in the program areas indicated that six of the seven sites were safer, and "there was evidence that decreases in the size and intensity of shooting hot spots were linked to the introduction of CeaseFire in four of these areas" (Skogan et al., 2008: 8–15). A critical component of the analysis was examining the impact on gang-related activities and homicides. The findings indicate that gang homicide density, reciprocal killings, and gang involvement in homicides decreased in about half of the areas examined.

Baltimore Safe Streets Program. To date, there has only been an interim evaluation of Baltimore Safe Streets (Webster et al., 2009). This program was modeled after Chicago CeaseFire. The analysis focused on differences between attitude changes and program effects on violence in the target areas and a comparison area. The analyses indicated that participants' views on gun violence were much different in one of the target areas. The analysis found, even after controlling for other variables, significantly reduced support for gun violence to settle disputes in McElderry Park but no significant change in Ellwood Park. Controlling for various indicators, the results indicated that being a resident in McElderry reduced support of gun violence to settle disputes.

The reduced support for violence in McElderry Park was coupled with overall positive results for the program there. The area had seen "an average of 0.31 homicides per month

(3.7 per year) during the months prior to the implementation of Safe Streets in August 2007, but no homicides during the 14-month follow-up period," a reduction that was also statistically significant (Webster et al., 2009: 9). There was some diffusion of benefits to surrounding communities, where homicides also decreased. The program also led to a reduction of youth homicides in McElderry Park. The evaluation found no effect of the program in Ellwood Park, but there was an upturn in homicides in Union Square. The evaluation found an association with the program and fewer nonfatal shootings in Ellwood Park but with more such shootings in McElderry Park and Union Square. We discuss the Chicago and Baltimore programs subsequently.

Methods

The main focus of our analysis is to examine what impact, if any, One Vision had on violence in the targeted and surrounding communities. It is important to note, however, that we conducted a comprehensive implementation assessment as well, including field observations, interviews, and police ride alongs. These results are available elsewhere (see Wilson, Chermak, and McGarrell, 2010) but will be referenced in the Conclusions in an attempt to improve our understanding of the nature of the impacts.

Impact Assessment

We examined the impact of One Vision on violence using a quasi-experimental design that compared violence trends in the program's target neighborhoods before and after implementation with (a) trends in Pittsburgh neighborhoods where One Vision was not implemented through a propensity-score analysis and (b) trends in specific nontarget neighborhoods whose violence and neighborhood dynamics One Vision staff contended were most similar to those of target neighborhoods.[1] As part of the outcome analysis, we also explored the extent to which violence or violence-suppression benefits "spill over" into neighborhoods that are adjacent to the target neighborhoods. One Vision's primary goals were to reduce homicide and shootings. Given the data were at the neighborhood level, the outcome models assessed intervention effects by comparing the average outcome for the target neighborhoods with the average outcome for the nontarget neighborhoods. This is a standard way of assessing a difference in difference. Consistent with One Vision's first goal, we drew on existing data to incorporate homicides as an outcome variable. Unfortunately, changes in how Pittsburgh police recorded incidents precluded us from directly measuring

1. The untreated control group design with multiple pretests and posttests (Shadish, Cook, and Campbell, 2002) is a widely used quasi-experimental design that accounts for most threats to internal validity except selection bias or the chance that something "unique" and unobserved about the target or comparison areas influenced levels of violence in them and hence measurements of program effectiveness. Fortunately, propensity-score weighting and our ability to examine the impact of One Vision in multiple target areas with multiple start dates using two sets of comparison neighborhoods help limit selection bias.

progress toward the second goal of reducing shootings. For proxy variables, we gathered data on aggravated assaults and aggravated assaults with a gun. Although these categories of violence include shootings and might indicate program effects, they also include other forms of violent acts and hence are not a precise measure of One Vision's success in reducing shootings.[2]

The Pittsburgh Bureau of Police provided incident-level data for homicides occurring between January 1, 1997 and December 31, 2007, as well as for aggravated assault and gun assaults between January 1, 1996 and December 31, 2007. We aggregated these data into monthly counts for each neighborhood. The Pittsburgh Department of City Planning provided all remaining variables, which were extracted from the 2000 census (Department of City Planning, 2006).

Analyzing the effect of One Vision posed several challenges. Chief among these was that the implementation of the program was not random but based on levels of violence and expert opinion of the areas most suitable for it. This created the possibility that something particular about the neighborhoods chosen, aside from the One Vision program, could account for any change in levels of violence—or, specifically, in homicides, aggravated assaults, and aggravated assaults with a gun—after implementation.

To help control for the possibility of such selection bias, we used the statistical method of propensity scores (Rosenbaum, 2002; Rosenbaum and Rubin, 1983) to find the most appropriate (simulated counterfactual) neighborhoods to compare with the One Vision neighborhoods. For a sensitivity analysis, we used expert opinions in a subsequent analysis to select a second set of counterfactual neighborhoods and compared them with the One Vision neighborhoods. Finally, to assess whether One Vision had an impact beyond the target neighborhoods and into the neighborhoods surrounding them, we conducted a spillover analysis. Next, we summarize our approach to these analyses and present the results of them.

Defining One Vision's Target Neighborhoods

Pittsburgh is made up of 89 officially recognized neighborhoods that vary in size from 39 to 14,507 residents. One Vision was implemented in three target areas, each of which contained multiple neighborhoods. Becoming the target neighborhoods, the Northside included 18 neighborhoods, whereas the Hill District and Southside contained

2. In conducting our impact analyses, we attempted to minimize type I and II errors. To minimize the probability of rejecting a null hypothesis when it is true (type I error), we used a .05 alpha level, a standard benchmark, as the criterion to determine statistical significance. The probability of not rejecting the null hypothesis when it is false (type II error) relates to the ability to detect whether One Vision was associated with some change in the violence measures when it actually was. Such error is a function of sample size. We attempted to minimize it by expanding our sample as much as possible. We compiled longitudinal data on each neighborhood in our analysis. This yielded at least 3,036 observations (and as many as 10,512 observations) for each of our impact models. See the Outcome Models section for an example of how the sample size for each model is calculated.

6 and 8 neighborhoods, respectively. One Vision began operating in its Northside and Hill District target neighborhoods in May 2004; it expanded to eight Southside neighborhoods in May 2005.[3] The differential start dates enabled us to assess the impact of One Vision at two unique intervention points, strengthening the validity of our analysis and reducing the chance that some other unseen variable was the true cause of any program effects.

Designing the Simulated Counterfactual

Comparison Neighborhoods. Assessing the impact of a violence-prevention strategy, or any social program, requires comparing the actual experience of an area where a program was implemented to some benchmark on what likely would have occurred there without it. One of the greatest challenges to gauging a strategy's effectiveness is choosing or designing a comparison or counterfactual that best represents what a target neighborhood would experience without any sort of intervention. Ideally, an intervention would be assigned randomly to a large number of neighborhoods so that the intervention and nonintervention neighborhoods are statistically equivalent, meaning that any preexisting differences would be simply a result of chance. This standard is difficult to attain in field settings. In the case of One Vision, for example, community leaders chose target neighborhoods based on their assessment of which had the greatest propensity for violence and highest likelihood for One Vision to work effectively. So researchers instead select for comparison neighborhoods that are similar or are somehow matched to the target neighborhood on key dimensions related to the outcome variables (in this case, measures of violence). As a quasi-experiment, such a design cannot rule out every threat to validity (i.e., the ability to link outcomes to the intervention). Nevertheless, when conducted properly, quasi-experiments represent the best available option for assessing program effectiveness.

To begin evaluating One Vision's effect on violence, we weighted the 55 nontarget neighborhoods (i.e., all other Pittsburgh neighborhoods not chosen as a target) based on how well they matched the target neighborhoods. These nontarget neighborhoods represented a simulated counterfactual for the target neighborhoods without the intervention. All nontarget neighborhoods were used in the analysis, so we lost no cases in the matching process. Here, we used the method of propensity scores (Rosenbaum, 2002; Rosenbaum and Rubin, 1983) to reduce selection bias. This strategy has been used previously to assess neighborhood effects (Tita and Ridgeway, 2007; Tita et al., 2003). The method of propensity scores can produce causal estimates using observational data by weighting or

3. In May 2004, One Vision also started working in the neighborhoods of Beltzhoover and Saint Clair, which are traditionally considered "Southside" neighborhoods. However, we excluded these from the analysis because, given the different start date from the other Southside neighborhoods, they would need to be modeled independently from the other Southside neighborhoods and with only two neighborhoods the model may have produced unreliable estimates. Given these neighborhoods received One Vision services that could have affected violence, they were also inappropriate to use as counterfactual neighborhoods. We therefore excluded them from our analyses.

TABLE 1

Comparison of Target and Nontarget Neighborhood Characteristics

	Target Neighborhoods		Nontarget Neighborhoods Before Propensity Weighting		*p*	Nontarget Neighborhoods After Propensity Weighting		*p*
	Mean	*SD*	Mean	*SD*	Value	Mean	*SD*	Value
Homicide rate in 2003	0.41	0.56	0.61	2.43	.38	0.63	1.59	.90
Aggravated assault rate in 2003	13.95	26.41	9.25	31.18	.01	18.60	46.27	.94
Gun assault rate in 2003	5.18	9.82	2.31	5.37	.01	4.65	7.68	1.00
Population density	8.22	9.26	6.33	4.41	.42	6.35	3.83	.94
% population aged 15–24 years	14.97	9.23	16.60	12.36	.37	16.06	10.22	.57
% no high-school grad	24.90	9.81	20.39	11.77	.04	24.27	9.29	.61
% Black	45.47	35.00	29.13	32.71	.06	47.11	36.39	.92
% professionals	0.25	0.12	0.34	0.16	.00	0.27	0.12	.87
% income < $25,000	53.95	17.76	45.50	14.93	.03	51.49	13.17	.74
% in poverty with child	11.27	13.40	6.34	7.06	.01	9.50	7.75	.77
% public assistance	10.55	9.63	6.76	8.37	.00	9.31	6.68	.60
% vacant housing unit	18.97	14.49	11.97	8.61	.01	17.11	8.89	.76
% moved in 5 years	42.67	13.55	38.19	11.94	.11	39.73	9.88	.74

matching different neighborhoods in a way such that target and nontarget neighborhoods have similar characteristics, thereby reducing selection bias in the process of comparison.[4] The propensity score for a neighborhood is the probability that a neighborhood with a particular set of features is a member of a target neighborhood. We employed a two-step process for estimating the propensity scores. First, we sought to control for as many neighborhood characteristics as possible; yet we were sensitive to our sample size and the available power to detect statistically significant differences. We therefore employed logistic regression using the backward selection method to identify the variables that should be used in the estimation of propensity scores. Initially beginning with 30 socioeconomic-demographic neighborhood characteristics, the selection process identified 13 characteristics useful for calculating propensity scores. These are listed in Table 1. Second, we estimated the propensity score with generalized boosting methods (GBM) using the 13 neighborhood

4. See Apel and Sweeten (2010) for an overview of the propensity-score methodology and its use in criminology.

characteristics potentially correlated with the violence rate in a neighborhood.[5] When fitting this model, the outcome was an indicator of whether a neighborhood was a target neighborhood, and the covariates were the neighborhood characteristics. Table 1 illustrates that after propensity score weighting, no statistically significant differences were found between target and comparison neighborhoods relative to these characteristics. We used the resulting model to predict the probability of intervention assignment for every neighborhood in the sample.[6]

A second way we tested for an impact of One Vision was to compare changes in the outcome variables in the target neighborhoods with a set of neighborhoods One Vision staff advised were most like the target neighborhoods. One Vision staff suggested 17 neighborhoods for this. We used these neighborhoods to create another comparison area, which permitted an additional test of impact that had face validity as determined by local experts.

Spillover Areas. In addition to intervention effects in the target neighborhoods, it is possible that the One Vision program produced displacement effects in nearby neighborhoods. The program might have shifted violence from neighborhoods where outreach and other program activities were focused to surrounding neighborhoods where they were not. Conversely, some researchers (Clarke and Weisburd, 1994; Eck, 1993; Weisburd et al., 2006) contend that interventions might extend crime-suppression benefits. Accounting for such possible "spillover" effects is necessary to gauge the true benefits, or possible drawbacks, of the program.

We analyzed the possible spillover effects for the Hill District and Southside. We did not do so for Northside because it is largely surrounded by the Ohio and Allegheny Rivers, which, local experts contended, largely separate the area from the rest of the city. Our methods for the spillover analysis were similar to those for our counterfactual comparison analyses. We determined the extent of a spillover effect through change in violence in the

5. Following McCaffrey, Ridgeway, and Morral (2004), we used GBM to estimate propensity scores. GBM is a flexible nonparametric approach to modeling $\log(p_i / (1 - p_i))$ that handles a large number of variables in an automated and systematic manner. Ridgeway and McCaffrey (2007) showed that it provides estimated propensity scores that yield better estimates of effects than other approaches do. In particular, GBM automatically selects parameters for inclusion in the model and does not arbitrarily exclude potentially important predictors. It also allows for interaction and nonlinearity in the propensity scores. With p_i estimated for each neighborhood, we used $w_i = 1 / p_i$ as the weight to be used in the Poisson regression model.

6. A common method for selecting comparison neighborhoods among all candidate nontarget neighborhoods involves matching every target neighborhood with the nontarget neighborhoods that have the most similar propensity score. This process eliminates nontarget neighborhoods that are dissimilar to the target neighborhoods. The nontarget neighborhoods matching a target neighborhood are used as simulated counterfactual neighborhoods without the program. In our analysis, we used an improved version of the propensity-score method called doubly robust (Kang and Schafer, 2007; Robins and Rotnitzky, 2001) because it can yield more consistent estimates.

neighborhoods that were each adjacent to the Hill District (6) and Southside (6) relative to all other nontarget neighborhoods at the time One Vision was implemented (43).

Outcome Models

To estimate the outcome models, we employed Poisson regression, which often is used to model information on counts, such as the number of homicides in a neighborhood, where lower bound values are truncated at zero and upper bound values have no limit. Because the neighborhoods differ in size, we modeled for violence rates, or the number of incidents per 100,000 residents. For the outcome Y_{it}, the number of homicides (or aggravated assaults or gun assaults) in a given month or year t in neighborhood i, for example, the probability of observing any specific number of crimes depends on a unique parameter, the mean number λ_{it} of crime, which for this distribution, turns out to be the same as the variance of the distribution. We model the count of incidences using the regression

$$\log\left(\frac{\lambda_{it}}{N_{it}}\right) = \mu_i + \alpha_1 \text{Treat}_{it} + \alpha_2 \text{Post}_{it} + \alpha_3(\text{Treat}_{it} X \text{Post}_{it}) + \beta\, \text{Month}_{it} + \beta\, \text{Year}_{it} + \beta\, \text{X}_{it,} \mu_i \sim N(\theta, \tau^{2,})$$

where X_{it} represents neighborhood characteristics including the population density per square mile and the proportions of employed residents in a professional occupation, housing units that were vacant, population aged 15 to 24 years old, residents aged 5 years or older who lived elsewhere 5 years previously, households with public assistance income, and households with an annual income less than $25,000. Treat_i represents the treatment of interest, taking a value of 1 for target neighborhoods and 0 for nontarget neighborhoods. With monthly homicide data collected from January 1997 through December 2007 and monthly aggravated assault and gun assault data collected from January 1996 through December 2007 by neighborhood, an indicator (POST_{it}) of the crime data before and after implementation also is included as well as an interaction between the treatment and the postimplementation that allows for an estimation of the change in crime between treatment and nontreatment neighborhoods, a difference in difference.

This model controls for a month and year effect to capture trends and serial dependence, as well as a random neighborhood effect μ_i normally distributed with mean θ and standard deviation τ. Because some neighborhoods were more populated than others, we used the population size N_{it} in a neighborhood at time t as an offset. It allowed for the estimation of rate of crime per person. $e^{\alpha 1}$, the exponential of the treatment regression estimate $\alpha 1$, the main effect, is the ratio of the rate of crime between target and nontarget neighborhoods (when the treatment of interest is the One Vision program). Because our interest was in α_3 the interaction effect, which is a straightforward difference of difference in the case of a linear model, and because we used a Poisson regression, we used the method of predictive margins to turn our estimates into expected count of crime per 100,000 persons in a

neighborhood.[7] This yields a difference-in-difference equivalence to the interaction effect. From the Poisson regression model, this estimated an average count of crime "hypothetically assuming" that all of the neighborhoods were nontarget neighborhoods and then estimated an average "if hypothetically" One Vision was implemented in all neighborhoods. The difference between those obtained crime counts is equivalent to the main effect, α_1. We did a similar transformation for the pre–post effect as well as the interaction (i.e., the difference in difference).

The number of observations used to estimate each model is a function of the number of neighborhoods in the particular analysis and the number of months for which we have data. For example, the model used to estimate the impact of One Vision by comparing homicides in Northside with those in all other nontarget neighborhoods is 9,636. This is calculated by multiplying the number of neighborhoods in the analysis, 73 (18 target plus 55 nontarget), by the number of months for which we have data, 132 (11 years of 12 months each). We had full data for each neighborhood, so we lost no cases in the analysis. Census-derived socioeconomic-demographic variables were constants and not adjusted or interpolated in any way.

One Vision's Impact

General Violence Trends

Before exploring the empirical impact of One Vision, it may be helpful to review the general trend of violence in the target and comparison neighborhoods. Figures 1–3 show the annual counts of homicides, aggravated assaults, and aggravated assaults with a gun (or gun assault) in the neighborhoods that comprise the three target areas and the nontarget area. The behavior of the trend is much more illustrative than the aggregate level as the number of neighborhoods differs in each area (e.g., the nontarget area has the highest level of violence in each of the figures because it contains many more neighborhoods than the target areas). These illustrate general increases over time. Keeping in mind the frequency is low, homicide levels spiked in 2003 and then temporarily fell in 2004 (Figure 1). At this point, they generally increased in the nontarget and Hill District neighborhoods and fell in the Northside neighborhoods. Figures 2 and 3 highlight that aggravated and gun assaults spiked in the nontarget and Northside neighborhoods in 2002 and in the Hill District and Southside neighborhoods in 2003. In 2006, Hill District aggravated assaults spiked again. By 2007, Northside aggravated assaults spiked, whereas they fell in the Hill District (they remained relatively stable in the nontarget and Southside neighborhoods). From 2004 to 2007, the neighborhoods in each of the areas exhibited different gun assault patterns.

7. Because the Poisson regression coefficients can be interpreted only as the expected increase (or decrease) in the log count of violence per population size as a result of One Vision, we converted the regression estimates into the estimated number of count per 100,000 people using the method of predictive margins (Graubard and Korn, 1999).

FIGURE 1

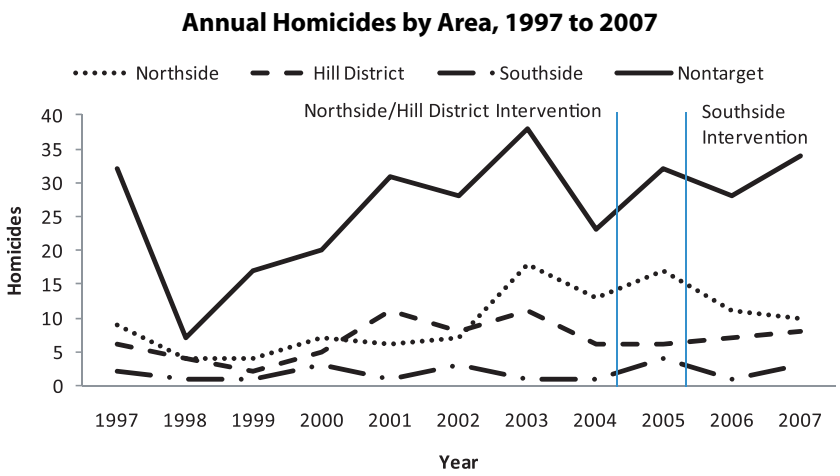
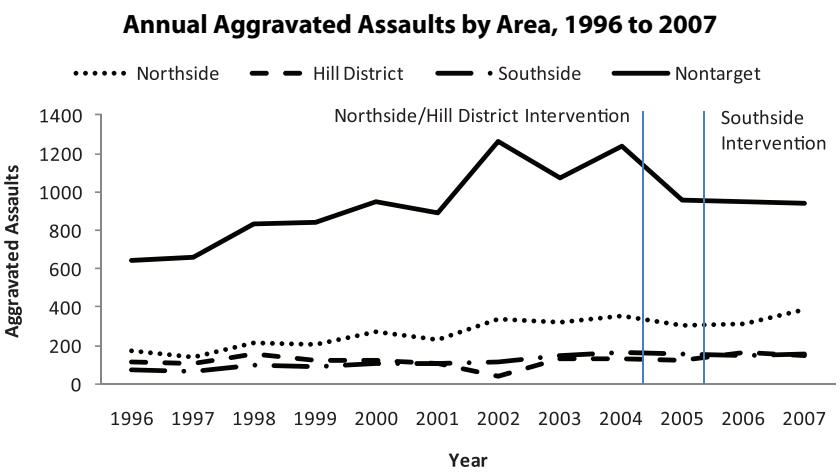


FIGURE 2



The patterns in nontarget neighborhoods substantively declined and leveled off. The Northside patterns spiked in 2005, fell in 2006 and then spiked again to its highest level in 2007. This Hill District patterns substantively increased and the Southside remained relatively stable until both fell in 2007.

Impact Relative to the Propensity-Based Comparison

Although the One Vision initiative was implemented during a time of increasing violence, its effect is best assessed by comparing changes in crime in the target neighborhoods with those in the comparison areas. Such analysis must control statistically for other variables that could

FIGURE 3

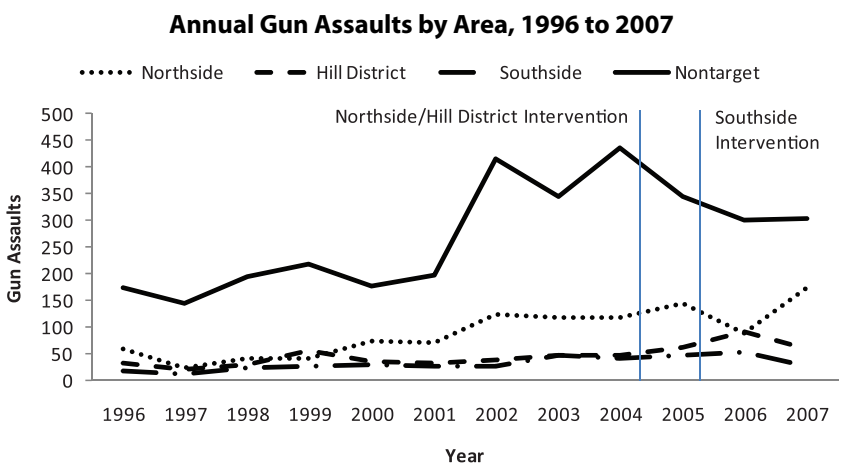


TABLE 2

Test of One Vision Intervention Effects, Propensity Score–Weighted Counterfactual Neighborhoods

Outcome	Predicted Monthly Rate Change (per 100,000 residents)	*p* Value
Northside		
Homicide	0.0219	0.7432
Aggravated assault	25.2095	0.0000
Gun assault	9.2824	0.0000
Hill District		
Homicide	–0.6710	0.3374
Aggravated assault	7.7365	0.0255
Gun assault	5.2893	0.0012
Southside		
Homicide	–0.2540	0.6976
Aggravated assault	25.3953	0.0000
Gun assault	4.9865	0.0015

explain changes in violence, including the time period of observation and neighborhood conditions.

Table 2 summarizes the outcomes of the models used to assess the impact of One Vision on violence in all the target neighborhoods compared with nontarget neighborhoods.[8]

8. As noted in the Outcome Models section, these models control for seven neighborhood characteristics. To preserve space, we do not provide the full results of the outcome models. However, they are all available upon request.

TABLE 3

Test of One Vision Intervention Effects Relative to Comparison Neighborhoods Suggested by One Vision Staff

Outcome	Predicted Monthly Rate Change (per 100,000 residents)	*p* Value
Northside		
Homicide	0.2845	0.7588
Aggravated assault	26.7970	0.0000
Gun assault	14.6100	0.0000
Hill District		
Homicide	–0.9174	0.2681
Aggravated assault	6.4579	0.1922
Gun assault	9.4336	0.0016
Southside		
Homicide	–0.6288	0.7438
Aggravated assault	25.0327	0.0000
Gun assault	4.8154	0.0057

We weighted the nontarget neighborhoods by propensity scores on how closely they matched the target neighborhoods. As noted previously, the sample sizes, calculated by multiplying the number of neighborhoods in the particular analysis by the number of months for which we had data, varied from 8,316 to 10,512. The results show that One Vision was not associated with any change in homicide rates relative to all Pittsburgh neighborhoods not served by One Vision. They show aggravated assault and gun assault rates increased in the target neighborhoods relative to the comparison neighborhoods after program implementation. The table presents effects in predicted change in a monthly rate of occurrence per 100,000 residents. These data suggest that the rates of aggravated assault increased similar amounts in the Northside and Southside (approximately 25 per month) but by a smaller rate in the Hill District (approximately 8 per month). Gun assault monthly rates increased more in the Northside (approximately 9 per month) than in the Southside and the Hill District (approximately 5 per month).

Impact Relative to the One Vision-Suggested Comparison

As a second way to assess the impact of One Vision, we examined changes in violence in the target neighborhoods compared with neighborhoods that One Vision staff suggested were most similar based on their intimate familiarity with the neighborhoods. Table 3 highlights these results, again controlling for the time period and differing neighborhood characteristics. The number of observations for each of these models ranged from 3,036 to 5,040. With one exception, the assessment of the program's impact on violence was essentially the same as shown previously. This analysis showed that One Vision did not

TABLE 4

Test of Spillover Effects, Propensity Score–Weighted Counterfactual Neighborhoods

Outcome	Predicted Monthly Rate Change (per 100,000 residents)	*p* Value
Hill District		
Homicide	−0.5546	0.6483
Aggravated assault	−14.2040	0.0379
Gun assault	6.1647	0.0979
Southside		
Homicide	−0.8695	0.8012
Aggravated assault	28.7132	0.0000
Gun assault	5.5715	0.0072

have an effect on homicide rates. It showed One Vision was associated with increases in the monthly rate of aggravated assaults in the Northside (approximately 27) and Southside (approximately 25) areas but was statistically unrelated to changes in the rate of aggravated assaults in the Hill District. This comparison with areas suggested by One Vision staff also showed increased gun assault rates in the target neighborhoods areas relative to those not targeted.

The Impact of One Vision on Violence in Adjacent Neighborhoods

To account for potential spillover effects of One Vision's implementation, either displaced-violence or violence-suppression benefits, we used impact analyses to assess change in violence in the neighborhoods adjacent to the Hill District and Southside relative to all the remaining nontarget neighborhoods in the city (matched to the spillover neighborhoods by propensity scores). As Table 4 shows, the models detected no spillover effects for homicide as a result of One Vision's implementation (the sample sizes of the models varied from 6,486 to 7,050). By contrast, the table shows One Vision was associated with spillover effects in aggravated and gun assaults. After One Vision was introduced, neighborhoods adjacent to the Hill District saw a reduction in aggravated assaults but no statistically significant change in gun assaults relative to other comparison neighborhoods. The neighborhoods surrounding Southside experienced increases in both aggravated and gun assaults. The suppression benefit to the neighborhoods contiguous to the Hill District was approximately 14 aggravated assaults per 100,000 residents per month. The increased rate of this offense in the neighborhoods next to Southside was nearly 29 per 100,000 residents. The detrimental spillover effect of One Vision on gun assault rates per month was approximately 6 incidents per 100,000 residents in the neighborhoods adjacent to the Southside.

Discussion

The Overall Impact of One Vision

Using two forms of comparison, each of which controlled for neighborhood attributes, seasonal effects, and trends over time, we found no quantitative evidence One Vision helped reduce violence. We found no effect of the program on homicide rates. We did find that the onset of One Vision efforts was associated with increases in aggravated assaults and gun assaults in all three target areas (excepting the comparison of aggravated assaults in the Hill District and the comparison area suggested by One Vision staff).

Our spillover analyses also indicated that the introduction of One Vision was associated with no change in homicide rates. We did find introduction of One Vision associated with an increase in aggravated assaults in the Southside spillover neighborhoods and a decrease in such assaults in the Hill District spillover neighborhoods. We found that the program was associated with increases in gun assaults in the Southside spillover neighborhoods but had no effect in the Hill District.

It is a challenge to explain why a program did not produce any effect, but it is an even greater test to discuss why a program had a negative effect. Before attempting to explain the negative effects, we contrast features of the Pittsburgh program with similar ones in Chicago (Skogan et al., 2008) and Baltimore (Webster et al., 2009). We also contrast the One Vision street-worker program with the original Boston Gun Project that involved street workers as part of a broader violence-reduction strategy. Considering the different findings, we think such comparisons are critical to help policy makers think through the implications of adopting such strategies and identify key implementation strategies. We also think it helps set up the need for additional research.

Comparing One Vision with Other Initiatives

CeaseFire Chicago. As noted previously, the individuals involved in the creation of One Vision were influenced significantly by a program administered by the Chicago Project for Violence Prevention called CeaseFire Chicago (Skogan et al., 2008). The design of CeaseFire Chicago reflected research documenting the success of various public health strategies. The goals of this program include disrupting the cycle of violence and changing attitudes and norms about specific behaviors. The program invested considerable resources in communicating, particularly to high-risk individuals, the costs of being involved in violence; in connecting individuals to services that might provide an alternative to violence; and in directly confronting individuals (usually gang members) who might resort to violence to resolve a conflict. CeaseFire used various community mobilization, education, and mentoring strategies to communicate the dangers of violence. A critical aspect of the program provided "on-the-spot" alternatives to violence and intervention before a conflict escalated in violence. The program also sought to influence perceptions about the risks and costs of involvement in violence.

Several key individuals and groups were critical to implementation of the Chicago program. First, the program employed outreach workers in each targeted community. Each outreach worker had a caseload of approximately 15 clients identified and assessed as being in need. These workers lived in or knew the neighborhoods where they worked and thus had street credibility and a good sense of individuals who were in need. Outreach workers worked the streets by talking with individuals, identifying clients, and then counseling and connecting these clients to needed services. It seems that working with clients was their primary task, but they also were expected to distribute information about the program and its "stop the violence" message to groups and individuals. Outreach workers mediated conflicts as well. Skogan et al. (2008) concluded that the outreach workers succeeded at identifying and working with high-risk clients. In fact, interviews with the clients indicated that, "after their parents, their outreach worker was typically rated the most important adult in their lives" (Skogan et al., 2008: 8–10). Nevertheless, it is difficult to assess the comparable levels of risk clients had in Chicago, Pittsburgh, and other cities with similar programs.

Second, the program employed another group of street-savvy individuals that focused specifically on conflict mediation. These individuals, who are called violence interrupters, were former gang members, who had street credibility because of their past. They were expected to use their understanding of the individuals and groups living in a neighborhood to prevent violence. The violence interrupters identified brewing conflicts or reacted to shootings that occurred and would gather intelligence about these conflicts and then attempt to mediate nonviolent solutions. They talked with gang members, as well as friends and families of gang members and shooting victims, focusing "on affecting risky activities by a small number of carefully selected members of the community, those with a high chance of either 'being shot or being a shooter' in the immediate future" (Skogan et al., 2008: ES-1). A significant amount of their time focused on responding to retaliatory shootings. Skogan et al. (2008: 8–11) estimated that "40 percent of intervener's mediation efforts concerned potential shootings that would have been in retaliation for an earlier imbroglio."

Third, other key contributors to CeaseFire Chicago included community members, social service organizations, and clergy. The program attempted to build and enhance community partnerships. These partnerships were valuable for many reasons, including the access to jobs and services they offered to clients and the legitimacy partners gave the program and its antiviolence message.

Fourth, police and prosecutors were frequent collaborators with CeaseFire Chicago staff. The role of police and criminal justice partners in changing the perceived risk of illegal gun carrying was a formal part of the Chicago CeaseFire logic model. Additionally, police shared information with the program after an incident so that staff could calculate a response. Police also collaborated with them for marches and vigils, walking with program staff and assisting with traffic and crowd control.

Baltimore Safe Streets Program. The Baltimore Safe Streets program was implemented in three high-crime neighborhoods—McElderry Park (East Baltimore), Union Square

(South Baltimore), and Ellwood Park (East Baltimore)—in mid-2007 and early 2008. Safe Streets, like the programs in Chicago and Pittsburgh, attempted to decrease violence by communicating to residents and high-risk individuals the impacts of violence on their communities; reaching out to persons in need, especially high-risk youth; and identifying and then intervening in potentially violent conflicts.

The interim evaluation focuses on the first 14 months of program implementation. It discusses implementation of the program and its effects on attitudes toward gun violence as well as on the number of homicides and shootings. Two different community groups implemented the program model in Baltimore. The implementation in Union Square was abbreviated because problems caused the program to cease after 5 months. Each implementing group was to collect data on the ratio of outreach workers to clients and the number of face-to-face contacts with clients, referrals for services, mediations of disputes, flyers distributed, and violence responses initiated.

The results indicate that the number of clients and face-to-face contacts increased as expected after the implementation of the program. Outreach workers made 450 face-to-face contacts in McElderry Park and just fewer than 100 contacts in Ellwood Park in August 2008. Outreach workers also made a large number of referrals to various services, an average of 26 per month. Most referrals were for employment issues. There was "considerable month-to-month variation" in the number of conflicts mediated (Webster et al., 2009: 6). Between August 2007 and August 2008 in one target area, the number of mediations ranged from six to eight in some months to less than two in others. There were no statistically significant changes in Ellwood Park, but the authors found that there was not a single homicide in the McElderry Park neighborhoods for at least 17 consecutive months (p. 14), but nonfatal shootings decreased less here than in the comparison areas.

However, it also is important to note that although these researchers note that there were problems with implementation in the Union Square neighborhood that resulted in the program being eliminated, the 5 months of activity here was associated with an increase in homicides and shootings in the targeted neighborhoods relative to the comparison areas. The authors discuss the problems with the implementation but unfortunately do not attempt to explain why there might have been increases in shootings or homicides and if the program might have contributed to the increases.

One Vision Versus Chicago and Baltimore. Although the amount of information on the Baltimore program is somewhat limited given that it has only an interim evaluation, several noteworthy differences exist between One Vision on the one hand and the Chicago and Baltimore programs on the other that highlight the difficulties in evaluating programs that are on paper very similar but in practice are quite different. First, although it is difficult to detect dosage of such programs, the organization documents we reviewed for the implementation assessment point to some limitations in the administration of the program model in Pittsburgh.

Specifically, it does not seem that One Vision used the documentation of activities in any systematic way to select actions for the targeted neighborhoods or to monitor the performance of the community coordinators. In contrast, the Chicago program in particular seemed to rely on the information of these documents as an accountability mechanism. The Chicago Project for Violence Prevention (CPVP) essentially supported local organizations to administer the model and then monitored the activities and coordinated with these local programs. Completed forms were a key source of accountability in Chicago. Moreover, Skogan et al. (2008: 2–25) reported:

> During the evaluation period we saw a tightening of policies and procedures on the part of CPVP that reflected the adoption of a more centralized management role. CPVP took a more active role in regulating program activities and reviewing site records. CPVP staff made an increasing number of site visits to ensure better program implementation, and new central office positions were created to handle program implementation and documentation issues. Sites were held more accountable to meeting standards regarding shooting responses, client caseload size, and other program activities. CPVP also became more assertive about the hours that sites were to be open, to parallel the hours when violent crime actually occurs.

Second, both the observations and the organizational documents to some extent reveal that the street workers were involved in a variety of important activities and worked to help people in dire need. Nevertheless, the clients with whom the Pittsburgh community coordinators worked and the types of conflicts mediated seemed to be different from those in Chicago or Baltimore. Specifically, Baltimore and Chicago workers focused almost exclusively on the activities of and conflicts between high-risk violent individuals. Indeed, in Chicago, the clients of CeaseFire workers had extensive criminal histories, which were consistent with those most at risk for being involved in homicides as both victims and offenders (Skogan et al., 2008: ES-10). Similarly, in Baltimore:

> [O]utreach workers logged hundreds of contacts with these high-risk individuals during which they encouraged alternatives to violence, mediated conflicts, provided informal mentoring, and made referrals for services that could decrease risks. The outreach workers interfaced with dangerous gangs with access to guns that operated under circumstances where the odds of lethal altercations are alarmingly high. (Webster et al., 2009: 14)

In McElderry Park, a site that did not have any homicides during the evaluation period, outreach workers intervened in 53 high-stakes disputes and altercations. In Chicago, violence interrupters estimated that 40% of the conflicts mediated could have resulted in retaliation shootings (Skogan et al., 2008). In contrast, few of the conflicts mediated in

Pittsburgh were specifically directed at retaliations. In our field research, we found that One Vision staff, especially the executive staff, attempted to assist shooting victims and discourage retaliations, but that the street workers were not working potential violent conflicts. Data indicated that only 1.8% of the conflict mediations were in response to a potential retaliatory event. We found that street workers did interact with gang members, but they did not necessarily intervene in gang conflicts. Many simply focused on protecting specific gang-affiliated individuals. The implementation data also indicated that the street workers responded to conflicts unsystematically. They typically mediated a conflict when coming into contact with involved individuals in the regular course of their day. They rarely focused on systematically identifying key violence threats and developing responses to them.

Third, Pittsburgh street workers had a variety of responsibilities that made it difficult to manage their workload. They were the heart and soul for program implementation, expected to intervene and mediate conflicts, assist clients, attend violence responses, and participate in community programming. Each of these tasks required different skills and training. As a result, many street workers might have emphasized what they enjoyed doing and those things at which they were most effective and ignored other responsibilities. The Chicago model, in which outreach workers focus primarily on working with clients and mentoring individuals and violence interrupters focus on responding to gang conflicts and responding to shootings, has much more potential for allowing workers to specialize and perhaps become more effective with specific tasks.

Fourth, one difficult challenge of quasi-experiments is the inability to control for other variables that might have contributed to program outcome. Communities with high rates of violent crime might have multiple simultaneous programs and strategies. In the McElderry Park area of Baltimore, there were other law-enforcement initiatives, including "close monitoring of individuals with histories of gun offending, increased police presence in areas with the highest numbers of shootings, and efforts to suppress illegal gun possession and sales" (Webster et al., 2009: 15). Similarly, there were several other initiatives, such as PSN, occurring in Chicago at the same time as CeaseFire.

One Vision Versus Comprehensive "Pulling Levers" Programs

In the original Boston Gun Project, street workers were part of a broader antiviolence strategy that was driven by a multiagency criminal justice task force. The overall mission, reducing homicide and gun violence, was consistent with One Vision, but the tactics included a comprehensive effort to change the perceived risk of groups of chronic offenders from both violent victimization and incarceration. Like those in One Vision, street workers sought to convince at-risk individuals not to carry guns and to avoid conflict and retaliation. Unlike those in One Vision, Boston street workers were backed by direct communication from police, district attorneys, and federal prosecutors on the consequences for illegal gun possession and use. This strategy, which is known as *pulling levers*, had a significant impact on homicide and gun violence in Boston (Braga et al., 2001; NIJ, 2001; but also see Berk,

2005; Rosenfeld, Fornango, and Baumer, 2005a, 2005b; Weisberg, 2005) and in cities that have attempted to replicate the Boston model. These include Indianapolis, Indiana; Lowell, Massachusetts; Stockton, California; and Los Angeles, California (Braga, 2008; McDevitt et al., 2007; McGarrell et al., 2006; Tita et al., 2003). They also include Chicago, where the pulling-levers strategy was a key aspect of a PSN program, which led to a 37% reduction in homicide (Papachristos, Meares, and Fagan, 2007).

In evaluations of complex interventions, it is difficult to identify what elements are critical to success or failure. Like the One Vision strategy, pulling levers as implemented in Boston and other locations consisted of many different elements, making it difficult to identify the elements that produced changes in violent offending behaviors. There were multiple parts of the strategy, including a communication campaign, the work of ministers, home visits, and other police strategies.

Both Chicago and Baltimore had active PSN programs at the time of their street-worker programs. These included efforts to communicate a message aimed at felons against carrying firearms and to increase the federal prosecution of felons possessing or using firearms. Although there was also a PSN program in Pittsburgh, there is no evidence of coordination among the police, the PSN task force, and One Vision. We do not have evidence of such coordination in Baltimore or Chicago. Nevertheless, the fact that Chicago CeaseFire was occurring during a time when Chicago's PSN initiative was holding face-to-face offender notification meetings with high-risk individuals, albeit in targeted neighborhoods, might indicate that the street-worker intervention is more powerful when supported by the credible threat of prosecution for illegal gun carrying and use. It is interesting to note that PSN offender-notification meetings were also occurring in Baltimore during its street-worker program, although we do not have evidence of coordination between PSN and the program.

Assessing the Negative Effects

The problems in implementation in Pittsburgh and some of the differences between One Vision and the strategies in Chicago and Baltimore might help explain the null effects we found in regard to homicide but would not be consistent with the data that showed increases in aggravated and gun assaults. An important question to consider is whether it is plausible that the program contributed to these increases. Importantly, the increases that were uncovered in Pittsburgh are not completely in contradiction to the findings in Baltimore. That is, researchers in Baltimore found significant increases in homicides and shootings in one of the target neighborhoods, but they did not explain why this might have occurred in the Union Square neighborhood. The potential that a program such as what was examined here contributed to increases in violence is a serious concern and worth exploring, and the variation in results that were found when comparing Pittsburgh to Chicago and within Baltimore point to a critical need for more research on this topic. Subsequently, we provide some thoughts about what might be occurring that account for these differences.

Malcolm Klein's research is important here. Klein (1971, 1995) discussed many critical issues that help us better understand street gangs, but his ideas on the "centrality of cohesiveness" were particularly important to thinking through reasons for increased levels of violence after an intervention. In an effort to understand a program that was introduced in Los Angeles in the 1960s called the Group Guidance Project, Klein evaluated closely the activities of gang workers. Similar to what the community coordinators were asked to do in Pittsburgh, these gang workers tried to assist gang members by organizing group activities, assisting them with building skills, and advocating for them as they interacted with criminal justice and social service bureaucracies. What he discovered was that the introduction of these street workers actually increased delinquency and isolated the youth from the community. Importantly, he found when two of the most aggressive, antipolice gang workers took on different responsibilities, the gangs they had been working with essentially disintegrated. Klein wrote (1995: 45), "The original two workers had inadvertently become the focus of the gangs' cohesion. Their active group programming, their antipolice attitudes, their total commitment to the groups had become even stronger glue than the members' original need to come together for identity, status, and belonging." He found also that in the areas where the gang workers were retained, cohesiveness continued to increase.

In Pittsburgh, like most other cities, gangs are generally not very cohesive entities, and thus street workers might increase cohesiveness and, therefore, levels of violence between and among them. It is thus possible that we might be observing a "paradox of programming" (see Klein, 1971)—the presence of outreach workers increased the cohesion of gangs, making some groups more organized, in turn leading to increased violence. Comparisons of programs like those implemented in Pittsburgh, Chicago, and Baltimore might vary the nature and type of gang structures that exist in a particular city or even with neighborhoods within that city. For example, the gang networks in Chicago and Baltimore might be very stable, making it straightforward to identify and mediate conflicts. There are many important studies that explore the evolving gang structure in Chicago, which might best be described as a "chronic gang city" (Tita and Ridgeway, 2007: 233; see also Venkatesh, 1997). The gang structure in Pittsburgh, by contrast, consists of loose conglomerations of groups and would be better described as an emerging gang city (Tita and Ridgeway, 2007). It is possible that the intervention might have brought more people into the gangs by providing opportunities for more individuals to be exposed to gangs, and more clearly defining conflicts as group-based threats.

The process evaluation related to this project revealed several important things about the nature of the "street work" done by the community workers. First, the community coordinators we observed were working to provide activities for the youth in the targeted neighborhoods. They focused on creating activities (e.g., summer basketball leagues, cookouts, etc.) and client-centered outreach. Such activities were deemed to be important to providing alternatives to youths at risk, but they might serve as central meeting places that resulted in the development and enhancement of social networks. Second, although

an analysis of the organizational documents revealed that the street workers had contact with gang members, the results showed that their contacts with known gang members was actually lower than expected. Approximately 41% of the conflicts that were targeted by street workers involved gang activities. This means that they were working with many youth who were not known gang members but were bringing them into programming that was designed to assist gang members and provided opportunities to make connections with others. Third, one of the critical ways that Pittsburgh was different from Chicago was in the involvement of the police. In Chicago CeaseFire, the police and related criminal justice partners were an explicit component of the logic model. Specifically, the police and criminal justice system were considered key components in changing the perceived risk and costs for illegal gun possession and use (Skogan et al., 2008). Pittsburgh police were certainly not absent in the targeted neighborhoods during the study period. Nevertheless, there was not the type of coordination between One Vision and the police that Chicago enjoyed. In addition, our ethnographer observed outright hostility between the community coordinators and police in Pittsburgh. Although there seemed to be a good relationship between the executive staff of One Vision and the police, this did not translate to what was occurring on the street. Thus, as community coordinators were in contact with youth and active gang members, they might have pushed them toward being isolated even more from the community as Klein (1971, 1995) observed in Los Angeles.

Our results are tentative as there are other plausible explanations for the increase. For example, preceding conditions in Pittsburgh might have contributed to the effects we saw in some of the violent-crime measures. Other unique social conditions also might have contributed to these results. Violence was increasing prior to the implementation of the program in the targeted neighborhoods. It is possible that One Vision simply did not work as intended (to reduce crime and violence), and the target neighborhoods did in fact realize a marked increase, whereas the comparison neighborhoods did not (or at least less of an increase). The analysis might then suggest what we found, although there were some other neighborhoods that One Vision staff thought would be problematic as well, and they were comparison neighborhoods. But one has to be concerned about the iatrogenic effects we discovered. What is interesting is that in Baltimore there was also variation in effects by neighborhood, and there seemed to be some effects in one of the targeted neighborhoods, no effect in the other, and negative effects in a third area. Chicago, however, produced positive effects in all neighborhoods evaluated: Shootings and gang-related homicides decreased in all of the targeted neighborhoods. Although we find no effects on homicide, we do find that aggravated and gun assaults went up in the targeted neighborhoods and in part also increased in the spillover areas. An important question for future research is why such different results? These contradictory results call out for a need for additional research especially in the area of understanding issues related to cohesiveness and change after the implementation of such programs.

The logic behind using street-savvy individuals to respond to and manage potentially violent conflicts—from identifying to understanding to searching for solutions to them—may be appealing as individuals can use their street credibility to a positive way and help organizations, including the police, monitor brewing conflicts and beefs more effectively. Because the escalation of a street conflict or "beef" to an act of violence may take some time, there is an opportunity for prevention with better intelligence. It is clear that being close to the street and "in the game" is required to obtain good intelligence, but being so forces the street worker to walk a fine line. How much standing should one have? Too little and the worker might be ineffective and in danger. Too much and the worker might become corrupt. This points to the importance of talk and street gossip and its impact on conflicts and ultimately violence—a topic that has received very little attention (but see Lauger, 2010). It is important to think through how a street worker, when even attempting to do right in responding to and addressing beefs, might cause what is a complete but inadvertent effect. The workers used in Pittsburgh, as in other places, were chosen because they had street credibility and in theory could use their status as a starting point in communicating with rival gang members and others in a neighborhood. Their position as a worker might even legitimize their status and their street credibility. They seem to have connections and thus can help individuals in many different ways. If they are confronted with a conflict and chose to intervene, it is plausible that the connections that they have and the communications they make could deescalate the beef. However, it is equally plausible, unless the gang worker is adequately trained and prepared, that their communications might escalate the conflict. The gang worker becomes a critical node of communication across gangs, and what they say, how they say it, and what they ignore all can impact how rival gang members react. The result could certainly increase the hostility and violence between groups.

Policy Implications

Innovative programs are critical to addressing the major issues facing our most disadvantaged cities. Anderson's (1999) important work describing the code of the streets demonstrates how individuals living in these neighborhoods are affected by their environment. The people who live in these neighborhoods adapt to their environment in different ways. The code of the streets becomes a guide to living their lives (see Stewart and Simons, 2009)—adopting a lifestyle that, for many, includes violent criminal activities. There have been many attempts to inject programs into these communities. One Vision represents one of the strategies implemented in Pittsburgh to address concerns about violence. Yet, our evaluation found the onset of the program to be associated with increases in violence in these Pittsburgh neighborhoods. In this section, we discuss several issues related to the limitations of implementing such programs.

First, the implementation of the One Vision program deviated in several ways from ideal implementation. One Vision lacked consistent documentation; the completion of

documentation was sporadic and varied by areas. One Vision staff seemed to rarely use the documentation in any systematic way to guide program actions. Street workers focused more on persons in need than on those at risk. This contributed to street workers having a broad variety of tasks and workloads that were difficult to manage. Finally, program actions were neither as frequent nor as focused on gangs and drugs as had been expected. In particular, it does not seem that One Vision routinely focused on the most serious offenders and highest risk individuals.

Second, the program did not intervene with the group or gang structure generating violence. It seems that Chicago CeaseFire, likely reflecting the prevalence of gangs in Chicago, focused on gangs explicitly. The original Boston Gun Project and the successive programs in Indianapolis, Lowell, High Point, and Stockton included a group accountability component. Gangs, cliques, or groups of chronic offenders were told that they would be held accountable for the continued violence of any of their members. As evident in other programs, this form of intervention calls for a greater law-enforcement component.

A related but alternative explanation is that the gang structure in Pittsburgh might require a different approach. Gangs in other cities where similar initiatives apparently have succeeded have more stable and persistent structures than are evident in Pittsburgh. Pittsburgh gangs seem to be far less cohesive, perhaps making it more difficult to identify and mediate conflicts among them. Outside Los Angeles and Chicago, such a fluid gang structure seems to be the norm (NIJ, 2002; Weisel, 2002).

Among the key components of the Chicago and Baltimore programs are the following:

1. Change the norms about the acceptability of violence
2. Increase the perceived costs of involvement in behaviors associated with violence (e.g., illegal gun carrying)
3. Increase the perceived legitimacy and fairness of antiviolence interventions
4. Hold groups of offenders accountable for continued violence
5. Increase linkages to a variety of social supports and legitimate opportunities ("widen decision alternatives"; Skogan et al., 2008: ES-2)

The questions raised about One Vision relate to target populations, dosage, and comprehensiveness. One Vision emphasized the first, third, and fifth components listed previously. It is not clear whether its work with the highest risk groups was intense enough to help reduce overall violence. That is, although One Vision might have had some success in working with individuals in the target areas, these successes might not have been on a scale sufficient to change the levels of violence as measured in this evaluation. One Vision did not partner with local police and prosecutors to communicate a consistent and credible deterrent message that might have changed the perceived risk associated with illegal gun carrying and use, nor did it explicitly focus on influencing social networks of at-risk individuals. The lack of a systematic and integrated law-enforcement component to complement One Vision's

activities might, in part, explain its inability to demonstrate a measurable reduction in violence.

One also cannot help but to wonder to what extent community conditions matter in the selection of target areas. The three chosen and studied in this evaluation were thought by community leaders to have significant violence problems and attributes conducive to the activities One Vision sought to implement. However, our examination of the violence data suggested, for example, that the frequency of homicide in the Southside was substantially less than in the Northside and the Hill District. Although it raises the question as to whether One Vision could have had a measurable impact on violence in the Southside because of its amount of observed violence, the answer is obscured given that we did not detect violence reductions in the target neighborhoods with more per capita violence.

One Vision was established to address the serious problem of lethal violence in particular neighborhoods of Pittsburgh. The program leaders looked to CeaseFire Chicago to follow a "promising practice" model for implementation in Pittsburgh. The program staff was trained in the CeaseFire approach. The finding that One Vision did not have an impact on violence in the target neighborhoods raises several critical issues for a field attempting to move toward evidence-based practices. Are the CeaseFire Chicago results stable over time? Are they transferable to other communities that differ from Chicago in gang structure or parallel systems (such as community policing) coordinating with CeaseFire? If the results are stable and not unique to Chicago, then what was missing in Pittsburgh?

We speculated on some of the potential differences in the One Vision program; yet these are post hoc observations. The results from the Baltimore evaluation will be important in addressing these questions. The results from Chicago and some of the results from Baltimore suggest the promise of street-worker programs. The results from Pittsburgh and one of the target neighborhoods in Baltimore suggest the need for continued rigorous evaluation. Taken together, there seems to be enough promise for continued programmatic experimentation but also enough questions that future programs should be coupled with continued evaluation. One critical area to study is the impact of such programs on cohesiveness and thorough analysis of the changing social networks after the implementation of such programs is warranted. This research is needed to assess the efficacy of this type of program in reducing community violence as well as to identify program components associated with violence reduction.

Study Limitations

All studies have limitations that should be considered in interpreting their findings. Evaluations of the sort used in this article face difficulties in identifying best comparison areas, measuring program delivery and performance, and isolating program effects from other effects. True random designs are generally not possible for such social programs. Quasi-experimental designs can approach the rigor of random selection and experimental

analysis. Nevertheless, they cannot control for some variables, such as other ongoing initiatives or community changes that might contribute to program outcome. It is possible that the rise in violence we observed was caused by some other change in the target communities that we could not identify and separate from the assessment of program effects.

Similar to design challenges, there are several measurement limitations. First, as noted previously, One Vision's main focus has been on reducing homicide and shootings in its target areas. Although we had data on homicides, changes in the Pittsburgh Bureau of Police's reporting policies precluded us from gathering and assessing longitudinal shooting data. As a consequence, we analyzed the broader categories of aggravated assaults and aggravated assaults with a gun. Although it is possible that these measures could detect changes in shootings, they include other forms of violence whose changing levels might mask program effects on shootings. Second, our data did not permit us to assess gang and group violence and how One Vision's efforts have affected it. Third, homicide is a rare occurrence. Detecting measurable changes in variables with low frequency and variation is generally difficult. Further distinguishing these offenses to examine only those that are gang or group related would make an analysis even more problematic. Finally, our control measures are not as precise as we would like. Necessarily, we drew on U.S. Census Bureau data for socioeconomic and demographic data of the neighborhoods in our analysis. These data illustrate variation among the neighborhoods in 2000 but do not identify changes in them since then.

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