BETTER GUN ENFORCEMENT, LESS CRIME*

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Research Summary:
Project Safe Neighborhoods (PSN), which for the past several years has been the major federal initiative to combat gun violence, includes several elements (such as gun locks and other efforts to reduce gun availability) that research suggests are likely to have at best modest effects on gun crime. In general, enforcement activities targeted at the "demand side" of the underground gun market currently enjoy stronger empirical support. However much of PSN's budget has been devoted to increasing the severity of punishment, such as by federalizing gun cases, which seems to be less effective than targeted street-level enforcement designed to increase the probability of punishment for gun carrying or use in crime.

Policy Implications:
PSN and other enforcement activities could be made more effective by redirecting resources toward activities such as targeted patrols against illegal gun carrying. Given the substantial social costs of gun violence, an efficiency argument can also be made for increasing funding beyond previous levels.

KEYWORDS: Gun Violence, Law Enforcement, Project Safe Neighborhoods

INTRODUCTION

What, if anything, can be done about America's problem with gun violence? Despite a dramatic decline in gun violence during the 1990s, nearly 30,000 Americans lost their lives to gunfire in 2001. Of particular concern is gun crime, which is the focus of this article. Homicides account for less

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than two fifths of all gun fatalities; nearly three fifths are suicides, whereas accidents account for only a small share of gun deaths.[1] Yet concern about gun crime is widespread, it affects the way many people live their lives, and it accounts for perhaps 80% of the approximately $100 billion in social costs that gun violence imposes on American society each year (Cook and Ludwig, 2000).[2] Skepticism that much can be done to combat this problem is fueled in part by the prevalence of gun ownership in America—35% of households own a total of more than 200 million guns, of which at least 65 million are handguns (Cook and Ludwig, 1996).

Most political debate has focused on efforts to regulate the supply side of the gun market to restrict access to high-risk people such as youth or convicted felons, which is what most people mean by "gun control." The consequences of such regulations are difficult to predict on the basis of social science theorizing alone. Widespread gun ownership can help protect against or deter crime (Cook, 1991; Kleck and Gertz, 1995; Lott, 2000; Ludwig, 2000), but it may also increase gun availability to high-risk people prohibited from legally owning guns (Cook and Ludwig, 2003, 2004a, 2004b). Although identifying the net relationship between gun prevalence and crime is fraught with difficulties, the best available evidence to date suggests that the latter effect may dominate: on net more guns in private hands seem to produce more homicides (Cook and Ludwig, 2005; Duggan, 2001).[3]

1. Unintentional injuries were 5% of gun deaths in 2001. Available online: http://www.cdc.gov/ncipc/wisqars/, Accessed 10/4/04.

2. The $100 billion figure is an estimate for the social costs of gun *misuse* and so ignores the benefits to society from widespread gun ownership, in the same way that studies of the social costs of automobile accidents ignore the benefits from driving. The thought exercise here is to estimate the value to society from eliminating gun misuse without affecting the benefits from legal gun ownership. The cost estimate comes from contingent valuation (CV) survey responses to what people would pay to achieve a 30% reduction in assault-related gunshot injuries. These CV questions should in principle capture the full social costs of crime-related gunshot injuries, although in practice there remains some debate about the reliability of the CV measurement technology (Cook and Ludwig, 2000). The estimate also assumes that societal willingness to pay is linear with the proportion of gun violence eliminated, and so it ignores complications from the possibility of diminishing or increasing marginal returns.

3. A different conclusion is reached by Lott (2000), who essentially relies on cross-sectional variation across states in gun ownership. In contrast, Duggan (2001) and Cook and Ludwig (2005) use across-state, over-time variation that can help parse out the confounding effects of unmeasured state "fixed effects" that influence both gun prevalence and crime rates. More importantly, Lott's proxy measure for state gun ownership rates (from voter exit polls) suggests that personal gun ownership rates increased by about 50% from 1988 to 1996 (Lott 2000, p. 37). In contrast, the survey deemed most reliable on this topic, NORC's General Social Survey (GSS), suggests rates were stable over this period (Kleck, 1997, p. 98-99). Moody and Marvell (2005) use a panel setup with state-level survey data from the GSS on gun or handgun ownership rates and

Of course even a positive association between gun ownership and homicides, if true, would not necessarily imply that government efforts to regulate the supply side of the gun market will reduce crime. The net effect of such regulations depends on, among other things, the degree to which such efforts raise the price of guns to prohibited versus legal owners and the price responsiveness (elasticity) of both groups to price. To date little is known about the net impacts of such regulations, as argued by a recent blue-ribbon panel commissioned by the National Research Council (NRC): "Arguments for and against a market-based approach [to regulating access to guns] are now largely based on speculation, not on evidence from research. It is simply not known whether it is actually possible to shut down illegal pipelines of guns to criminals nor the costs of doing so" (Wellford et al., 2005, p. 8). The limited evidence available to date on the efficacy of supply-side regulations could be from problems with existing data sources and evaluation techniques, loopholes in the existing system of firearm regulations,[4] or the possibility that people at high risk for misusing guns are not very responsive to the time, money, or risk associated with obtaining a gun. Although more recent qualitative research raises the possibility that something about the regulatory environment may affect gun availability to high-risk people (Cook et al., 2005b), formal quantitative evaluations are at present not very informative on this point.

The good news is that targeted enforcement may be more effective, although current efforts are not as effective as they could be. This article reviews what is known about the effectiveness of current federal efforts at "gun enforcement," which are defined for our purposes as those activities that comprise what has been the federal government's major initiative in

impute missing values using percent suicides with guns, the proxy used by Cook and Ludwig (2005). Moody and Marvell do not find a statistically significant relationship between guns and murder, perhaps because the GSS is not intended to provide representative estimates at the state level and so provide a noisy proxy for gun ownership in a state-level panel data analysis. In this case, the point estimates for gun ownership rates will be attenuated toward zero. In addition to the problem of constructing a valid proxy for gun ownership at the county or state level, all of these studies suffer from not having a clear source of identifying variation in gun ownership rates across jurisdictions over time.

4. One possible explanation is that past modifications to the basic regulatory system established by the 1968 Gun Control Act are too limited to have much impact. Perhaps, most importantly, most laws governing gun sales apply only to those made by licensed firearm dealers (FFLs) and exempt the 30% to 40% of gun transfers made in the so-called "secondary market" (Cook and Ludwig, 1996; Cook et al., 1995). State efforts to regulate the secondary market, and even local efforts to ban handguns, may be undermined by across-state gun trafficking (Cook and Braga, 2001; Webster et al., 2001). Whether more restrictive gun laws at the *federal* level would help reduce gun crime is currently not known, but in any case, this type of national legislation seems unlikely for the foreseeable future.

this area, Project Safe Neighborhoods (PSN). Unfortunately the type and quality of evidence available on the effectiveness of different pieces of PSN varies widely across different elements of the program. As a result, my discussion necessarily involves some subjective judgments about what types of analogous evidence is relevant about the effectiveness of interventions that have not been (or cannot be) subject to a high-quality evaluation.

PSN received over $1.1 billion in funding during fiscal years 2001–2004, building on President Bush's pledge during the 2000 campaign to better enforce existing gun laws through programs like Richmond, Virginia's Project Exile.[5] Given that Project Exile involved federal prosecution of all eligible gun cases in Richmond, particularly for "felon in possession" cases, it is perhaps no surprise that PSN features a large dose of federal gun prosecutions. PSN also builds on the perceived success of Boston's Operation Ceasefire, in which collaborative problem solving by local and federal law enforcement resulted in focused efforts to disrupt gun trafficking and, perhaps more importantly, deter gangs from engaging in violence, particularly gun violence. A large share of the PSN budget has also been devoted to other activities such as school-based prevention programs to reduce youth demand for guns as well as supply-side activities such as gun locks and improving background checks for FFL gun sales. The federal budget's line item for PSN was eliminated by Congress in late 2004 (Lichtblau, 2004). Nevertheless, understanding what PSN has accomplished and how the program might be improved remains relevant, in part because many of the program's activities will continue with other sources of federal funding.

Some basic principles from the research literature provide guidance about how to make PSN and subsequent enforcement activities more effective. First, there is currently stronger evidence in support of interventions targeted at the "demand" rather than "supply" side of the gun market—that is, efforts to directly reduce gun misuse rather than availability. Second, for a given level of law enforcement spending, we may achieve a greater deterrent effect by increasing the certainty rather than the severity of punishment.[6] Third, gun crime is disproportionately committed by and against a small subset of the population (Cook et al., 2005a; Kates and

5. For example the 2000 Republican Party platform pledged to "vigorously enforce current gun laws.. . .Through programs like Project Exile, we will hold criminals individually accountable for their actions by strong enforcement of federal and state firearm laws."

6. We might expect a bigger deterrent effect from a change in the certainty rather than severity of punishment if only because people tend to discount the future (Cook, 1980).

Polsby, 2000; Kennedy et al., 1996), which suggests efficiency gains from targeting resources against this group.

Consistent with these principles, the main argument of this article is that enforcement resources could be made more effective by prioritizing demand-side enforcement activities, particularly targeted police patrols that seek to deter high-risk people from carrying guns illegally. The evidence for targeted patrols, although more promising than that available for most other interventions, is nonetheless subject to some lingering uncertainty. Moreover, the intervention raises the possibility of a difficult tradeoff in balancing the competing goals of reducing gun crime and preserving civil liberties. But the conclusion that targeted patrols against illegal gun carrying seem particularly promising reflects a growing consensus within the firearms research literature (Kleck, 1997, pp. 393–394; Wellford et al., 2005, pp. 234–235) and is consistent with a growing body of evidence on the effects of policing on crime more generally (see, for example, Levitt, 1997, 2002a; Skogan and Frydl, 2004).

The remainder of this article elaborates on the pragmatic question of how to improve PSN and gun enforcement more generally. I do not discuss Constitutional issues raised by PSN concerning federalization of law enforcement, primarily because I have no special expertise on this topic.[7] The next section describes PSN in more detail and notes that much of the program's funding has been devoted to activities that are unlikely to have much effect on gun crime, whereas an even larger share is spent on programs that could in principle have some modest effect but are unproven in practice.

Then we will discuss in more detail what the U.S. Department of Justice (DOJ) describes as the two "model firearms programs" for PSN: Richmond's Project Exile and Boston's Operation Ceasefire. Based on my evaluation with Steve Raphael (Raphael and Ludwig, 2003), I argue that we can be fairly certain that Exile does not produce the supernormal effects on gun crime that have been claimed. By "supernormal" I mean an effect that is much larger than what we could expect from investing in other proven law enforcement activities. Given that there is fairly good

7. Gene Healy of the Cato Institute views PSN as "an affront to the constitutional principle of federalism. The initiative flouts the Tenth Amendment by relying on federal statutes that have no genuine constitutional basis. Moreover, the program will very likely lead to over-enforcement of gun laws and open the door to prosecutorial mischief affecting the racial composition of juries. As the constitutional and policy implications of Project Safe Neighborhoods become more apparent, the Bush initiative looks less like a commonsense solution to crime and more like a political gimmick with pernicious unintended consequences" (Healy, 2002, p. 1). For a more general discussion of federal involvement with efforts to control street crime, see Heymann and Moore (1996).

evidence that additional spending on prisons and police reduces crime and on the margin may produce benefits in excess of costs, perhaps more so with police than imprisonment (Levitt, 1996, 1997, 2002a, 2002b, 2004), the opportunity costs of diverting scarce resources away from these activities provides a useful benchmark for assessing the impact of various PSN activities.[8] We cannot reject the notion that Exile yields "normal" returns similar to those from spending at the margin on police and prisons generally. The available evidence for Operation Ceasefire involves more uncertainty about whether the program had an effect on gun crime or yields supernormal returns.

I will argue that the most favorable risk-return combination seems to arise for targeted police patrols against illegal gun carrying, although such patrols have not featured prominently in PSN. One drawback is that the stop-and-frisk activity associated with such patrols are not as narrowly targeted as the enforcement activities under Exile and Ceasefire and as a result impose greater costs on the citizenry at large. These costs might be minimized by using crime statistics to target patrols on the highest-risk places and times, providing intensive officer training, and even involving the community in program design and implementation. But inevitably policy makers will face some difficult normative questions about the tradeoff between public safety and the additional intrusions associated with targeted patrols. In any case, targeted patrols seem to offer the greatest chance of supernormal returns to criminal justice expenditures, and increased expenditures in this area would help offset the effects of reduced funding over the past several years for the federal COPS program (Donohue, 2004). A shift from longer prison terms for gun offenses (as with Exile) to stepped-up anti-gun policing is also consistent with more general evidence that at current spending levels, the returns to an extra dollar for police may exceed that from additional spending on prisons (Levitt, 2004, p. 179).

There may also be an efficiency argument for increased funding for even a reconfigured and more effective version of PSN. Given the substantial costs of gun violence to society, on the order of $1 million per injury (Cook and Ludwig, 2000; Ludwig and Cook, 2001), targeted patrol may produce benefits that are much larger than the program's costs. It raises the possibility that even if the entire PSN budget was reinstated and devoted to such patrols, PSN might still be underfunded at current levels if

8. The degree to which increased imprisonment reduces crime through deterrence or incapacitation effects and the degree to which the former may be driven more by changes in the certainty than by severity of punishment remains unclear. See Levitt (2002b) and Mustard (2003).

the goal is to invest up to where marginal benefits and costs are equal (Becker, 1968).

PSN

If the federal government had $1.1 billion to spend on reducing gun violence, how should the money be allocated? Table 1 outlines the Bush Administration's answer to this question.[9] In what follows, I discuss these expenditure categories in more detail and offer some thoughts about their potential effects on gun crime in American.

TABLE 1. PROJECT SAFE NEIGHBORHOODS FUNDING, FISCAL YEARS 2001–2004 (MILLIONS)

Activity	FY 2001	FY 2002	FY 2003	FY 2004	Total 01–04
Gun Locks		$50.0	$24.8	$4.9	$79.7
(Project Childsafe)					
Background-check data	$47.3	$38.0	$42.7	$32.6	$160.6
(National Criminal History Improvement Program)					
ATF/FBI	$86.5	$111.4	$120.2	$123.5	$441.6
Crime-gun tracing (YCGII)					
Ballistics imaging					
Training					
School-based prevention	$13.0	$13.0	$13.0	$13.0	$52.0
Gang Resistance Education and Training (GREAT)					
Prosecution and enforcement					
Federal prosecutors	$15.0	$32.0	$39.0	$40.0	$126.0
State and local prosecutors	$74.8				$74.8
Project Sentry and PSN grants		$69.8	$59.6	$44.5	$173.9
Total	$236.6	$314.2	$299.3	$258.5	$1,108.6

Source: Budget figures for each budget category other than GREAT were generously provided by Robyn Theimann and Scott Burau of the U.S. Department of Justice on 10/27/04. The GREAT budget figure is taken from http://www.psn.gov/About.asp?section=63, accessed 9/20/04. (Note that this budget table assumes that GREAT is funded at $13.0 million per year, and that this funding was folded into the overall ATF/FBI line item reported by Theimann and Bureau.)

One challenge is that the existing body of empirical research on "what works" to reduce gun crime is limited. Many policies or programs have not

9. I should note that the funding reported in Table 1 differs slightly from what has been reported on the DOJ's PSN website, which itself changes periodically over time. My thanks to Robyn Theimann and Scott Burau at DOJ for their help in carefully walking me through the intricacies of the PSN budget.

been the subject of even bad empirical research, much less anything like a high-quality evaluation. In other cases, existing research findings or social-science reasoning can rule out large (supernormal) program effects, but they cannot reject smaller impacts that are nevertheless large enough for the program to pass a benefit–cost test. In my discussion, I try to highlight the degree of uncertainty associated with my assessment of program impacts. Also uncertain in some cases is the degree to which a given intervention can be successfully replicated. These two dimensions together represent the uncertainty or risk associated with any possible use of PSN funding, against which we might compare the expected benefits. As noted, one relevant benchmark for the magnitude of such benefits is what could be achieved by simply allocating funds to ongoing criminal justice activities, such as more police or prison capacity (see Levitt, 2004).

The $130 million devoted to gun locks and middle-school prevention programs seems unlikely to have much effect, based on what we know about gun storage and prevention programs. The $600 million or more devoted to supply-side enforcement activities—improved data for background checks on gun buyers, more or better tracing of guns and bullets found at crime scenes—could have some modest effect on gun crime, but there is currently not much direct empirical evidence on this point. Perhaps the most promising element of PSN is the nearly $375 million devoted to demand-side enforcement activities such as prosecution and grants to local PSN task forces. The actual impacts of the two model programs for these activities—Project Exile and Operation Ceasefire—are taken up in detail in the next section.

GUN LOCKS

PSN's gun-lock program is presumably intended to prevent gun thefts, of which there are around 500,000 per year (Cook and Ludwig, 1996), as well as unintentional or self-inflicted injuries by unauthorized users such as children. But the distribution of 65 million gun locks, PSN's ultimate goal,[10] seems unlikely to have much effect on how guns are actually stored in the United States.

A fundamental question is why so many guns are currently stored unlocked—53% of all long guns and 57% of handguns (Cook and Ludwig, 1996, p. 21). The cost of gun locks is unlikely to be the explanation; companies such as Armadillo Firearm Security Products sell trigger locks that can be purchased off the Internet for just $5 (plus shipping). By comparison, survey data suggest that the average gun costs its owner nearly $400,

10. Available online: http://www.usdoj.gov/01whatsnew/safer_america/gun_crime.html, Accessed October 10, 2004.

and most gun owners have multiple guns (Cook and Ludwig, 1996). Ignorance about safe gun-storage practices also does not seem to explain the prevalence of unlocked firearms; owners who have participated in gun training programs are no more likely than other gun owners to store their guns safely (Weil and Hemenway, 1992). The most likely explanation is that owners wish to keep their guns readily available for self defense. Some support for this view comes from the fact that people who have guns for self defense are more likely than other gun owners to store their guns unlocked and loaded (Cook and Ludwig, 1996).

The first row of Table 2 summarizes my judgment about the risks and returns associated with the gun-lock component of PSN. The good news is that there is little uncertainty about whether we can successfully implement this intervention; distributing gun locks is fairly straightforward. However, on the basis of what we currently know about the patterns and determinants of gun storage, we can be fairly confident that any impact of distributing gun locks on gun crime will be close to zero.

SCHOOL-BASED PREVENTION

The second row of Table 2 summarizes the doubts I have about the Bureau of Alcohol, Tobacco, and Firearm's (ATF's) middle-school-based prevention program, Gang Resistance Education and Training (GREAT), the intervention's hopeful acronym notwithstanding.

TABLE 2. RISKS AND RETURNS TO DIFFERENT GUN-ENFORCEMENT ACTIVITIES

Program	Potential Returns	Evaluation Uncertainty	Implementation Uncertainty
Gun locks	Close to zero	Fairly certain	Fairly certain
Middle-school prevention	Zero to normal	Uncertain	Uncertain
Better data for gun-purchase background checks	Zero to normal	Uncertain	Fairly certain
More and better gun tracing, ballistics	Zero to normal	Uncertain	Fairly certain
Project Exile (federal gun prosecutions)	Zero to normal	Fairly certain	Uncertain
Operation Ceasefire (gang deterrence)	Zero to super-normal	Uncertain	Uncertain
Targeted anti-gun patrols	Super-normal	Fairly certain	Uncertain

GREAT is a "life skills competency program designed to provide middle-school children the ability to avoid gangs, resist conflict, make responsible decisions and develop a positive relationship with the law enforcement community."[11] Existing evaluation research leaves us with

11. Available online: http://www.atf.gov/about/snap2003.htm, Accessed October 10, 2004.

considerable uncertainty about whether GREAT has much effect on youth gun violence. The best available study of GREAT finds modest beneficial effects on the likelihood that participants are victimized, have positive attitudes toward gangs, or engage in risk-seeking behavior, but no effects on the ultimate outcomes of interest—gang involvement, drug use, or delinquency (Esbensen et al., 2001). Whether these differences between participants and nonparticipants reflect the causal impact of GREAT is unclear, given that the two groups have somewhat different outcomes even before the start of the program. Follow-up sample attrition further undermines the strength of the study's research design.[12]

Evidence from other school-based prevention programs suggests that GREAT's effects on gun crime are unlikely to be large (certainly not larger than what we would get from investing in prisons or police instead), could be zero, and in any case might be difficult to achieve on a large scale. Evaluations of previous prevention programs for delinquency and drug use suggest that although some trial efforts operated by research teams can be effective, replicating these successes on a larger scale without researcher involvement has proven to be difficult (Manski et al., 2001). One explanation for this "going-to-scale" problem may be the difficulty of ensuring that programs are implemented as designed (Gottfredson et al., 2000). Another concern is that many successful school-based prevention programs involve peers in their operations (Boyum and Kleiman, 2002; Lynskey, 1998), whereas GREAT seems to rely mostly on law enforcement to deliver the message.

DATA, BACKGROUND CHECKS, AND TRACING

PSN also includes more than $600 million over four years to help states

12. The GREAT evaluation team surveyed 49% of the sample during the first post-program year (parental consent was obtained for 57% of the students, and 86% of those with parental consent completed surveys, with $0.57 \times 0.86 = 0.49$), and only 38% of the sample was surveyed during the fourth post-program year. The low response rate undermines our confidence that the treatment and control groups that are surveyed consist of comparable types of students, particularly during the fourth post-program year, when most of the program impacts were found. To their credit, Esbensen et al. (2001) try to adjust for these problems by controlling for individual fixed-effects and observable student characteristics, although these are necessarily imperfect fixes. Esbensen and Osgood (1999) use a cross-sectional research design to evaluate the effects of GREAT, where identification of the program's effect hinges crucially on whether assignment of the GREAT treatment to some classrooms but not others is orthogonal to unmeasured determinants of youth behavior. Esbensen and Osgood argue that treatment assignment in this application was something like random, but if this is not true, then the cross-section research design provides limited power to account for other confounding factors (see Duncan et al., 2004 for a more general discussion of the limits of this "measure the unmeasured" approach).

improve the data systems used to support background checks on prospective gun buyers ($160.6 million), as well as nearly $441.6 million for ATF crime-gun tracing activities. As summarized in Table 2, the implementation challenges with both activities seem less daunting than with some of PSN's other elements. However, some uncertainty also remains about the degree to which spending in these areas will translate into reductions in gun crime. Although tracing seems more promising in some sense than improving data for background checks, I doubt that under even the best-case scenario, tracing would be much more productive than investments in other standard criminal justice activities.

Since the Brady Act went into effect in early 1994, licensed FFLs in all states have been required to conduct background checks. Previously, buyers could just sign a form attesting that they were eligible to purchase a gun under federal law, leaving open the possibility of a "lie and buy." This background check requirement applies only to licensed gun dealers. Anyone who is not "engaged in the business" of selling guns, and so is not required to obtain a federal firearms license under the 1968 Gun Control Act, is not required to conduct background checks.[13]

What effect would better background checks have on crime? My evaluation with Philip Cook of the Brady Act suggests that the shift from "lie and buy" to (perhaps imperfect) background checks seems to have had no detectable effect on gun crime in the states that were forced to change dealer practices as a result of the law (Ludwig and Cook, 2000).[14] Marginal refinements in the quality of these checks would have to have much larger effects than the imposition of any background-check requirement at all to produce a detectable impact on gun crime.[15]

PSN funding for ATF includes money to expand and improve crime gun

13. As Cook et al. (1995, p. 75) note, this is defined as: "a person who devotes time, attention and labor to dealing in firearms as a regular course of trade or business with the principal objective of livelihood and profit through the repetitive purchase and resale of firearms, but such term shall not include a person who makes occasional sales, exchanges or purchases of firearms for the enhancement of a personal collection or for a hobby, or who sells all or part of his personal collection of firearms" [from 18 U.S.C. § 921 (a) (21) (C) (1994)].

14. Lott (2000) finds that the Brady Act increased some types of crime. But Ludwig and Cook (2000) show that juvenile crime trends in the Brady "treatment" and "control" states differed even before the Brady Act took effect, suggesting that Lott's estimates, which use overall crime rates, combining those committed by juveniles and adults, are likely to confound preexisting differences between treatment and control states in juvenile crime with the treatment effect of Brady. Ludwig and Cook (2000) instead restrict their analysis to homicides with adult victims.

15. Of course depending on the nature of these data improvements, they might also have some utility for other criminal-justice activities.

tracing. In recent years, ATF has typically been able to successfully identify the first legal buyer and seller for about half of those crime guns that are submitted for tracing (Cook and Braga, 2001). Data on the "ballistics fingerprints" of bullets found at crime scenes could be used to determine when multiple guns are used in the same crime, or if a registry of such fingerprints was generated for new guns, such data could even be used to identify the first legal buyer and seller of the crime gun. Finally, ATF's funding under PSN also includes money for the Integrated Violence Reduction Strategy (IVRS), through which ATF and other law enforcement agencies identify and prosecute gun traffickers. This ATF funding will in part help identify people or gun dealers who may be channeling guns into the secondary market.

What effect will this capacity have on gun crime? The answer is, at least in my mind, not clear. Although ATF trace data suggest that a disproportionate share of all crime guns were first purchased at a relatively small share of all FFLs, no one currently knows how much of this is from illegal (or at least changeable) dealer behavior versus other factors such as the dealer's proximity to high-crime areas. Similarly, although some crime guns are undoubtedly procured through illegal "straw purchases," where someone expressly purchases a gun from an FFL on behalf of an ineligible individual, the actual prevalence of this behavior as a source of crime guns is not known. Also important will be the ability of prosecutors to secure convictions against misbehaving dealers or straw purchasers, which under current law is often far from straightforward.

Suppose that law enforcement does identify and successfully prosecute a "dirty dealer" or a straw purchaser. Although it is natural to assume that the removal of a source of guns into the secondary market will affect overall supply, the degree to which this actually occurs will depend in part on the adaptability of the supply side of the secondary market. A case study of Chicago's experiences when the Brady Act went into effect suggests that the secondary market might be remarkably adaptable to change. Before Brady, nearly 40% of all crime guns were first purchased in a state with lax gun laws that was later required to change their gun dealer practices as a result of Brady. After 1994, the share of Chicago's crime guns traced to such "Brady states" declined by about three quarters, accompanied by a substantial increase in the fraction of crime guns that were first purchased in some other part of Illinois outside of Chicago (Cook and Braga, 2001). Yet over the same time the fraction of homicides committed with firearms in Chicago hardly changed at all (Cook and Ludwig, 2003). Either the demand for guns by high-risk people in Chicago is quite inelastic to price, or the massive shift in gun-trafficking patterns had little effect

on the cost of doing business in the secondary market.[16]

In sum, I am more optimistic that crime-gun tracing could have some modest effect on gun crime than would improving the data systems used to support background checks on gun buyers. I suspect that taking dirty dealers and straw purchasers out of circulation would have some effect on gun availability to high-risk people, but we currently know so little about how the secondary gun market works that there is no way to know how large any such impact might be. For both interventions, we cannot rule out the idea of zero effect, and in both cases, a safe (perhaps generous) upper bound is the impact we could achieve from investing these resources in more police or prisons, i.e., normal returns.

PROSECUTION AND ENFORCEMENT

The most promising element of PSN, and arguably the heart of the program, is nearly $375 million in funding for the prosecution and enforcement of gun crime. DOJ notes that "Project Safe Neighborhoods expands on existing programs such as Project Exile (Richmond, VA) and Operation Ceasefire (Boston)," activities that receive $126 million in PSN funding for federal prosecution of gun cases and almost $250 million for a variety of state and local prosecution and enforcement activities.

In FY 2001, PSN included $15 million to hire 113 additional Assistant U.S. Attorneys (AUSA) across the country to focus on prosecuting gun crimes. The next year funding increased to $32 million, to support the 113 previously hired AUSAs and to hire an additional 93 federal prosecutors. The FY 2003 and 2004 budgets included $39 and $40 million in support for the 206 new AUSAs hired during the first two years.

One difference between Project Exile and Project Safe Neighborhoods is that the former focuses exclusively on federal prosecution, whereas under the latter, "criminals who use guns will be prosecuted under federal, state or local laws—depending on which jurisdiction can provide the most appropriate punishment."[17] In FY 2001, PSN provided $74.8 million to support the hiring of 540 new state and local prosecutors for the next three years to focus on gun crimes.[18] In FY 2002, the PSN program sought to

16. One possible explanation for a limited impact on the street price of guns is that most of the price markup in the secondary market may come from retail distribution, which presumably entails a nontrivial risk of injury, rather than from moving guns from FFLs to secondary market "wholesalers." It seems to be true in the market for cocaine (Moore, 1990).

17. US DOJ, "Fact Sheet-Project Safe Neighborhoods: America's Network Against Gun Violence," 5/13/03.

18. More accurately, PSN provides 80% of the costs of each new prosecutor and provides local grantees with a maximum of three years with which to use the available funding.

build a different sort of capacity by allocating around $30 million for each U.S. Attorney's district to hire a research partner and community engagement/media staff, plus another $10 million to support 40 competitive grants for "new and innovative" enforcement programs. In response to local demand for more discretion over how to use federal PSN funding, in FY 2003 and 2004, DOJ provided $59.6 and $4.5 million in block grants to local PSN task forces composed of U.S. Attorneys and other federal, state, and local agencies.[19]

What sort of activity has this collection of funding streams produced across the country? Most clearly, PSN has led to an increase in federal gun prosecutions in every U.S. Attorney District, and it seems to have increased the number of state and local gun prosecutions as well. Some districts explicitly cite Richmond's Project Exile as a model for their federal prosecution strategy, whereas others seem to make more selective use of federal prosecutions. Every district seems to have experienced an increase in collaboration and planning among local, state, and federal law enforcement agencies, one of the important elements of Boston's Operation Ceasefire. In terms of specific intervention strategies, 15 districts report that they are targeting gun trafficking, whereas 21 districts mention that they target gang activities. The degree to which this gang-oriented enforcement involves the group-oriented deterrence strategy as in Boston's Ceasefire is difficult to determine.[20] Several PSN grantees indicate that they are now making greater use of research, data analysis, and/or crime mapping. Data activities that help identify "hot spots" could in principle help focus law enforcement resources, which is one key to the targeted patrol program discussed below, although whether mapping leads to targeted patrols across PSN sites is not clear. Finally, several PSN grantees are engaged in activities that focus on unique local priorities such as gun involvement in domestic violence, a particular concern in rural areas.

In short, most U.S. Attorney districts across the country seem to be implementing at least certain elements of Project Exile and Operation Ceasefire, which is not surprising given PSN's origins and the fact that both programs are cited by the DOJ as model firearm programs. The effects of Ceasefire and Exile are considered next.

19. In FY 2002, a total of $20 million of these PSN grants was funded through Project Sentry, which focuses on juvenile gun crime, whereas $14.9 million of the FY 2003 grants and $14.8 million of the FY 2004 grants were devoted to Sentry. DOJ required grantees to devote a share of their overall PSN grant to combating youth gun violence equal to at least the fraction of the overall grants funded through Project Sentry.

20. See *PSN in Practice: Local Contributions to the Network Against Gun Violence*. Available online: http://www.psn.gov.

EFFECTS OF PSN's "MODEL FIREARM PROGRAMS"

PSN seeks to expand on Richmond's Project Exile and Boston's Operation Ceasefire. In what follows, I argue that there is very strong evidence that Project Exile in Richmond has not produced supernormal reductions in crime, although we cannot rule out more modest impacts (Table 2) or the possibility that Exile passes a benefit–cost test. The available evaluation evidence for Operation Ceasefire in Boston does not lend itself to a sharp bottom line, and it leaves the door open to the possibility of large (supernormal) impacts. There is also some uncertainty about the degree to which successful versions of either program can be exported to other settings.

PROJECT EXILE[21]

Richmond, Virginia's Project Exile was first announced on February 28, 1997. The heart of the program consists of the coordinated efforts of Richmond law enforcement and the U.S. Attorney's Office for the Eastern District of Virginia to prosecute in federal courts all felon-in-possession-of-a-firearm (FIP) cases, drugs-gun cases, and domestic violence-gun cases, regardless of the number.[22] Exile also includes training for local law enforcement on federal statutes and search-and-seizure issues, a public relations campaign to increase community involvement in crime fighting, and a massive advertising campaign. The advertising campaign is intended to signal zero tolerance for gun offenses and highlight federal sentences.[23]

Project Exile is effectively a sentence enhancement program, because the federal penalties for these firearm offenses are more severe than those in effect in Virginia in 1997. The disparity between the federal and the state systems may be particularly dramatic for FIP convictions, for which the federal penalty is five years with no chance of early release, which is of some relevance given that most additional federal convictions under Exile seem to be FIP cases (Figure 1).[24,25]

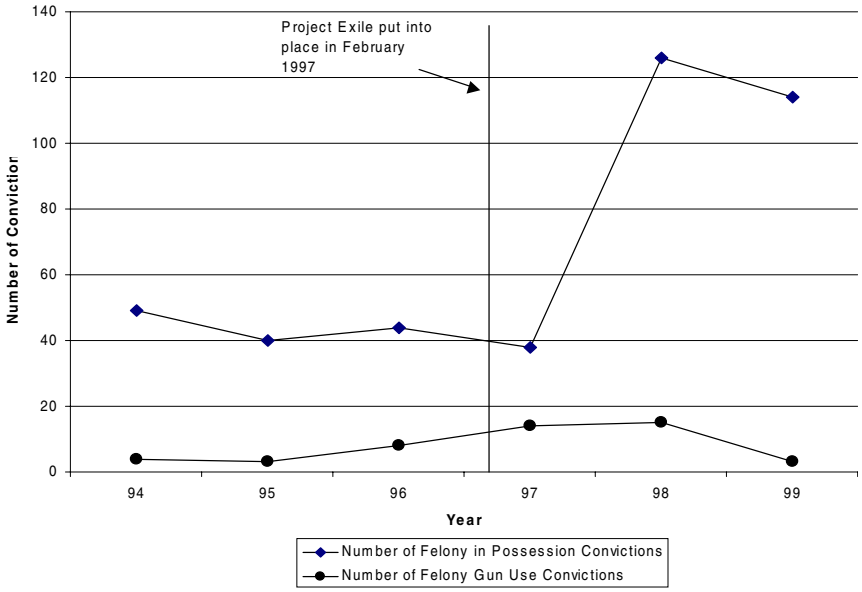
21. This section draws on the evidence and discussion presented in Raphael and Ludwig (2003).

22. U.S. Code Title 18, 922(g) (1); U.S. Code Title 18, 924 (c). In principle the local U.S. Attorney for Richmond also has the option of prosecuting those who sell a handgun or ammunition to juveniles [U.S. Code Title 18, 924 (x)], although in practice federal prosecutors rarely seem to take such cases, in part because the penalty for the first conviction of this offense is simply probation.

23. For a detailed description of Project Exile, see the summary statement available from the U.S. Attorney's Office for the Eastern District of Virginia.

24. Besides the difference in prison terms, gun offenders diverted into the federal system are denied bail at a higher rate than those handled in state courts, and they serve time in a federal penitentiary that may be located out of state. Both aspects of the

FIGURE 1. THE ANNUAL NUMBER OF FELON-IN POSSESSION AND FELONY-GUN-USE CONVICTIONS PROSECUTED BY THE U.S. ATTORNEY'S OFFICE FOR THE EASTERN DISTRICT OF VURGINIA, 1994–1999



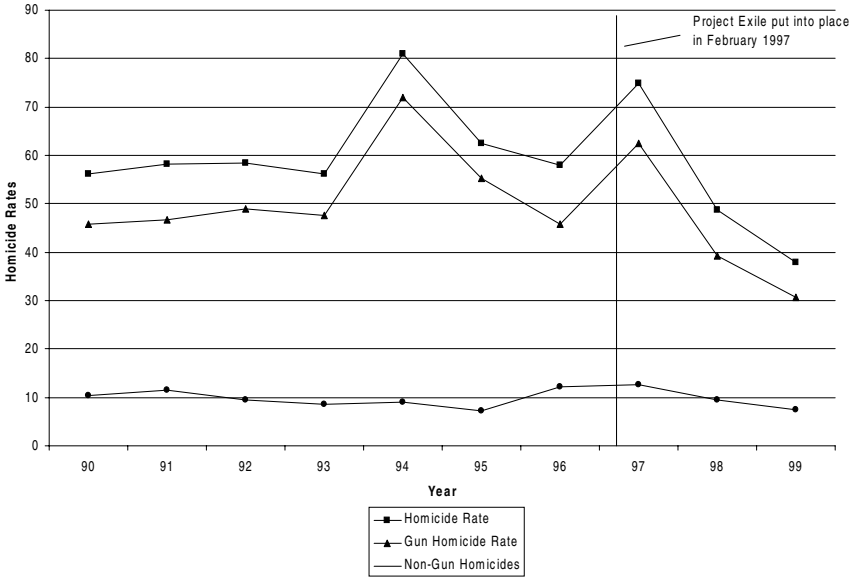
Claims for Exile's success stem from the 40% reduction in gun homicides observed in Richmond from 1997 to 1998 (Figure 2). Despite this acclaim, some skeptics have questioned the effectiveness of Project Exile, pointing out that homicides increased in Richmond in the last ten months of 1997 after the program's announcement. In fact, Richmond's homicide rate increased by 40% between 1996 and 1997. Deciding which year should be counted as the first post-Exile period in Richmond is obviously crucial to any evaluation of the program's effects. The data on firearms prosecutions by the U.S. Attorney for the Eastern District of Virginia, which are shown in Figure 1, indicate that the number of prosecutions did not show a noticeable increase until 1998. We treat 1998 as the first year

program are thought to impose additional costs on offenders. In sum, the primary criminal-justice change introduced by Project Exile seems to be an increase in the prison penalties for carrying guns by those with prior felony convictions.

25. One prosecutor from Philadelphia suggested that the increase in federal prosecutions in Richmond may have been concentrated among FIP cases because these lend themselves so readily to an easy conviction—the prosecutor simply needs a rap sheet and a single law enforcement witness.

Project Exile is fully in effect, which increases the odds that we find a program impact.[26]

FIGURE 2. ALL HOMICIDES, GUN HOMICIDES, AND OTHER HOMICIDES PER100,000 RESIDENTS IN RICHMOND, VIRGINIA, 1990–1999



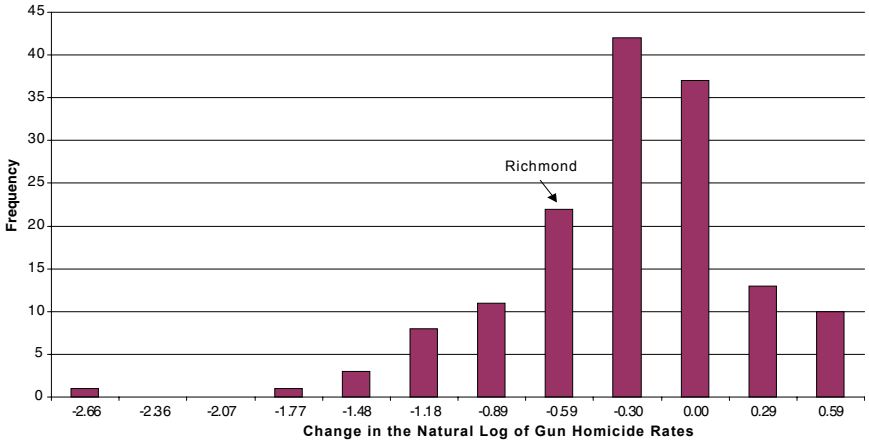
Even with 1998 chosen as the first Exile year, whether the program has been successful is not obvious from Figure 2. The large year-to-year changes in homicide rates observed in Richmond suggest that much of the increase observed in 1997 may reflect transitory factors that would have disappeared anyway. Using this unusual year as a base for calculating the change is bound to inflate the apparent impact of the program. Moreover, the patterns in Figure 2 seem to indicate that, 1997 aside, homicide rates in Richmond were trending downward even before the launch of Project

26. In principle Exile may still have had some effect on crime in 1997 through an "announcement effect," in which the publicity surrounding the program changes the expectations that criminals have about the penalties for gun offenses. It is also possible that word about Project Exile spread among Richmond's criminal population after the initial wave of federal indictments, which may have occurred in 1997, rather than convictions, which seem to have started in 1998. However, we accept the argument that convictions are likely to be an important part of the program's deterrent effect and thus choose 1998 as the first year in which Exile is considered to be in full effect.

Exile. To the extent that the post-Exile declines simply reflect the continuation of trend, the raw numbers offered in support of the program are likely to overstate Exile's impact.

Did Richmond's crime trend during this period differ from what we would have expected in the absence of Exile? One way to answer this question is to consider whether Richmond's experiences were unique compared with other cities over the same time period. Figure 3 shows a histogram of proportional changes in gun homicide rates from 1995-1996 to 1998-1999 for cities with populations of 100,000 or more. To avoid the problems associated with 1997, we omit this year. The proportional change in gun homicide rates in Richmond around the time of Project Exile was not very unusual compared with the experiences of other cities during these years.[27]

FIGURE 3. HISTOGRAM OF THE CHANGE IN THE NATURAL LOG OF THE CITY-LEVEL GUN HOMICIDE RATES, ln(95/96) TO ln(98/98)



Proponents of Project Exile might take some solace in that the proportional decline in gun homicides after Exile was at least above average compared with other cities. But these simple pre–post-comparisons fail to account for the crucial role of Richmond's initial conditions in predicting

27. We focus on gun homicides in our calculations because Exile is designed to deter the use and illegal possession of firearms. Because of the possibility of substitution from gun to non-gun violence, a reduction in gun homicide is a necessary but not sufficient condition for a program impact on the overall homicide rate. However, given that most homicides are committed with guns, we find similar findings when we focus on all homicides, gun and non-gun together.

future changes in city homicide rates. Richmond experienced unusually large increases in homicide rates during the decade or so preceding the implementation of Project Exile. These initial conditions carry important implications for our evaluation because those cities with the highest homicide levels during the early 1990s, and with the largest increases in homicide before this period, also experienced the largest decreases during the late 1990s. The increase in crime from the mid-1980s to mid-1990s is believed to have been driven by crack use and gun involvement in crack markets (Blumstein, 1995). Steve Levitt (2004) has argued, persuasively in my view, that most of the crime drop during the 1990s is from some combination of increased incarceration and police hiring, the effect of abortion legalization in the early 1970s, and the end of the crack epidemic. Whatever the cause, this empirical regularity—the higher they climb, the farther they fall—suggests that the pre–post-Exile change in homicides in Richmond may largely have been a function of the run-up during earlier periods.

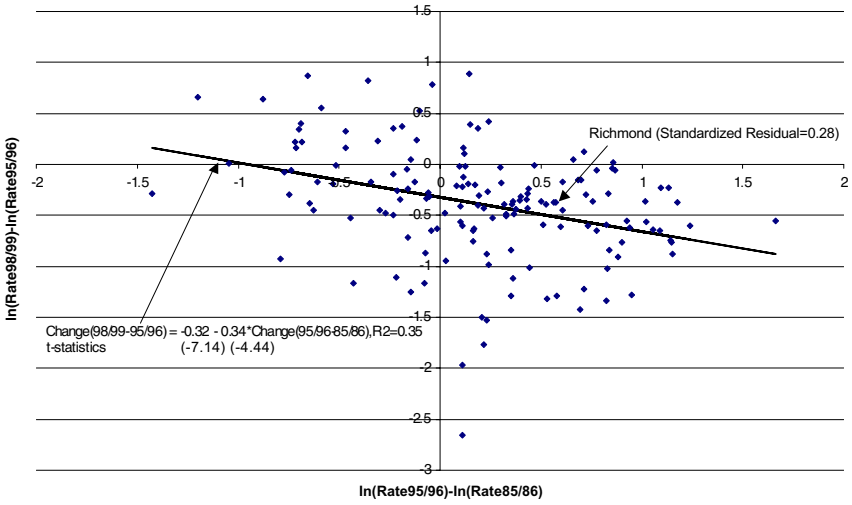
Figure 4 shows more formally that the decline in gun homicides observed in Richmond after Exile is almost exactly what we would have predicted in Exile's absence. The figure plots each city's change in log gun homicide rates from 1995–1996 to 1998–1999 against the city's change over the prior decade, from 1985–1986 to 1995–1996. The figure also includes the fitted linear regression line that summarizes the overall relationship between the homicide changes across time periods. The slope to this fitted regression line is negative, which implies that areas that experience larger increases in homicide rates go on to experience larger reductions thereafter. The regression predicts that Richmond would have experienced an even larger proportional decline in homicide in the absence of Project Exile, as evidenced by the fact that the Richmond data point lies above the regression line.[28] Similar results hold when we look at other types of crime measured by the FBI's Uniform Crime Reporting system.

In principle there is the possibility that Exile was effective, but that this impact is masked in our analysis because other cities to which Richmond is compared enacted their own effective interventions. Arguing against this counterexplanation is that among the large-scale factors (aside from the waning of the crack epidemic) that Levitt argues explains most of the crime drop in the 1990s—increased imprisonment and police hiring, as well as legalized abortion in the early 1970s—Virginia does not seem to be exceptional compared with other states.[29,30]

28. We obtain similar findings when we use the actual not log gun homicide rate, the overall homicide rate, or calculate the regression lines weighting each data point by the city's mid-1990s population.

29. For example, data from the Alan Guttmacher Institute show that the trends in abortions per 1,000 women ages 15–44 for Virginia and the United States tracked each

FIGURE 4. SCATTER PLOT OF THE PRE–POST-EXILE CHANGE IN THE NATURAL LOG OF GUN HOMICIDE RATES [ln(98/98) – ln(95/96)] AGAINST THE CHANGE IN THE NATURAL LOG OF GUN HOMICIDE RATES OVER THE PRIOR DECADE



We can confidently rule out the idea of supernormal but not normal returns to spending on Project Exile. Suppose that everyone in Exile would have been prosecuted and imprisoned by the state of Virginia absent the program, so that the main effect of Exile is to, say, double the average prison sentence length. The cost of federal prosecution in this case is just a transfer from the federal to the state government, whereas the

other quite closely during the period 1973–2000. (Available online: http://www.agi-usa.org/pubs/state_ab_pt/virginia.pdf, Accessed on 9/29/04.) Data from the FBI's *Crime in the United States* publications in 1990 and 2000 show the number of total police personnel in the United States increased by around 30% over this period, compared with an increase of 32% in Virginia. And the number of prisoners in state or federal correctional facilities increased by nearly 80% from 1990 to 2000 in the United States as a whole and by nearly 72% in Virginia (*Sourcebook of Criminal Justice Statistics*, 1991, 2001).

30. We also might expect Exile to produce a disproportionately large effect on adult rather than on juvenile crime, because mostly adults will have prior felony convictions that make them eligible for federal prosecution. Presumably other interventions administered by different cities over this period will not have the same adult-oriented focus, and in fact some programs (like Operation Ceasefire in Boston, described below) have a juvenile focus. In this case, we would expect any Exile-driven effect in Richmond to be manifest by a relatively larger decline in adult than in juvenile homicide arrests compared with what is observed in other areas. But this does not seem to be the case.

social cost of Exile would equal the costs of the additional 2.5 years in prison per conviction. As Levitt (2003) notes, existing estimates suggest that, on average, a one-person increase in the prison population in general reduces 0.004 homicides through the combined effects of deterrence and incapacitation, so that the expected effect of the 80 additional FIP convictions secured under Exile would be too small to be detected with our data. Note that Exile might still pass a benefit-cost test even if each FIP conviction produces the average effect on gun injuries suggested by Levitt and has no effect at all on other types of crime.[31]

On the other hand, Richmond's Project Exile could produce smaller effects on gun violence than Levitt's elasticity estimate for the average effects of imprisonment if the "zero tolerance" approach led to federal prosecution of many low-risk cases, which is a concern raised by Greenwood (2003). This hypothesis is nicely illustrated by the case of Katica Crippen, who was prosecuted under Colorado's Operation Safe Neighborhoods. A 1997 drug arrest left Crippen with a felony on her criminal record. As part of an extramarital affair several years later, she agreed to let her boyfriend take, and post on the Internet, a photograph of her wearing nothing but the electronic monitoring bracelet required by her parole, holding a gun in each hand. Her husband saw the picture on the Internet and turned her in to her parole officer, which led to a FIP conviction by the U.S. Attorney's Office in Denver.[32] Crippen might be guilty of poor taste and even worse judgment. But whether she also represents a danger to public safety is less clear.

In sum, more selective federal prosecution of gun cases could in principle achieve supernormal returns to criminal justice spending, although there is currently no good empirical evidence on this point. Moreover, the impact of selective federal prosecution hinges on the ability of PSN task forces to successfully identify those cases in which federalization would achieve the greatest benefits. In any case, what we can be quite confident of is that widespread federal prosecution of every eligible gun case does not produce the supernormal returns that have been claimed for Project

31. The average cost per person per year in prison (including foregone earnings) is estimated to be on the order of $36,000 in 2004 dollars (from Donohue and Siegelman, 1998). If each person-year of prison time averts 0.004 homicides produces benefits to society on the order of $4,000 to $24,000, depending on whether we use a value per statistical life on the low end or in the middle of the plausible range (see Cook and Ludwig, 2005). If we assume that there are five nonfatal assault-related gunshot injuries for every gun homicide, and that the social cost per injury is $1 million, then the benefits from fewer injuries equals around $20,000 per year per prisoner (Cook, 1985; Cook and Ludwig, 2000; Ludwig and Cook, 2001).

32. See Craig R. McCoy, "Colorado Woman, Jailed for Posing with Guns, Is Still Bewildered." *The Philadelphia Inquirer*, January 26, 2003.

Exile.[33]

OPERATION CEASEFIRE

Operation Ceasefire was the result of a collaboration called the Boston Gun Project Working Group started in 1995, involving among others a research team from Harvard's Kennedy School of Government, the Boston PD, the state departments of probation and parole, ATF, and the local U.S. Attorney's office. Data analysis by the working group suggested that Boston's gun homicide problem was driven in large part by young, gang-affiliated youth who committed gun violence often for reasons related to chronic disputes among rival gangs—"gang beefs." Ceasefire was put into place in June 1996, with the goal of targeting law enforcement resources to both reduce the supply of guns to gangs and increase the costs to gangs of using guns in crime (see Braga et al., 2001; Kennedy et al., 2001).

Crime-gun trace data from ATF suggest that most crime guns in Boston originate out of state. Operation Ceasefire sought to increase the price of guns by targeting the suppliers that serve as the source of guns for Boston gangs. Ceasefire devoted additional attention to using ATF crime-gun trace data to identify possible sources of both across- and within-state gun trafficking into Boston, and to target these sources for prosecution.[34]

On the demand side, Operation Ceasefire focused police resources on deterring violence by gang members, particularly gun violence. Gangs were informed by law enforcement that gun use by any member would produce a concentrated crack-down on all the gang's members and activities (including income-generating activities) by law enforcement at all levels of government, a strategy known as "pulling levers." One hope was to help change social norms within the gang about gun crime. Another hope was that a halt in intergang violence would provide a "cooling off" period that would break the dynamic of violence fueled by gang beefs and retaliatory attacks.

Any formal evaluation of Ceasefire as it was implemented in Boston must confront two complications. The first is distinguishing between noise and trend in the city's crime rate during the 1990s. High-frequency (monthly) data suggest that youth homicide counts may have started to decline in Boston in Fall 1995 even before Ceasefire went into effect in summer 1996 (Braga et al., 2001, p. 205). When Piehl et al. (2003) search

33. Previous research on the effects of sentence enhancements for violent gun crimes, as distinct from the gun carrying cases that are the focus of Exile, yield mixed results. Compare, for example, McDowall et al., (1992) with Marvell and Moody (1995).

34. Related efforts included increased attention to restoring obliterated serial numbers on confiscated crime guns, which is the identifying characteristic used to trace crime guns back to their original purchase, as well as by trying to identify trafficking sources by debriefing arrestees, particularly those with gang affiliations.

the time series for the "optimal break" in trend starting with the observations for January 1996, the data point to summer 1996, although there would still seem to be the possibility of a sharp break in trend before their search window.

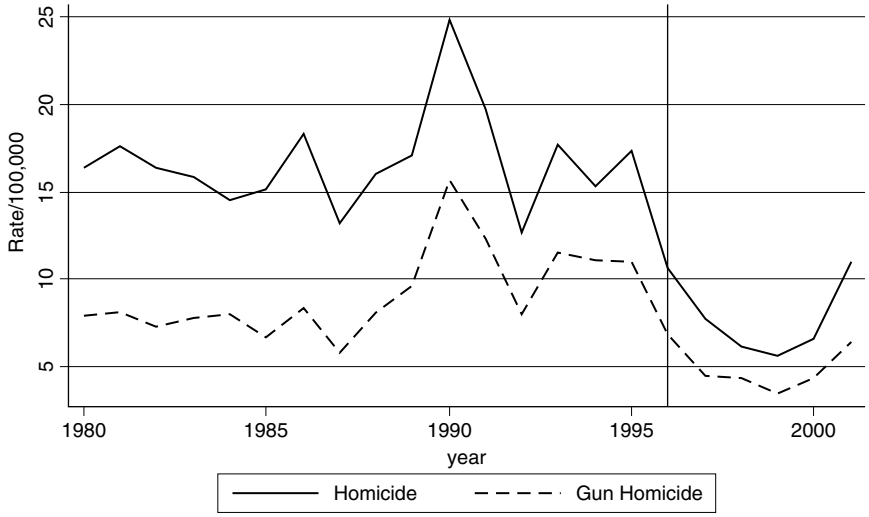
Short-term fluctuations in the data can be smoothed out in part by looking at annual data over a long-term horizon, as in Figure 5. Homicide rates in Boston, as in most of the largest American cities, peaked during the early 1990s and were significantly lower at the end of the decade (see Blumstein, 2000, p. 38; Eck and Maguire, 2000, p. 234; or Levitt, 2004, p. 168). But one way in which Boston's homicide trend is unusual is that after declining in the early 1990s, rates increased again in 1993–1995, despite the trend in "fundamentals" that Levitt (2004) argues drove crime rates down everywhere during the 1990s—before resuming their decline.[35] Most studies use this 1993–1995 period as the "pre-treatment" benchmark period for evaluating Ceasefire. However if the 1993–1995 increase represents a temporary deviation from trend, then comparing post-Ceasefire rates to this period may to some extent confound the impact of Ceasefire with that of mean reversion. One way to circumvent this problem is to focus on the long-run trend in homicides in Boston, for example, over the period 1991–2001. Under this approach, Boston's decline in homicides was (in proportional terms) about average compared with what was observed in the 25 largest cities in the country (Levitt, 2004, p. 168).

Another challenge for any evaluation of Operation Ceasefire in Boston is to construct a valid estimate for the counterfactual scenario of what would have happened in the city in the absence of the intervention. Differing perspectives about the most appropriate comparison group for Boston have led to competing claims about the efficacy of the Ceasefire intervention.[36] When Braga et al. (2001) compare Boston's experience before and after Ceasefire with what was observed in other large cities across the country, they conclude that Boston experienced an unusually pronounced decline in youth homicides. In contrast, Fagan (2002) argues that any unmeasured determinants of homicide may vary by geographic area rather than (or in addition to) by city size. Fagan shows that around the time of Ceasefire, the proportional decline in youth gun homicides was

35. Youth homicides show a similar pattern; see Cook and Ludwig (2004a) or Braga et al. (2001).

36. In principle, another complication with the evaluation of Ceasefire in Boston is the launching in 1992 of the Ten Points Coalition, a collaboration between the Boston Police Department and leading African-American clergy in the city. However, the time path of homicides in Boston does not show a decline at this point, although this is admittedly a weak test of the role of this effort (see Berrien and Winship, 2003; Berrien et al., 2000; Winship, 2002).

FIGURE 5. BOSTON HOMICIDE TRENDS



as large or larger in other townships throughout Massachusetts compared with Boston.

It is intriguing to note that other cities that have implemented Ceasefire-style interventions more often than not also report similarly encouraging before–after trends as in Boston (Braga and Kennedy, 1998; Braga et al., 2002; McGarrell and Chermak, 2003; Wakeling, 2003). Unfortunately the idiosyncratic downward trends in crime observed for most U.S. cities during the 1990s complicates efforts to derive strong inferences from these cities as with Boston's Ceasefire.

Given the limits of quantitative evaluations of Ceasefire, other forms of evidence such as qualitative findings may in principle also help shed light on the program's effectiveness. In this case, suggestive support for the gang-oriented deterrence component of Ceasefire comes from ethnographic fieldwork from Chicago's South Side, which Sudhir Venkatesh has conducted as part of a collaborative project with Philip Cook, Anthony Braga, and myself on underground gun markets. Venkatesh finds that to some extent the Chicago police may already informally use some components of Ceasefire's gang-oriented deterrent approach, and that as a result many gang leaders seek to regulate gun use by current and former members (Cook et al., 2005b).

Although Ceasefire's gang-deterrence strategy holds considerable conceptual appeal and enjoys some support from qualitative research, the limits of the available quantitative evaluations necessarily leave us with some uncertainty about the program's actual impacts in practice (Table 2). Another source of uncertainty comes from the difficulty of replicating the successful collaboration across agencies that was achieved in Boston, at least as suggested by the Los Angeles experience.[37]

MORE EFFECTIVE GUN-LAW ENFORCEMENT

How could PSN and gun enforcement generally be made more effective? I begin by discussing a demand-side enforcement strategy for which there is strong empirical support (at least comparatively) and little PSN or other federal funding—targeted patrol against illegal gun carrying. Although targeted patrols seem to achieve supernormal returns to criminal justice spending, this strategy may entail difficult tradeoffs between crime control and civil liberties. I conclude with some thoughts about what a more effective federal enforcement initiative might look like.

TARGETED PATROL[38]

The heavy concentration of gun violence among a small subset of city residents and neighborhoods provides the rationale for targeting police resources against the highest-risk people and places. In contrast to focusing very tightly on criminal gun use by gang members, as in Operation Ceasefire, an alternative approach is to widen the enforcement lens a bit and focus on efforts to reduce illegal gun carrying, a precursor to much gun misuse, in high-crime communities. My evaluation with Jacqueline Cohen of Pittsburgh's experience with targeted patrol provides evidence of effectiveness that is fairly strong, at least compared with what is available for other law-enforcement interventions. On the other hand, targeted patrol may entail larger nonmonetary costs compared with Exile or Ceasefire.

In 1998 the city of Pittsburgh implemented a targeted patrol program

37. A study of the Los Angeles attempt to implement a similar "pulling levers" intervention reinforces the idea that keeping complex interagency collaborations focused on a given program can be difficult. Although the L.A. Ceasefire program intended to reallocate resources in a dynamic way to crack down on gangs as they became involved in gun violence, as in Boston, a high-profile shooting in the city during the program's early stages led to a sustained focus on the two gangs involved with the triggering event (Tita et al., 2003). The result is a significant deterrent message to two of the city's gangs but with little change in expected penalties to most gangs. See also Braga (2002) for a discussion and possible solutions.

38. The discussion in this section draws heavily from Cohen and Ludwig (2003).

that provides a unique opportunity for evaluation.[39] Using funding provided by the Alfred P. Sloan Foundation, the Pittsburgh Police Department (PPD) assigned additional police resources to selected high-crime communities within the city. These patrols were relieved from responding to citizen requests for service (911 calls) to work proactively to search for illegally carried guns. Police contacts were initiated mainly through traffic stops and "stop-and-talk" activities with pedestrians in public areas. Carrying open alcohol containers in public and traffic violations were frequent reasons for initiating contact. When warranted for reasons of officer safety (usually because of suspicious actions or demeanor), these stops sometimes moved to the types of pat-downs on the outside of clothing to check for weapons that are allowed under the Supreme Court's 1968 decision in *Terry v. Ohio* (392 U.S. 1, 1968). When there was reasonable suspicion of criminal activity, the contact might escalate to more intrusive searches inside pockets, under coats, and in waistbands as part of an arrest.

These targeted patrols were directed to two of Pittsburgh's five police zones that had unusually high crime rates (zones 1 and 5). Each zone includes around 35 census tracts, nearly ten square miles and between 55,000 and 80,000 residents.[40] Under the targeted patrol program, one additional patrol team was assigned to each of the two designated zones. These teams consisted of four officers and a sergeant (all in uniform) traveling in three vehicles, usually two marked patrol cars and one unmarked car. Each team worked four-hour shifts from 8 P.M. to midnight twice weekly for 14 weeks from July 19 to October 24, 1998, focusing on the

39. One widely cited source of support for the efficacy of targeted patrol is the Kansas City Gun Experiment, which assigned additional police resources to more vigorously pursue illegal guns in one high-crime neighborhood of the city but not another (Sherman and Rogan, 1995; Sherman et al., 1995). Although the "treatment" neighborhood experienced a 65% increase in the number of guns seized by the police and a 49% reduction in gun crimes, neither outcome measure showed much change over this period in the "control" neighborhood. Although these findings are suggestive, the program is not an "experiment" as scientists use the term: The target neighborhood was selected first, and then the control area was selected in part on the basis of its similarity to the treatment community. Unlike with a randomized experiment there is no guarantee that the two neighborhoods would have had similar crime rates had the policing intervention not been launched. Some support for this concern comes from the fact that gun crimes were more common in the "control" neighborhood for extended periods even before the new policing program was initiated. An evaluation of a similar program in Indianapolis suffers from the same general limitation (McGarrell et al., 2001).

40. Although the zones have high crime rates, there is some heterogeneity across census tracts within zones. For example, in around one third of tracts in zone 1 and one fifth in zone 5, fewer than 5% of residents are African-American. Each zone also contains seven tracts in which more than half of all residents are African-American. And amid the high-crime tracts in each zone are census tracts that experienced no youth homicides at all in the recent peak year, 1993.

high-crime evenings of Wednesday through Saturday nights.[41] With the assistance of maps and reports of recent shots-fired activity, patrol teams identified and targeted "high-risk places at high-risk times."

During this period, 51 special patrol details were fielded across the two zones involving nearly 1,000 officer-hours (including the sergeant's time). Given the absolute size of these target zones, the "dosage" of the intervention may seem low in absolute terms. However, the patrols covered 30% of the high-risk times on the highest-risk days (Wednesday through Saturday) each week. Moreover, the three-vehicle, five-officer teams represented a large increment to customary patrol resources in the target police zones. Police vehicles increased by 20% and patrol officers by 25% in target zone 5, the city's highest crime zone. The increases were even larger in the other target area (zone 1), with a 35% increase in vehicles and a 50% increase in officers.

One way to evaluate the Pittsburgh program would be to simply compare trends in gun violence between the two "treatment" zones and the three "control" zones from the pre-patrol to post-patrol periods. Yet this type of standard "difference-in-difference" comparison is not without limitations: One concern is that crime rates in the treatment communities may simply have followed a different trajectory over time from those of the control areas anyway, even without the intervention.

One unique aspect of Pittsburgh's program is that the police patrols were launched on some days of the week (Wednesday through Saturday, the "on days") but not others (Sunday to Tuesday, "off days") within the treatment communities. We compare trends between the treatment and control neighborhoods in the periods before and after the policing program is launched, focusing on gun misuse during the on days. If the policing program has an effect, we would expect a greater decline during the on days of the week in the treatment than the control communities, and this decline should be larger than the difference in trends across neighborhoods observed during the off days. To the extent that unmeasured confounding variables cause the treatment neighborhoods to have different trends from the control areas during every day of the week, this approach controls for these omitted factors by comparing across-area trends during on versus off days. This strategy thus isolates the causal effects of those factors unique to the target neighborhoods after the launch of the police patrols during the days when these patrols are active—factors such as the police patrols themselves.

More formally, we compare within-week across-neighborhood trends in

41. Specific patrol days were designated to ensure a mixture of different days covered in each zone. The most common pattern (found in half the weeks) was alternating days, either Wednesday and Friday or Thursday and Saturday, in individual zones.

both shots fired reports to the police and hospital emergency department admissions for gunshot wounds using the "difference-in-difference-in-difference" (DDD) research design described by Table 3.[42] If the targeted patrol program reduced gun crime within the treatment communities, we would expect to see a relatively larger decline in gun violence during the on days compared with the off days, so that [(B-A) – (D-C)] < 0. Of course it could just be the case that the on and off days experience different trends in crime rates citywide. To rule out this possibility, we compare the relative decline over time in on versus off days in the treatment communities compared with what is observed in the control areas. If the targeted patrols have an effect on gun violence, we expect this difference to be negative; i.e., we expect that [(B-A) – (D-C)] – [(F-E) – (H-G)] < 0.

TABLE 3. RESEARCH DESIGN FOR PITTSBURGH TARGETED PATROL EVALUATION

	Pre-Period	Post-Period	Estimated Differences
Target (treatment) zones			
Wednesday–Saturday	A	B	(B-A)
Sunday–Tuesday	C	D	(D-C)
Difference-in-difference			(B-A) – (D-C)
Control zones			
Wednesday–Saturday	E	F	(F-E)
Sunday–Tuesday	G	H	(H-G)
Difference-in-difference			(F-E) – (H-G)
Difference-in-difference-in-differences			[(B-A)-(D-C)] – [(F-E)-(H-G)]

Note: Pre-period is defined as the 6 weeks before the targeted patrol program, whereas post-period is defined as the 14 weeks during which the patrol program was in operation.

Table 4 provides evidence consistent with the idea that the targeted patrol program has reduced shots fired in the treatment zones compared with the control zones. First, notice that the trend in shots fired from the

42. As described in detail by Cohen and Ludwig (2003), the shots fired data come from Pittsburgh's 911 Operations Center and include information about the date, time, and address of the reported incident. Some adjustment is made to these data to avoid double-counting multiple calls to report the same incident. Because discharging a firearm within the city limits of Pittsburgh is against the law, our measure of shots fired captures an event that is technically a "gun crime." But more important, data on shots fired are correlated both over time within Pittsburgh and across neighborhoods at a point in time with other measures of gun crime. Data on assault-related gunshot injuries come from hospital discharge records from the Allegheny County Health Department, which collects data from four trauma centers that together capture more than 90% of gunshot injuries treated in hospitals in the Pittsburgh area. We classify these injuries into treatment and control zones on the basis of the victim's home ZIP code. Analysis of homicide data from 1990 to 1995 from Pittsburgh suggests that in 81% of cases, the victim lives in the same police zone in which the murder occurred.

TABLE 4. IMPACT ESTIMATES FOR SHOTS FIRED, PITTSBURGH TARGETED PATROL EVALUATION

	Pre-Period	Post-Period	Estimated Differences
Target (treatment) zones			
Wednesday–Saturday	0.979	0.670	–0.310
Sunday–Tuesday	0.444	0.702	0.258
Difference-in-difference			–0.568**
			(0.088)
Control zones			
Wednesday–Saturday	0.323	0.281	–0.042
Sunday–Tuesday	0.208	0.387	0.179
Difference-in-difference			–.0221*
			(0.120)
Difference-in-difference-in-differences			–0.347**
			(0.133)

Note: Pre-period is defined as the 6 weeks before the targeted patrol program, whereas post-period is defined as the 14 weeks during which the patrol program was in operation.
* Statistically significant at 10% cutoff; ** statistically significant at 5% cutoff.; standard errors in parentheses.

pre- to post-program period is similar for the treatment and control communities during the "off" days, with an increase of 0.258 for the former and 0.179 for the latter. The fact that the treatment and control areas track each other during those days of the week when the targeted patrols are not operating strengthens the case that the control zones provide a reasonable estimate for what would have happened in the treatment zones in the absence of the intervention. In contrast to the similar trends during the "off" days, shots fired declined by a relatively larger amount in the treatment zones during the on days, equal to –0.310 versus a more modest decline of just –0.042 in the control zones. The DDD estimate shown in the final row of Table 4 is equal to –0.347, which is equal to about a one-third reduction in shots fired and is statistically significant at the 1% cutoff (using a one-sided test). Table 5 provides similar findings for assault-related gunshot injuries from hospital discharge data, which suggest the patrols may have reduced such injuries by more than two thirds in the treatment communities. These findings are robust to changing the number of weeks used to define either the pre- or post-program periods, or to changing the mixture of zones used to construct the comparison group.

As an additional check for bias from omitted variables, we also replicate the analysis using data from 1997 and 1999, the years before and after the patrol program was in operation. That is, we calculate the DDD estimate using the same calendar days that define the pre- and post-program periods and on and off days for non-program years. We find no evidence of "phantom" treatment effects during either 1997 or 1999 for assault-related

TABLE 5. IMPACT ESTIMATES FOR ASSAULT-RELATED GUNSHOT INJURIES, PITTSBURGH TARGETED PATROL EVALUATION

	Pre-Period	Post-Period	Estimated Differences
Target (treatment) zones			
Wednesday–Saturday	0.250	0.089	–0.161
Sunday–Tuesday	0.028	0.131	0.103
Difference-in-difference			–0.264*
			(0.208)
Control zones			
Wednesday–Saturday	0.073	0.080	0.007
Sunday–Tuesday	0.028	0.077	0.050
Difference-in-difference			–0.042
			(0.042)
Difference-in-difference-in-differences			–0.222*
s			(0.165)

Note: Pre-period is defined as the 6 weeks before the targeted patrol program, whereas post-period is defined as the 14 weeks during which the patrol program was in operation.
* Statistically significant at 10% cutoff; ** statistically significant at 5% cutoff.; standard errors in parentheses.

gunshot injuries. For shots fired, we find no phantom effect in 1999, although we do find signs of a significant DDD "effect" across zones in 1997. The fact that we find an "echo" of the program impact the year before the patrols were implemented highlights the fact that this and any nonexperimental research design is not as strong as a randomized experiment. We are thus more cautious about drawing inferences about shots fired than about assault-related gunshot injuries.

It is interesting to note that Pittsburgh's patrol program seems to have reduced gun crime in the target neighborhoods even though Pennsylvania's court system as a whole does not seem to treat gun cases more seriously than do other states. Measuring the probability of arrest and conviction for weapons offenses at the state level is complicated in part by the lack of data on the prevalence of illegal gun carrying. From the data that are available, we may conclude that the probability of conviction conditional on being arrested for a weapons offense is not substantially different in Pennsylvania than in Virginia, the site of Project Exile.[43] Taken together these findings seem consistent with the idea that increasing the

43. The ratio of prison commitments for weapons offenses to weapons arrests in Pennsylvania equaled 0.027 in 1995, 0.017 in 1996, 0.065 in 1997, and 0.030 in 1998. By comparison, the ratio in Virginia equaled 0.028 in 1995, 0.031 in 1996, 0.038 in 1997, and 0.055 in 1998.

probability of being caught for carrying a gun illegally, as under Pittsburgh's targeted patrol program, may be more effective than increasing penalties as by federalizing such cases under Richmond's Project Exile.

If we focus just on the monetary costs of targeted patrol, then we would conclude this strategy clearly produces supernormal returns. For example, in Pittsburgh's case, the cost of supporting the officer hours necessary for targeted patrols was on the order of $35,000. In contrast, if the costs of gun violence are on the order of $1 million per injury, then the benefits of Pittsburgh's targeted patrol program may be as large as $25 million. Given the disparity between estimated benefits and costs, our conclusion of supernormal returns to targeted patrol would seem to be robust to even large measurement errors on either the benefit or the cost side of the equation.

On the other hand, nonmonetary costs might be particularly important with targeted police patrols. As Mark Moore notes, "the strategy obviously invites the police to be more intrusive . . . [police] will stop and 'pat down' many people who are not carrying weapons. Each of these misses or errors imposes a cost. . . . One can also worry that the costs of police intrusiveness would not be borne equally by individuals in the society, but would instead by concentrated among the poor, the young, and the racial minorities who would be the likely, and statistically reasonable, targets of police scrutiny" (1980, pp. 26–27). The relationship among race, class, and both victimization and offending rates, together with the persistence of racial and class residential segregation, means that anti-gun patrols targeted at high-crime neighborhoods will disproportionately harm, and help, low-income minorities (Skolnick and Caplovitz, 2003 make a similar point).

One key question for targeted patrolling against illegal guns is whether this can be done in a way that does not harm community-police relations. Many observers use New York City's experience during the 1990s as Exhibit A for what not to do (Berrien and Winship, 2003; Fagan and Davies, 2003; Skolnick and Caplovitz, 2003).[44] The increase in police–community hostility thought to have resulted from the NYPD's aggressive stop-and-frisk activities could in principle even undermine public safety in the long run. As Jeffrey Fagan and Garth Davies argue, "Social control within communities functions well when there is a strong interaction of informal and formal (legal) social control. When legal control engenders resistance, opposition or defiance, the opportunity to leverage formal social control into informal social control is lost" (2003, p. 210).

44. For example, Fagan and Davies (2003, p. 195) argue: "In New York, aggressive stop-and-frisk strategies produced a style of racial policing that had stigmatizing effects on minority communities, generally, and socially toxic effects on African-Americans."

On the other hand, Pittsburgh's experience provides something of an "existence proof" that stepped-up targeted patrol can be done without aggravating community–police relationships. After implementation of the Pittsburgh program, the city's police department experienced no increase in citizen complaints, although whether there were more subtle changes in community attitudes toward the police is more difficult to determine. Moore's (1980) study of policing in several U.S. cities suggests that more proactive policing can increase weapons arrests without much change in the demographic profile of arrestees. The keys to success may include intensive officer training and strong police management (Pittsburgh's PD paid particular attention to both dimensions with their program), the use of crime data analysis to carefully focus patrol activities on the highest-crime places and times to reduce the "false positive" rate (on this point, see also Kleck, 1997, p. 394), and perhaps even the involvement of the community in designing and implementing the intervention. For example, in Boston, local ministers helped steer the police toward the most troubled teens and more generally seem to have enhanced the legitimacy of police activities in that city (Berrien and Winship, 2003).

INVESTING IN ENFORCEMENT

Given the state of current gun-policy research, voters and their representatives are forced to make policy decisions in the face of considerable uncertainty. Given the significant costs of gun violence, even programs that yield modest benefits can pass benefit–cost tests. Yet available data and statistical techniques often are not capable of detecting small impacts, and theory alone usually provides a weak basis for predicting the relative benefits and costs of a given intervention strategy. One way to deal with the resulting uncertainty about "what works" is to prioritize funding for those programs that provide the greatest chance of the largest (beneficial) impacts; that is, invest in a gun-enforcement portfolio that provides the best possible combination of risk and return.

Table 2 summarizes my judgments about the risks and returns to different gun-enforcement activities. Given current knowledge the most promising use of PSN funding would seem to come from targeted patrols against illegal gun carrying, which focuses on increasing the certainty rather than the severity of punishment and for which there is the best current evidence for the possibility of supernormal returns. It stands in sharp contrast to the priority that PSN placed on federal prosecution of gun-carrying cases. Perhaps more selective federalization of the highest-risk gun cases could produce supernormal returns as well, although we might expect many of these sorts of cases to already be priorities for state and local prosecutors.

A related strategy would be to focus on constructing a "mixed portfolio" of stepped-up enforcement activities that includes increased emphasis

on both targeted patrols against illegal gun-carrying and Ceasefire-style activities for gang deterrence in areas where gangs account for a large share of gun violence. The advantage of a mixed-portfolio approach is to balance the different forms of uncertainty that hold for both of these approaches. As noted, the limitations of available quantitative evaluations of Ceasefire leave us with uncertainty about this activity's net effect on gun violence, despite the conceptual appeal of this approach and encouraging qualitative findings from Sudhir Venkatesh in Chicago. Additional uncertainty is introduced by the implementation challenges of securing and sustaining cooperation from different agencies and levels of government, as the Los Angeles experience suggests. With targeted patrol, there is arguably less uncertainty about whether the program will reduce gun crime, although this uncertainty is not zero. There is more uncertainty about whether targeted patrols can be implemented in a way that produces tolerable costs to society from stop-and-frisk activity. One approach to dealing with these different dimensions of uncertainty is to hedge and invest in both activities to some degree.

As an aside, there is a particularly strong case to be made for investing in efforts to reduce these different sources of uncertainty that currently face policy makers. PSN would seem to provide a perfect opportunity to evaluate these and other possible enforcement interventions through the use of randomized experiments, given the availability of federal research funds and the fact that local agencies are typically launching new interventions de novo. One objection might be that PSN funding per USA district is too modest to support an experiment with a large enough sample size as to be informative. But powerful predictors for criminal offending at the individual or beat level are readily available, which can substantially improve the statistical power of randomized experiments in this application.[45] As far as I know, none of the PSN sites are using their research funds for randomized experimental evaluations.[46]

The basic argument made in this article is positive and optimistic. It is possible to reduce gun crime in America, and in fact, there are ways to enhance the efficiency of our current expenditures on gun enforcement.

45. Covariates such as information on past criminal involvement for people or crime rates for neighborhoods can improve statistical power by accounting for residual variation in our crime measures of interest. The explanatory power of past crime measures for future crime is considerable. For example, using beat-level data for Chicago, a regression of crime rates this year against crime rates for the past five years can predict 66% of the variation across beats in murder rates, 91% of the variation in robbery rates, and 97% of the variation in aggravated assaults.

46. Philip Cook, Jennifer Hill, Tracey Meares, and I are in discussions with the Chicago Police Department about the possibility of randomly assigning some high-crime neighborhoods of the city to receive stepped-up patrols targeted against illegal gun carrying, but whether this will come to pass remains unclear.

Even a maximally productive version of a federal enforcement program could still be "inefficient" in the sense that at existing PSN funding levels, the marginal benefits from such spending could substantially outweigh the marginal costs (see Becker, 1968). Pittsburgh's targeted patrol programs seem to produce benefits in excess of (at least quantifiable) costs at the current scale of operations, which might also be true for Boston's Operation Ceasefire, if only because that program's costs are so low. Whether it is true at the enhanced scale of operations that might be funded at existing PSN levels is not clear.[47] But given the considerable costs of gun violence to American society, there is a strong argument for at the very least continuing PSN funding at previous levels in addition to modifying the program's priorities.

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47. The Pittsburgh patrol program was remarkably inexpensive, on the order of $35,000 by our estimates, but it seems to have produced changes in gun violence that reduce social costs by millions of dollars. Because Operation Ceasefire relies primarily on reallocating existing resources, if that program is in fact as successful as some believe, then the benefit–cost ratio would be even larger than with targeted patrol. At current funding levels, PSN could distribute around $250 million per year across each of the 94 U.S. Attorney districts in the country, for an average of more than $2.5 million per site. The marginal benefits and costs of targeted patrol and Ceasefire at this larger scale of operation are currently unknown.

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