

Policy Retrospectives

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AIMING FOR EVIDENCE-BASED GUN POLICY

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INTRODUCTION

In an era when it has become fashionable to advocate that social policy be guided by evidence, social scientists should be riding high. But there is a problem: The available evidence on what works in the social-policy arena is typically something less than definitive. Experts disagree. Sometimes the only consensus that can be mustered among researchers is that “more research is required,” often a dubious assertion when there has been voluminous research already. Meanwhile, policy-makers are left free to either ignore the research evidence, or to search out an expert who supports their position.

A case in point is the recent report on gun violence of an expert panel of the National Research Council. In one topic after another, the NRC’s blue-ribbon panel concludes that the existing evidence is inconclusive (Wellford, Pepper, & Petrie, 2005).¹ It calls for an investment in better data, the invention of better

¹ Or in the NRC panel’s own words: “The committee found that answers to some of the most pressing questions cannot be addressed with existing data and research methods . . . the committee concludes that existing research studies and data include a wealth of descriptive information on homicide, suicide and firearms, but, because of the limitations of existing data and methods, do not credibly demonstrate a causal relationship between the ownership of firearms and the causes or prevention of criminal violence or suicide. . . . 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 . . . [anti-gun] policing programs are widely viewed as effective, but in fact knowledge of whether and how they reduce crime is limited” (Wellford, Pepper, & Petrie, 2005, p. 2, 6, 8, 10).

methods for extracting sound conclusions from nonexperimental evidence, and more research funding. There is essentially no guidance offered for policy development. At one level, the NRC report's nihilistic conclusion about this body of research is not surprising. Gun policy is a contentious issue, among social scientists as much as laymen. In this heated context, the NRC panel made a reasonable decision to assess the evidence according to the usual scientific standard that insists on proof beyond reasonable doubt. Their work provided a public service in pointing out that a number of claims bruited by researchers rested on thin evidentiary ice. But the panel missed an opportunity to provide guidance on how policymakers could best take advantage of available research in this area. The evidence may not be definitive, but it is far from irrelevant to making good policy. In this review essay, we offer several conclusions that we believe are defensible and relevant to policy choice.

One of our strongest conclusions is that reducing gun violence is important and deserves attention from policymakers. That statement may appear obvious, given that gun robberies and assaults victimize hundreds of thousands of people a year, resulting in 45,000 injuries and about 12,000 homicides (two-thirds of the total); a majority of suicides are committed with guns as well, with 17,000 victims a year. But these statistics do not really settle the question of policy importance. In particular, policymakers might be tempted to ignore the problem because gun violence, specifically gun assaults and homicides, is concentrated among those segments of society that do not carry much influence in the policymaking process—young, poor members of racial or ethnic minority groups, a large percentage of whom have criminal records.

A second reason for ignoring the damage that guns do is a belief that guns *per se* are not really an important aspect of lethal violence—the type of weapon used in assault or suicide is just an incidental detail, and the focus should be on the underlying problems that produce the high overall rates of violence. Yet another reason, probably the one that we have heard most frequently from colleagues and friends, is that there is nothing to be done: Restricting gun commerce, ownership, and misuse is futile when there are over 200 million guns already in private hands.

As we shall see, the evidence tends to contradict each of these reasons for doubting the importance or potential for reducing gun use in violence. This is a problem worthy of public attention. A recent estimate indicates that interpersonal gun violence was an \$80 billion problem in the mid 1990s (Cook & Ludwig, 2000; Ludwig & Cook, 2001). Gun violence rates have declined since then, and the quality of life in America has improved as a result.

What specific policies are likely to best accomplish the goal of gun-violence reduction can't be determined with confidence, but the evidence does serve to bracket the likely effectiveness of some specific interventions and, more important, provides broad guidance for policy choice based on what we have learned about the technology of violence, criminal behavior, and the functioning of relevant markets and other institutions.

Here is a sampling of conclusions that we find to be supported by the evidence:

The gun-violence problem

- Gun use intensifies violence, increasing the case-fatality rate in assaults.
- Gun violence substantially reduces the standard of living in a community in which it is common, and not just for the immediate victims.

Guns and crime

- Most robberies and assaults do not involve guns and most street criminals do not own a gun, despite the evident advantages to the criminal of using a gun.
- Weapon choice by violence perpetrators is influenced under some circumstances by both access to guns and by the criminal justice consequences of carrying and misusing a gun.
- Widespread gun ownership does not convey public benefit in the form of reduced residential burglary rates or home invasion robberies, perhaps because burglarizing a home with guns in it is more profitable. Guns are a valuable form of “loot.”

Gun markets and regulation

- Access to guns by youths and criminals is mediated by the prevalence of gun ownership in a jurisdiction.
- Interventions of modest scope intended to regulate transactions and possession tend to have modest effects (at most) on gun misuse.

Law enforcement and gun misuse

- Directed police patrol against illicit carrying has promise for reducing gun violence.
- Programs directed toward imposing long prison sentences on felons in illicit possession of guns have not generated a discernible deterrent effect.

Of course, some advocates for gun rights would assert that the evidence on gun control, however strong it may be, is not relevant to policy choice. In this deontological view, the Second Amendment provides a right to gun possession that is not to be infringed by government action, regardless of the consequences (Cook, Moore, & Braga, 2002). We will not attempt to argue the Constitutional principles involved. Here we simply note that while the Supreme Court has not ruled in recent decades on the meaning of the Second Amendment, whatever the proper interpretation of the “right” to “keep and bear arms,” it is surely not absolute: Some sort of balancing test must apply. Furthermore, no one asserts a right to use a firearm in criminal assault, and few would argue that youths and felons have the right to possess a firearm. Thus, even if some day the Supreme Court asserts that the Second Amendment does provide an individual right to gun ownership, the Constitutional door will still be open to gun-oriented enforcement and probably moderate regulation of transactions and ownership.

The next section outlines the kinds of research-based evidence that are relevant to informing the debate over policies to reduce gun violence. We then turn to a review of the evidence, beginning with a brief summary of data on gun violence in America, and then of the array of gun policies currently in effect. Next we consider the evidence pertinent to a basic question: whether more guns lead to more, or less, crime and violence. We discuss the evidence for policies that seek to keep guns away from high-risk people, rather than change the overall prevalence of gun ownership, and then consider the available evidence for demand-side regulations and enforcement targeted at criminal gun use and illegal gun carrying. We conclude with a discussion of the implications of the available evidence for future research and policy.

WHAT TYPES AND STANDARDS OF EVIDENCE ARE RELEVANT?

The call for evidence-based social policy has followed the prominent effort to promote evidence-based medicine. For laypeople it came as something of a shock when experts began advocating for this approach. This “new” school of thought made it all too clear that medical practitioners had to make most decisions about diagnostic procedures and treatment without benefit of high-quality scientific evidence: without, that is to say, double-blind, placebo-controlled experiments comparing the efficacy and risks of available alternatives (see, for example, Murnane & Nelson, 2005).

What was (and continues to be) true for medical practitioners remains all the more true for social policy: For the vast bulk of policy issues, there is little or no evidence concerning what works and what’s worthwhile that would meet this standard.² For many social policy applications we either must give up on the goal of evidence-based policy, or develop a broader conception of what counts as evidence. For example, in the area of gun policy there is no realistic prospect of a rigorous experimental evaluation of ensuring that acutely suicidal people be deprived of gun access by the relevant authorities. But then again, neither has there ever been a randomized controlled study of the efficacy of parachute use for people jumping out of planes.³ The point is that in some cases theory and nonexperimental experience are adequate guides.

There is a related matter—what might be called the “null hypothesis problem.” While the NRC report on gun violence adopts the scientific standard on what constitutes acceptable evidence regarding probability of type I error, the standard for policy-relevant evidence should, in our view, be different and based on expected values of costs and benefits for the policies being evaluated. To see the difference between these two standards of evidence, consider a simple example. A study randomly assigns adults with a prior felony record to two groups: a treatment group, which receives just a single mood-stabilizing pill at a total cost of \$5 over the entire study period, and a placebo control group. Suppose that the difference in mean arrest rates across groups implies the treatment reduces gun crime by 25% but the p-value on this point estimate is just .15, short of the conventional .05 cutoff. Any academic referee worth her salt would reject a paper submitted for scientific publication that claimed this intervention “worked.” But would that referee really want to live in a jurisdiction where this evidence persuaded policymakers that they should not adopt the new treatment, but rather stick with the *status quo*?⁴

An analogy with the contrasting standards of evidence in criminal and civil cases is relevant here. In criminal cases, the prosecution is held to a high standard because convicting a factually innocent person is viewed as so costly. In the typical

² Why randomized experiments are not used more widely in social policy is the subject of a literature of its own; see, for example, Burtless (2002), Cook and Payne (2002), and Gueron (2002). Fortunately, the use of randomized experiments in social policy evaluation is becoming somewhat more common over time (Boruch, de Moya, & Snyder, 2002; Gueron, 2003), but nevertheless it is still true that for most social policy questions policymakers must rely on nonexperimental evidence for guidance. See Rebecca Maynard’s 2005 APPAM Presidential Address for additional discussion of the use of different types of evidence in the policymaking process.

³ This is a reference to the illuminating parody, “Parachute use to prevent death and major trauma related to gravitational challenge: Systematic review of randomised controlled trials” (Smith & Pell, 2003).

⁴ In the real world, evaluation results are subject to a variety of forms of uncertainty—specification, identification, measurement, external validity—that are not so readily quantified. But our simple example serves to illustrate the general principle.

civil case, on the other hand, the relevant costs have already been incurred in the form of financial losses or injury, and the question before the court is simply who is going to pay, the plaintiff or defendant? In that case the two types of error are arguably symmetric. The decision problems facing policymakers often share this symmetry. Programs are evaluated according to their effectiveness in dealing with an ongoing problem. If a program innovation is rejected as ineffective (or not proven), then the alternative is continuation of the *status quo*. In this context, privileging the status quo is not the right basis for making policy decisions.

Of course, estimating the expected value of a program innovation is more difficult than assessing the evidence in support of the claim that it is effective. Policy analysts are trained to critique evaluation evidence, pointing out potential flaws, but are perhaps not so well prepared to judge whether the preponderance of the evidence points in one direction or another.

A bit of history about research on gun policy places this problem in context. The NRC panel was funded in large part because of the controversies surrounding the work of economist John Lott. His book *More Guns, Less Crime*, first published in 1998, was the all-time best seller for the University of Chicago Press. That and other contributions from Lott and his co-authors were widely publicized and achieved great prominence among gun-control opponents. The central theme of his work is well captured by the title of his book: Guns are effective in self-defense, and an armed public deters crime of all kinds. The corollary is that any regulation on gun commerce, possession, or use has the potential to deprive some people of the protection offered by a gun and to undercut the deterrent value. His book attempted to establish this point empirically.

Notice the logical result for policy analysts facing somewhat ambiguous evidence. If they accept as plausible that private guns may be a deterrent to crime, then that possibility becomes part of the “expected value” calculation for policy innovations. In the pre-Lott days it may have been acceptable to rule out on a priori grounds the possibility that private guns have a general deterrent effect.⁵ Then an innovation that tightens regulations on guns would be expected to have positive expected value (ignoring costs), even if the direct evidence fell short of definitive. Here’s the logic: If a gun regulation *can’t* make things worse and *may* make them better (when it comes to crime and violence), then the expected value of the effect is “better.” Of course it would remain to be determined whether things were *enough* better to justify any additional costs to the taxpayers, not to mention possible loss of freedom.

While the NRC panel report faulted Lott’s methods and conclusions, the conclusion was not to reject the basic conceptual point that private guns may serve as a deterrent, but rather to demonstrate that that notion had not been proven. That conclusion leaves the door open to the possibility of perverse effects of gun control measures, and rules out the shortcut in “signing” the expected effect of a policy innovation directed at the general public. But when it comes to policies that act directly on criminal use of guns, the assessment is easier. Even John Lott has not argued that providing violent criminals with better access to guns would reduce the crime problem.

Gang-oriented deterrence strategies, such as those employed with Boston’s widely cited Operation Ceasefire, provide a concrete example where the logic of the situa-

⁵ Lott was not the first social scientist to produce evidence of the self-defense uses of guns. Sociologist Gary Kleck had been making the case that guns were useful in self-defense for many years (Kleck, 1991, 1997). Lott took the argument a step further by arguing that there was a powerful general deterrent effect as well.

tion, and some ancillary evidence, help overcome the weakness of the direct evidence of the impact. Ceasefire was launched by a consortium of law enforcement agencies to deliver a credible threat to Boston's gangs that using guns was not going to be tolerated, and that the entire gang would suffer when any one member of the gang used a gun. The hope was to provide gang leaders with an incentive to limit gun use by the members, for fear of a police crackdown. Formal empirical evaluations of Ceasefire and its replications remain ambiguous; as the NRC report notes, "Many complex factors affect the trajectory of youth violence problems, and, while there is a strong association between the youth homicide drop and the implementation of Operation Ceasefire, it is very difficult to specify the exact role it played in the reduction of youth homicide in Boston" (Wellford et al., 2005, p. 239). Yet qualitative interviews with gang members in Chicago provide additional support for the susceptibility of gangs to this sort of police pressure (Cook, Ludwig, Venkatesh, & Braga, 2005), and other evidence highlights the importance of gangs for the gun violence problem in many cities (Klein, 1995; Decker, 2003), and the importance of income-generating activities for gangs (Levitt & Venkatesh, 2000). These results, together with the somewhat limited evaluation evidence, lead us to conclude that Ceasefire has positive expected value, particularly given that the program's resource costs are so low.

Policy-Analytic Uses of Descriptive and Conceptual Information

A focus on scientifically credible evaluation evidence excludes much policy-relevant evidence, not only because of the "null hypothesis" problem, but also because public debates over social policy choice are not limited to conflicting estimates of the likely effects of specific policy interventions. They are concerned with basic beliefs about human nature and institutions, as well as descriptions of "what is." Gun policy is a useful case in point because the advocates are so vociferous about the beliefs that influence their policy judgments. The rhetoric concerns the *conceptualization* of the underlying problem, as well as generalizations about violent and criminal behavior. A variety of evidence is relevant to assessing these beliefs, and ultimately relevant to setting a productive direction for policy.

With respect to the conceptualization of the problem, the usual measures in public health assessments have focused on the body counts: the number of people killed and injured by guns. While a useful start, this measure of the problem has the unfortunate consequence of suggesting that the "problem" is rather limited with respect to which demographic groups are affected. It is a fact that youthful minority males with criminal records are vastly overrepresented as victims of gunshot injury and homicide. We have argued that this *ex post* perspective on the problem misses much of what is costly about interpersonal gun violence: its effects on the neighborhoods and cities in which it is common (Cook & Ludwig, 2000; Ludwig & Cook, 2001).

The same can be said for other types of violence as well, up to a point. Violent crime is both a threat to individual safety and a neighborhood disamenity that has much in common with noise, graffiti, lack of access to public services, and so forth. Property values in neighborhoods with high rates of violence are depressed, as people with the means to do so choose to live and work elsewhere. Compared with other common weapons, guns have a peculiar ability to create fear, resulting in a loss of peace of mind together with self-protective distortions in routine activities of work and play. There is no counterpart with other weapons to drive-by shootings and stray bullets. And the use of guns in violent encounters greatly increases the lethality of

those attacks. Given that understanding, the gun “problem” is seen to be broadly shared, and to have impact far beyond the immediate counts of injuries and deaths.⁶

This re-conceptualization is relevant to policymakers in that it suggests the problem is larger and that the incidence of the problem is more diffuse than suggested by public health measures. Furthermore, there is a well-developed framework for assessing the *ex ante* value of increased safety and amenity levels that can be applied. In this framework, the fact that many gunshot victims have criminal records is of far less salience because they are no longer the sole objects of concern.

Persuasive evidence is also available to confront beliefs about weapon lethality, the functioning of gun markets, the demand for guns by criminals, the extent to which only “bad guys” misuse guns, and a variety of related topics. Much of the evidence is descriptive and does not require much interpretation. For example, those who oppose broad regulations on gun ownership often use a Manichean rhetoric—that the population can be neatly divided into criminals and law-abiding citizens, and that only the former misuse guns, suggesting that it is pointless or worse to limit gun possession by the “law abiding” group (Cook & Ludwig, 2002). This assertion can be tested directly if we accept as an operational definition of “criminal” someone who has been arrested or convicted of a crime. We have performed a related test using data from Illinois. We found that while adults arrested for homicide in 2001 were much more likely than the population at large to have a criminal record, a majority of Illinois homicide offenders had not been convicted of a felony (at least since 1990), which is the main criterion for legal gun possession in federal law (Cook, Ludwig, & Braga, 2005).⁷ See Table 1 for details.

In addition to conceptualizing the problem and describing gun violence, descriptive and other types of evidence are useful in developing simple models that are rel-

Table 1. Criminal record of Illinois murder arrestees and general population, 2001.

	Population Prevalence	Prevalence Among Killers
<i>Criminal record during 1990–2000</i>		
Arrest	18.2	71.6
Violent arrest	7.8	37.0
Felony conviction	3.9	42.6
<i>Criminal record during 1996–2000</i>		
Arrest	11.3	63.2
Violent arrest	4.2	25.8
Felony conviction	2.1	32.5

Source: Cook, Ludwig, and Braga (2005).

⁶ Evidence to support the view that gun involvement in crime contributes to urban flight is suggested by three facts: (1) Guns increase the chances that violent crime results in homicide, as discussed below; (2) Cullen and Levitt (1999) show that each additional reported crime in a city leads to a reduction in city population of one resident; and (3) Julie Cullen and Steve Levitt were generous enough to recalculate these estimates for the effects of homicides specifically, and find each additional homicide reduces city population by around 70.

⁷ One potential concern with these findings stems from the possibility that the Illinois data used by Cook, Ludwig, and Braga (2005) may omit some felony convictions of juveniles. While that may be true, very few juvenile arrests result in a felony conviction, because that would require waiver to adult court. More direct evidence addressing this concern comes from similar findings derived using data for Boston, where complete records on juvenile criminal justice adjudications are available (Braga, Cook, Hargarten, & Ludwig, 2006).

evant for assessing broad directions for gun policy. Evidence relevant to assessing these broad approaches includes everything from direct observations about how underground gun markets operate (Is getting a gun more like getting a credit card, or like trying to get a babysitter on New Year's Eve?), to theories and evidence concerning the deterrence process (Do criminals really respond to incentives?), as well as outcome studies on specific gun-oriented interventions. Thus the important—and feasible—task for researchers, in our view, is to provide general guidance with respect to policy directions, and to assemble and interpret the evidence in ways that may be useful to policymakers while being as transparent as possible concerning assumptions. In this respect, we note the approach taken by James Heckman and his associates in a recent review article on skill formation:

The standard approach to survey writing in empirical economics is to compile lists of facts and 'treatment effects' from various empirical studies. Instead . . . we develop a comprehensive model of the skill formation process that is grounded in the best available empirical evidence. We distill general theoretical principles that can guide skill formation policy. . . . Although economic policy analysis should be grounded in data, it is important to recognize that those policies that can be evaluated empirically are only a small subset of the policies that might be tried. (Cunha, Heckman, Lochner, & Masterov, 2005, pp. 6–7)

While we do not attempt to develop a comprehensive model of gun violence in this essay, we believe that the economists' bulwark of rational self-interest provides guidance in sorting through the evidence and drawing conclusions.

Rational Violence

Our working hypothesis is simply that criminals and violent people tend to adapt their decisions to the personal costs and benefits of the consequences as they understand them. We believe there should be a presumption that this hypothesis of (limited) rationality is correct because it is so deeply rooted in social science, not to mention common sense. Even in the case of suicide, it is reasonable to suppose that a policy that made it more difficult for those who consider it to use their preferred means of ending their lives (often, a gunshot) would cause some to desist.

The "rational violence" hypothesis requires evaluators to ask whether and how an intervention is likely to affect the incentives facing individuals at risk for violence. A common distinction is between policies that focus on deterring gun misuse through increased likelihood and severity of punishment for using a gun in criminal assault (sometimes labeled "demand"-side policies), and policies that seek to pre-empt gun misuse by regulating gun and ammunition design, transactions, possession, and carrying ("supply"-side policies). Both of these approaches, if actually effective in changing the expected punishment or the costs of acquisition and use, have the potential to cause violent people to reduce gun misuse. As we will see, there are interventions that are intended to work within this framework but fail to have much effect on gun violence, because in practice they are too weak to yield effects that are detectable with available data and evaluation techniques. In that category we would place gun buy-back campaigns, laws that restrict concealed-carrying licensing, the Brady Law's requirement of a background check for buyers, and "Project Exile"-type interventions to sentence felons in possession of guns to long prison sentences. In our review, we devote particular care to the interpretation of null results, distinguishing between cases where the intervention is weak and cases where violence appears unresponsive to that type of intervention.

Incidentally, we are uncomfortable with using the “demand” and “supply” labels in this context, and even more so with the increasingly common assertion that the “demand” approach is more promising than the “supply” approach (Wellford, 2005). The “demand” and “supply” labels do not capture the complexity of the situation. In particular, regulations of suppliers are an essential aspect of policies to discourage criminals from using guns. For example, the supply-side regulation requiring that manufacturers, distributors, and licensed dealers keep a permanent record of each transaction produces an important investigative tool for the police, who sometimes are able to trace a crime gun to its owner through these records; the effect is to increase the likelihood that gun use in crime will result in arrest and punishment. Thus, in this case, a regulation imposed on suppliers supports the “demand-side” approach. Rather than “supply” and “demand,” we prefer to designate the broad policy approaches as “reducing access to guns” and “reducing use by making guns a liability.” More concisely, in place of “supply” and “demand,” we refer to “access” and “use.” Both approaches strike us as promising, at the same time that we believe interventions of either sort should be subjected to a skeptical assessment of how much difference they will make in the relevant incentives.

GUNS AND VIOLENCE

Compared to other developed nations, the United States is unique with respect to its high rates of both gun ownership and murder (Zimring & Hawkins, 1997; Miron, 2001; Hemenway, 2004; Wellford, Pepper, & Petrie, 2005).⁸ More controversial is the nature of the causal relationship, if any. The best available evidence suggests that guns make criminal attacks more lethal, although whether there is an “instrumentality effect” for suicide attempts is less clear. Guns also provide recreational benefits, and sometimes are used virtuously in fending off or forestalling criminal attacks. The question of whether the net benefits of private gun ownership outweigh the costs, from the perspective of either the individual or society at large, is taken up later in this discussion.

Gun Ownership

America has 200 to 250 million firearms in private circulation.⁹ While there are enough guns for every adult to have one, in fact, three-quarters of all adults do not.

⁸ Homicides and other deaths are fairly well measured in most developed countries as a result of government collections of death certificate information. What we know about the gun stock in America comes from two sources—cumulative counts of annual gun production and imports, which are subject to some uncertainty about the rate at which guns deteriorate (Kleck, 1997), and population surveys, which are subject to some underreporting (Ludwig, Cook, & Smith, 1998). Nevertheless, the two data sources paint a qualitatively similar picture, and the surveys also tell us something about who owns what.

⁹ This number can be estimated through two sources of data, from federal tax records on sales and from a survey. First, the number of new guns added each year is known from data kept by the federal government on manufactures, imports, and exports. The annual count of net additions can be cumulated over, say, the last century, with some assumption about the rate of removal through such mechanisms as off-the-books exports, breakage, and police confiscation (Cook, 1991; Kleck, 1997). The alternative basis for estimating the stock is the one-time National Survey of the Personal Ownership of Firearms (NSPOF), conducted in 1994; this is the only survey that attempted to determine the number of guns in private hands. (A number of surveys, including the General Social Survey, provide an estimate of the prevalence of gun ownership among individuals and households without attempting to determine the average number of guns per gun owner.) The NSPOF estimate for the number of guns in 1994 was 192 million, a number that is compatible with the “sales accumulation” method, assuming that just 15 percent of the new guns sold since 1899 had been thrown out or destroyed (Cook & Ludwig, 1996). Since the survey, the annual rate of net additions to the gun stock has been about 4–5 million per year (ATF 2001, 2002), or 50–60 million by 2006. Given a continued removal rate of just one percent, the stock as of 2006 would be around 220 million.

Recent survey data suggest that about 40% of males, 10% of females, and one-third of all households have at least one gun. The household prevalence of gun ownership has been in long-term decline, in part because household composition is changing—becoming smaller, and less likely to include an adult male. The upshot is that gun ownership is very concentrated. Most people who own one gun own many. In 1994, three-quarters of all guns were owned by those who owned four or more, amounting to just 10 percent of adults (Cook & Ludwig, 1996).

Around one-third of America's privately held firearms are handguns, which are more likely than long guns to be kept for defense against crime (Cook & Ludwig, 1996). In the 1970s, one-third of new guns were handguns (pistols or revolvers), a figure that grew to nearly half by the early 1990s and then fell back to around 40 percent (ATF, 2000a). Despite the long-term increase in the relative importance of handgun sales, a mere 20 percent of gun-owning individuals have only handguns; 44 percent have both handguns and long guns, reflecting the fact that most people who have acquired guns for self-protection are also hunters and target shooters. Less than half of gun owners say that their primary motivation for having a gun is self-protection against crime.

Given the importance of hunting and sport shooting, it is not surprising that gun ownership is concentrated in rural areas and small towns, and among middle-aged, middle-income households (Cook & Ludwig, 1996). These attributes are associated with relatively low involvement in criminal violence, and it is reasonable to suppose that most guns are in the hands of people who are unlikely to misuse them. On the other hand, gun owners are more likely than other adults to have a criminal record (Cook & Ludwig, 1996).

The majority of guns in circulation were obtained by their owners directly from a federally licensed firearm dealer (FFL). However, the 30 to 40 percent of all gun transfers that do not involve licensed dealers, the so-called "secondary market" (Cook, Molliconi, & Cole, 1995), accounts for most guns used in crime (see Wright & Rossi, 1994; Sheley & Wright, 1995; Cook & Braga, 2001). Despite the prominence of gun shows in current policy debates, the best available evidence suggests that such shows account for only a small share of all secondary market sales (Cook & Ludwig, 1996). Another important source of crime guns is theft—over 500,000 guns are stolen each year (Cook & Ludwig, 1996; Kleck 1997).

Gun Use

Including homicide, suicide, and accident, 30,136 Americans died by gunfire in 2003, a mortality rate of 10.4 deaths per 100,000 people.¹⁰ This figure is down substantially from 1990 (14.9 per 100,000), but is still much higher than what was observed in the U.S. in, say, 1960 (Cook & Ludwig, 2000). Intentional violence is the major exception to the secular decline in injury deaths during the last 50 years (Cook & Ludwig, 2000). More Americans die each year by gun suicide than gun homicide. However, more people suffer nonfatal gun injuries from crime than from suicide attempts; the case fatality rate for gun suicide is much higher than for gunshot wounds from criminal assaults. Several hundred people a year die in gun accidents, a figure that is heavily influenced by coroners' standards concerning what constitutes an accident as opposed to a homicide or suicide.

Even though everyone shares in the costs of gun violence, the shooters and victims themselves are not a representative slice of the population. The gun-homicide-

¹⁰ <http://webappa.cdc.gov/sasweb/ncipc/mortrate.html>, accessed February 1, 2006.

victimization rate in 2003 for Hispanic men ages 18 to 29 was six times the rate for non-Hispanic white men of the same age; the gun homicide rate for black men 18 to 29 was 100 per 100,000, 23 times the rate for white males in that age group.¹¹ There appears to be considerable overlap between the populations of potential offenders and victims: The large majority of both groups have prior criminal records.¹²

The demographics of gun suicide look somewhat different: While suicides and homicides both occur disproportionately to those with low incomes or educational attainment, gun suicides are more common among whites than blacks, and more common among the old than among young or middle-aged adults (Cook & Ludwig, 2000). Men are vastly overrepresented in all categories.

The costs of gun violence to society are more evenly distributed across the population than victimization statistics would suggest. The threat of being shot causes private citizens and public institutions to undertake a variety of costly measures to reduce this risk, and all of us must live with the anxiety caused by the lingering chance that we or a loved one could be shot. As one local district attorney notes, “Gun violence is what makes people afraid to go to the corner store at night” (Kalil, 2002). While quantifying the magnitude of these social costs is difficult, contingent valuation (CV) survey estimates suggest the costs of gun violence may be on the order of \$100 billion per year (Cook & Ludwig, 2000). Most (\$80 billion) of these costs come from crime-related gun violence. Dividing by the annual number of crime-related gunshot wounds, including homicides, implies a social cost per crime-related gun injury of around \$1 million (Ludwig & Cook, 2001).¹³

Instrumentality

Since both guns and homicides are unusually common in the U.S. compared to other developed nations, it is natural to wonder whether the two phenomena are linked. In the 1950s and 1960s, criminologists generally ignored the issue of weapon choice as a determinant of homicide, preferring to focus on more “fundamental” issues. One exception was Marvin Wolfgang (Wolfgang, 1958), although he argued that the gun itself had little effect on the outcome of a violent encounter—a judgment that he later retracted (Wolfgang, 1995).

In two seminal articles, Franklin Zimring (1968, 1972) provided systematic evidence that the weapon type matters independent of motivation. Zimring drew on crime data from Chicago to show that case-fatality rates in gun attacks are a multiple of those in knife attacks, despite the fact that the circumstances are generally quite similar. Many criminal assailants were inebriated at the time of the crime, and thus unlikely to be acting in a calculating fashion, and few attackers administered

¹¹ <http://webappa.cdc.gov/sasweb/ncipc/mortrate.html>, accessed February 1, 2006.

¹² See Kennedy, Piehl, and Braga (1996); McGonigal et al. (1993); Kates and Polsby (2000); Cook, Ludwig and Braga (2005).

¹³ Note that this estimate is intended to capture the 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 figure comes, in part, from CV responses about what people say they would pay to reduce crime-related gun violence by 30%. One potential concern is that these estimates assume that societal willingness to pay to reduce gun violence is proportional to the amount of gun violence eliminated, which may not be the case. And in practice there remains some uncertainty about the reliability of the CV measurement technology. In any case, most of the estimated costs of gun violence to the U.S. appear to come from crime, since suicide seems more like a private concern, and the estimated costs of gun crime by Cook and Ludwig (2000) fits comfortably next to more recent CV estimates for the social costs of crime more generally (Cohen, Rust, Steen, & Tidd, 2004).

more than one or two wounds to the victim, even in fatal cases. In serious attacks, he concluded, the difference between whether the victim lived or died was typically a matter of chance rather than a difference in intent, and the chances were higher with a gun than a knife. Zimring (1972) found further confirmation in comparing the case-fatality rates among shootings involving guns of different caliber. He demonstrated that victims were more likely to die in larger-caliber shootings, again suggesting that the intrinsic lethality of the weapon affected the outcome (Cook, 1991). Similar results hold for robbery: Using a gun instead of a knife to threaten someone appears to greatly increase the likelihood that the victim will be killed, despite the fact that gun use reduces the likelihood of victim resistance or injury (Cook, 1976, 1980, 1987).

The results on instrumentality are in line with our working hypothesis of rational violence. Compared with other common weapons, guns provide a means of inflicting a fatal wound quickly, from a distance, with little personal risk, determination, involvement, or strength required. Gun use in an assault increases the likelihood of death by making it easier to kill.¹⁴

The notion that guns make crime more lethal seems to be fairly well accepted now on all sides of the gun control debate (see, for example, Kleck, 1997, p. 227), and even among the general public. As Ozzy Osbourne argued in a *New York Times* interview, "I keep hearing this thing that guns don't kill people, but people kill people. If that's the case, then why do we give people guns when they go to war? Why not just send the people?"¹⁵

The direct evidence on whether guns exert an instrumentality effect on suicide is less strong than for criminal assaults. In suicide, unlike assault, there are other highly lethal means available to anyone who takes the time to plan, including hanging and jumping from a high building or bridge. Of course not all suicide attempts are the result of sustained intent: Youths, especially, may be subject to fits of despair resulting from transitory events.¹⁶

Self-Defense and Deterrence

The same features of guns that make them valuable to criminals may also make guns useful in self-defense. Just how often guns are used in defense against criminal attack has been hotly debated, and remains unclear. Estimates from the National Crime Victimization Survey (NCVS), a large government-sponsored in-person survey that is generally considered the most reliable source of information on predatory crime, suggest that guns are used in defense against criminal predation around 100,000 times per year (Cook, Ludwig, & Hemenway, 1997). In contrast are the results of several smaller one-time telephone surveys, which provide a basis for asserting that there are millions of defensive gun uses per year (Kleck & Gertz, 1995).

Why do these estimates for the number of defensive gun uses each year differ by more than an order of magnitude? One explanation is that the NCVS only asks questions about defensive gun use to those who report a victimization attempt, while the phone surveys ask such questions of every respondent. As a result, the scope for "false positives" will be much greater with the phone surveys compared to the NCVS

¹⁴ The NRC report does not directly take on the issue of whether guns make criminal attacks more lethal, other than to claim that "the issue of substitution (of the means of committing homicide or suicide) has been almost entirely ignored in the literature" (Wellford, Pepper, & Petrie, 2005, p. 6), a puzzling claim given that the seminal study in this area by Zimring compares outcomes for gun versus knife assaults.

¹⁵ Reene Jana (1998), "Questions for Ozzy Osbourne." *New York Times Magazine*. June 28, 1998. p. 8.

¹⁶ For more detailed reviews of this literature see Miller and Hemenway (1999), and Chapter 7 of Wellford, Pepper, and Petrie (2005).

(Cook, Ludwig, & Hemenway, 1997; Hemenway, 1997a,b). Moreover, as the NRC report notes, “fundamental problems in defining what is meant by defensive gun use may be a primary impediment to accurate measurement” (Wellford, Pepper, & Petrie, 2005, p. 103; see also McDowall, Loftin, & Presser, 2000). When respondents who report a defensive gun use are asked to describe the sequence of events, many of the cases turn out to have involved something far less threatening than one might suppose (Hemenway, 2004).

Whatever the actual number of defensive gun uses, the mere threat of encountering an armed victim may exert a deterrent effect on the behavior of criminals. A growing body of research within criminology and economics supports the notion that some criminals are sensitive to the threat of punishment (Cook, 1980; Nagin, 1998; Levitt, 2001). It is therefore not surprising that the threat of armed victim response may also figure in criminal decision: Around 40 percent of prisoners in one survey indicated that they had decided against committing a crime at least once because they feared that the potential victim was carrying a gun (Wright & Rossi, 1994). Whether that type of consideration actually affects crime rates is another matter, to which we return below.

Whatever the evidence, millions of households choose to keep a gun for self-defense. Many more keep guns for sporting purposes—hunting, target shooting, and collecting. The goal of gun policy in the U.S. has been to preserve these traditional uses of guns for most of the adult population, while reducing access and use by the highest-risk groups. Whether the current system achieves the proper balance between preserving access and preventing misuse remains, of course, the subject of considerable debate.

POLICY RESPONSE

Federal law affords most people access to most types of guns; the law is permissive but with delineated exceptions, specifying certain categories of people who are prohibited from possession, and certain categories of guns that are banned or tightly regulated. Federal law also establishes a licensing system for gun dealers, and regulates transactions and record keeping by these dealers. States and localities may supplement federal regulations on firearms commerce and use. In some cases, state laws supplement the federal restrictions regarding “who” and “what” is prohibited, or impose additional requirements on transactions. Almost all states regulate gun carrying more closely than guns in the home, and also specify penalties for misuse. Federal regulations on gun commerce are intended to help insulate states with more stringent regulations from those with lax regulation.

Gun Design

Efforts to regulate gun design began in earnest with the National Firearms Act of 1934 (NFA), which required the registration of hand grenades, machine guns, and sawed-off shotguns and imposed a confiscatory tax on transactions involving these weapons. The goal of the NFA was to regulate strictly a class of weapons of particular value to criminals but with little value for hunting or other sporting uses. Existing regulations of gun design are also targeted at the other end of the weapons market: The Gun Control Act of 1968 banned the importation of cheap, easily concealed handguns (“Saturday night specials”), and some states have banned such handguns altogether (Webster, Vernick, & Hepburn, 2002).

Even minor design regulations can generate political heat out of all proportion to the public interests at stake, as suggested by the recent expiration of the 1994 fed-

eral ban on new military-style “assault” weapons and large-capacity magazines. The law was, from the start, unlikely to have much in the way of either benefits or costs because many functional equivalents of the banned weapons were left on the market.¹⁷ Another example comes from recent design proposals focused on reducing gun accidents, which account for just a small share of all gun deaths, by adding new safety features to handguns, including mechanisms to indicate whether the weapon is loaded (see Vernick & Teret, 2000; Vernick, Meisel, Teret, Milne, & Hargarten, 1999)—design features that are unlikely to impose much cost on owners.

More sweeping proposals to change the design of firearms call for “personalized guns,” which prevent the weapon from being fired by someone other than the owner by means of a lock that is controlled by a standard key, a magnetic ring worn by the shooter, or more advanced biometric methods. Each of these personalization schemes would help prevent accidental discharges or suicides by unauthorized users, and could make the guns inoperable if they were stolen. Technologies such as fingerprint recognition that require specialized equipment to transfer the weapon from one person to another would have the additional effect of facilitating regulation of voluntary exchanges in the secondary market (Cook & Leitzel, 2002). It is interesting that personalized guns have come under attack from both sides in the gun control debate. In any event, the federal government is continuing to invest in developing new safety devices of this sort.

Gun Transactions

In most parts of the U.S., almost anyone can legally buy a handgun or long gun, except for those prohibited from acquiring firearms by the Gun Control Act (GCA) of 1968: minors; adults under indictment or having any prior felony conviction, or (due to a 1996 amendment) misdemeanor conviction for domestic violence; illegal aliens; those confined by court order due to mental illness; and a few other categories. These basic restrictions enjoy almost unanimous support in debates about gun policy. More controversial is what government should do to keep guns away from these high-risk categories people.

The GCA stipulates that anyone in the business of selling guns obtain a federal license from the Bureau of Alcohol, Tobacco, Firearms, and Explosives. Only licensed dealers can receive mail-order shipments of guns. Licensed dealers must require buyers to show identification and complete a form attesting that they are eligible to obtain a firearm. Information on the sale must be kept permanently on file. A number of states have stipulated additional requirements for a legal sale of a handgun, including a requirement of a criminal record check on potential buyers. In 1994, background checks in handgun sales by dealers became mandatory in all states as a result of the federal Brady Handgun Violence Prevention Act, a requirement that was extended to long-gun sales in 1998.

More lax are the requirements for gun sales by people who are not licensed dealers—defined by the 1968 GCA as anyone who is not “engaged in the business of selling firearms at wholesale or retail . . . engaged in the business of repairing firearms or of making or fitting special barrels, stocks, or trigger mechanisms to firearms,” or a pawnbroker: Non-dealers are prohibited from *knowingly* selling a gun to some-

¹⁷ The 1994 law’s prohibition on large-capacity magazines (holding more than 10 rounds) is perhaps more important because the damage that a shooter can inflict may be related to the number of rounds that can be fired before reloading. The very limited evidence that is currently available suggests that only a small share of all gun assaults involve more than 10 shots fired (Koper, 2004).

one banned from possession, but are not required to determine the buyer's eligibility or follow other paperwork reporting requirements. The exemption of sales by non-dealers from most existing federal regulations is a huge loophole in the federal regulatory system.

States or localities may go beyond the federal regulations on gun transactions. Washington, D.C., Chicago, and a handful of other cities have all but banned handguns, while Massachusetts, New York City, and some other jurisdictions have highly restrictive regulations that stop short of a ban. Other states have imposed licensing and registration systems to help law enforcement solve crimes and to help regulate secondary market transfers. For example, in Illinois all gun owners are required to obtain a Firearm Owners Identification (FOID) card. Gun owners are required to report thefts to the police, and are only allowed to resell their guns to those with a FOID. An owner whose weapon turns up at a crime scene is at risk for being visited by the police and held legally liable if the gun was transferred inappropriately to an ineligible buyer. The Illinois system thus provides gun owners with an incentive to verify a buyer's eligibility status, and to resist requests to serve as a straw purchaser for friends and family who are ineligible.

Most states, however, have chosen not to expand federal regulations on gun transactions or possession, and have pre-empted localities from doing so. States with lax controls serve as an attractive source for gun traffickers who seek to supply the black markets in tight-control states. The 1968 GCA was intended to insulate states from one another by prohibiting interstate transfers of handguns or long guns except to licensed gun dealers. Before 1994, however, trafficking of this sort had been an important source of guns to criminals in tight-control states; the Brady requirements appear to have reduced this type of interstate "arbitrage" (see Cook & Braga, 2001; Webster, Vernick, & Hepburn, 2001).

Justification for any of the regulations on gun design or acquisition rests in part on beliefs about their consequences. One fundamental assumption crucial to a variety of regulations concerns whether reducing the number of guns in private hands would lead to more or less violence and crime.

THE PREVALENCE OF GUN OWNERSHIP

If guns are more lethal than other means of violence, then keeping guns away from those at high risk of violence may save lives. The number of guns in circulation is then of direct policy interest, because more guns in private hands may increase availability to violent criminals through theft or voluntary transfers in secondary markets. Of course, an increase in the prevalence of guns could also serve as a deterrent to robbery, assault, and burglary.

What is the net effect of more guns on the volume of crime and violence? The weight of the available evidence points in the direction of more guns, more homicides. Gun prevalence has a more or less neutral effect on other common crimes, including assault and robbery; as a result, it appears that guns do not affect the overall volume of violence, but do affect its intensity. The effect of gun prevalence on suicide rates is less clear.

Gun Availability and Violent Crime

Perhaps the question of primary interest to individual citizens is whether guns make the owners and members of their household more or less safe. Several studies have demonstrated that a gun in the home is far more likely to end up being used

to kill a member of the household (including suicide) than to kill or injure an intruder (Hemenway, 2004, Ch. 5). But that comparison is not exactly on point: The number of intruders who are shot understates the total number of instances in which an intruder is repelled or scared off.

If guns in the home are dangerous to its occupants on balance, then we would predict that people who are victimized in their homes would be more likely to have a gun than non-victims, other things equal. This prediction has been tested by use of case-control studies that compare gun ownership rates of homicide victims with those of neighbors who share similar sociodemographic characteristics (Kellermann et al., 1993). The victims are indeed more likely to own a gun. But it's not clear that these studies have really controlled for other relevant factors. The decision to keep a gun is confounded in some way with other hard-to-measure individual characteristics associated with the risk of homicide victimization (such as whether the ultimate victim was being threatened by someone). Another problem is that the indicators of gun ownership used in these studies (reports by neighbors or others) may be confounded by the homicide or suicide.

A more subtle concern with case-control studies is that they ignore the possibility that individual gun ownership affects other people in the community. These external effects could be salutary, if widespread gun ownership deters criminals; or negative, if widespread ownership facilitates diversion to criminal use through theft and secondary sales. Hence it is important to assess the effects of overall rates of gun ownership within a community.

One way to learn about the effects of community gun prevalence on crime is to compare crime rates across jurisdictions that have different rates of gun ownership. Because there are no administrative data on gun ownership rates, small-area estimates must utilize a proxy. The best generally available proxy for gun prevalence is the fraction of suicides that involve a firearm (FSS), which is highly correlated with survey-based measures of gun ownership rates in cross-section data (at both the state and county level), and also tracks movements over time at the regional and state levels (Azrael, Cook, & Miller, 2004; Kleck, 2004; Cook & Ludwig, 2006).

The suitability of the FSS proxy depends in part on the specific application. For example, FSS is problematic in studying how guns affect suicide because the two measures will have a built-in negative correlation (Wellford, Pepper, & Petrie, 2005; see also Duggan, 2003). But overall, FSS appears to be a good proxy for gun ownership, perhaps better than direct survey-based estimates. In fact, the correlations between FSS and survey-based gun-prevalence measures are so high that they are compatible with a belief that FSS is *exact* (Cook & Ludwig, 2006).

Several studies report a strong positive correlation between the FSS proxy and homicide rates across counties (Cook & Ludwig, 2002; Miller, Azrael, & Hemenway, 2002).¹⁸ However, the fundamental problem with cross-sectional studies is that gun-rich jurisdictions, such as Mississippi, are systematically different in various ways from jurisdictions with relatively few guns, such as Massachusetts. The usual approach for addressing this “apples and oranges” problem has been to statistically

¹⁸ Kleck and Patterson (1993) use a similar proxy with city-level data and find no statistically significant cross-section relationship between gun ownership rates and homicide or other crime rates. However, rather than relying on a simple cross-section regression-adjusted comparison of crime rates across areas with different rates of gun ownership, they attempt to isolate variation in gun ownership rates that will be arguably unrelated to the unmeasured determinants of local crime rates. Their choice of “instrumental variable” to explain variation in gun prevalence—per capita rates of hunting licenses and subscriptions to gun magazines—is likely to be biased in the direction of overstating the net deterrent effect of guns on crime (see, for example, the discussion in Cook & Ludwig, 2003b).

control for the handful of local-area characteristics that are readily available in standard data sources, such as population density, poverty, and the age and racial composition of the population. But these variables never explain very much of the cross-sectional variation in crime rates (Glaeser, Sacerdote, & Scheinkman, 1996), suggesting that the list of control variables is inadequate to the task. Also unclear is whether widespread gun ownership is cause or effect of an area's crime problem, since high crime rates may induce residents to buy guns for self-protection. These same concerns are arguably even more severe with cross-sectional comparisons across countries.

Some of the problems with cross-section studies can be overcome by using panel data—repeated cross-sections of city, county, or state data measured at multiple points in time—to compare *changes* in gun ownership with *changes* in crime. Compared with Massachusetts, the state of Mississippi may have much higher homicide rates year after year for reasons that cannot be fully explained by standard socio-demographic or other variables. But by comparing changes across areas we implicitly control for any unmeasured differences across areas that are relatively fixed over time, such as a “Southern culture of violence” (see Butterfield, 1996; Loftin, McDowall, Wiersema, & Cottey, 1991). The reverse causation problem, in which crime may be both cause and effect of gun ownership, can be at least partially addressed within this “fixed effects” framework by relating changes in gun ownership *this* year with changes in crime rates *next* year.

The best available panel-data evidence suggests that more guns lead to more homicides. Mark Duggan's paper in the *Journal of Political Economy* (2001) identifies the relationship between guns and crime using over-time variation in panels of states and also counties. Duggan finds that a 10 percent increase in gun prevalence in one year increases a county or state's homicide rate the next year by around 2 percent (that is, the elasticity of homicide with respect to his gun proxy is around +0.2), but gun prevalence has little effect on other types of crime.

In a related study, we (Cook & Ludwig, 2006) use the same sort of across-county over-time variation in gun prevalence exploited by Duggan, but employ a more valid measure for household gun ownership (FSS) and extend the basic panel-data analysis in a variety of ways. It turns out that our estimates are quite similar to those from Duggan: The elasticity of homicide with respect to their gun proxy is on the order of +0.3 after correcting for measurement error with the proxy. All of this association is driven by a relationship between FSS and homicides committed with firearms, and there is little association of gun prevalence with other types of crimes.

The primary concern with these estimates is that neither Duggan (2001) nor Cook and Ludwig (2006) have a good “natural experiment” that generates variation in gun prevalence that is plausibly unrelated to other factors relevant for determining homicide outcomes. Put differently, why do some areas experience increases in household gun ownership while other areas experience declines in gun prevalence, and are these trends driven by omitted variables that also affect homicide? While there is no definitive answer to this question, we note that at least part of the variation across counties over time in household gun ownership rates seems to be due to a general convergence in gun prevalence across regions of the country.¹⁹ Esti-

¹⁹ This regional convergence in gun ownership rates could be due in part to regional convergence in the prevalence of agricultural employment and wages (Caselli & Coleman, 2001), since America's rural hunting tradition seems to be an important explanation for current gun ownership patterns (Cook & Ludwig, 1996). Whether this possible source of regional convergence would be orthogonal to other determinants of crime conditional on gun prevalence is not clear; although as Levitt (2004) notes, previous evidence on the effects of wages or income on crime suggests a modest relationship for property crime and little relationship for violent crime.

mates that use just the variation in gun prevalence attributable to this general inter-regional convergence again imply an elasticity of around +0.2.

We should note that recent empirical estimates are not unanimous on this point: John Lott (2000) estimates the elasticity of homicide with respect to gun ownership rates to be equal to -3.3, of the opposite sign and about an order of magnitude larger than the estimates reported by Duggan (2001) or Cook and Ludwig (2006). We put more weight on the latter two studies because of problems with Lott's data and methods. One problem with Lott's analysis is that by pooling state-level data for 1988 and 1996 and controlling for region rather than state fixed effects, his estimates will be identified primarily by interstate variation in gun ownership rates (Azrael, Cook, & Miller, 2004; Kleck, 2004). A more fundamental problem is that there are serious problems with the voter exit poll data that Lott uses to estimate state-level gun ownership. Voters are by no means a representative sample, and the voting "sample" changes from election to election. That may explain why Lott's data indicate that from 1988 to 1996 gun ownership rates increased for the U.S. as a whole from 27.4 to 37.0 percent (p. 36). Yet the best source of national data on gun ownership trends, the General Social Survey, indicates that individual gun ownership trends were essentially flat during this period (Kleck, 1997, pp. 98-99).

In the gun policy debate, a related claim about the benefit of widespread gun ownership is that it serves to deter burglars, and especially "hot" burglaries of occupied homes (Kleck, 1997; Kopel, 2001). This claim is based on little evidence of any kind. The only systematic analysis of this point of which we are aware (Cook & Ludwig, 2003b) demonstrates by use of the geocoded National Crime Victimization Survey data that the individual likelihood of residential burglary or hot burglary is not reduced by living in a county with high gun prevalence. One reason may be that a high gun prevalence increases the profitability of burglary, because stolen guns are readily fenced and easier to carry than televisions and many other types of loot.

These results taken together accord with the belief that guns do not contribute much to the overall volume of crime, and they do make violent crime more lethal (Zimring & Hawkins, 1997). Note that our conclusion here is at odds with the one advanced by Michael Moore in his widely cited documentary *Bowling for Columbine*.²⁰ Moore argues that Canada is like the U.S. in having lots of guns, yet it has a far lower homicide rate, suggesting that guns *per se* are not the problem. As it turns out, however, gun ownership in Canada is only about half as common as in the United States, and handgun ownership still rarer (Block, 1998). More important, other aspects of Canadian society lead that country to have a lower rate of violent crime than the United States. It is the *combination* of widespread gun ownership with a high rate of violence in the United States that produces such deadly results. Canada has less of both dimensions.

Gun Availability and Suicide

Do guns also increase the lethality or frequency of suicide attempts? The research on this point is less strong than for homicides, but seems to point—albeit tentatively—in the direction of more guns, more completed suicides. Much of the evidence here comes from case-control studies (see Miller & Hemenway, 1999; and Chapter 7 of Wellford et al., 2005), the limitations of which are men-

²⁰ *Bowling for Columbine*, United Artists / Alliance Atlantis, 2002.

tioned above. The best available study to date of how gun prevalence affects suicide completion rates is by Duggan (2003), using gun magazine subscription rates as a proxy for gun prevalence. Duggan presents cross-sectional evidence that exploits the fact that men are far more likely than women to own guns (Cook & Ludwig, 1996; Kleck, 1997) and to use a gun in a suicide attempt (Cook & Ludwig, 2000). He finds that states with relatively high gun ownership rates also have a higher ratio of male-to-female suicides compared with states with fewer guns. These findings are consistent with the idea that guns increase the lethality of suicide attempts. Duggan also presents panel-data estimates that compare variation across counties or states in gun ownership rates, yielding estimates for the effects of guns on suicide that are positive but much smaller than for homicide, and not statistically significant.

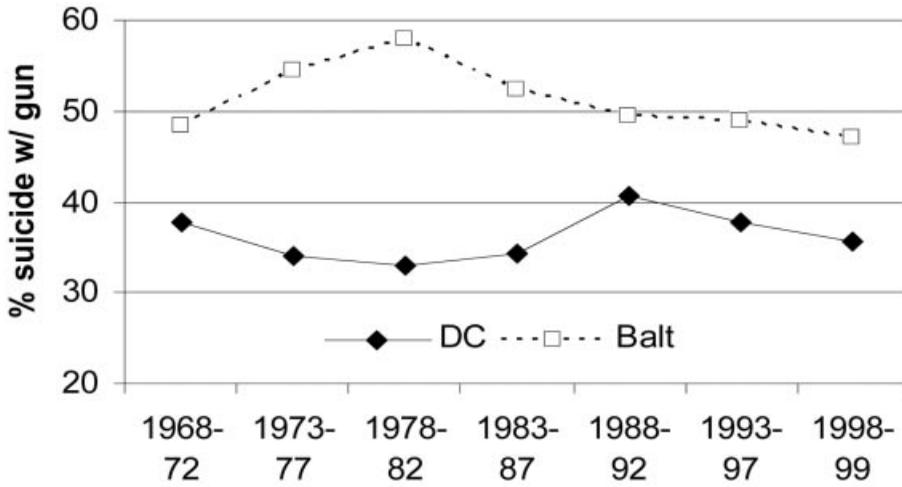
Policy Experiments

An alternative approach for learning about the effects of gun availability on public health and safety is to examine the effects of policy changes that influence overall gun ownership rates. While these policy experiments have commanded a great deal of public attention, they are not very informative about the effects of widespread gun availability on violence, primarily because even outright bans on handguns have surprisingly modest effects on gun ownership rates.

One widely cited policy change is Washington, D.C.'s 1976 ban on handgun acquisitions. By the late 1980s the notion that Washington's handgun ban had achieved anything useful seemed unlikely, given common references to the city as the "Homicide Capital of the World." Nevertheless the available data do suggest that homicides and suicides declined by around 25 percent around the time of the District's handgun ban, led by reductions in homicides and suicides with guns (Loftin et al., 1991)—before the violent tsunami caused by the introduction of crack cocaine.

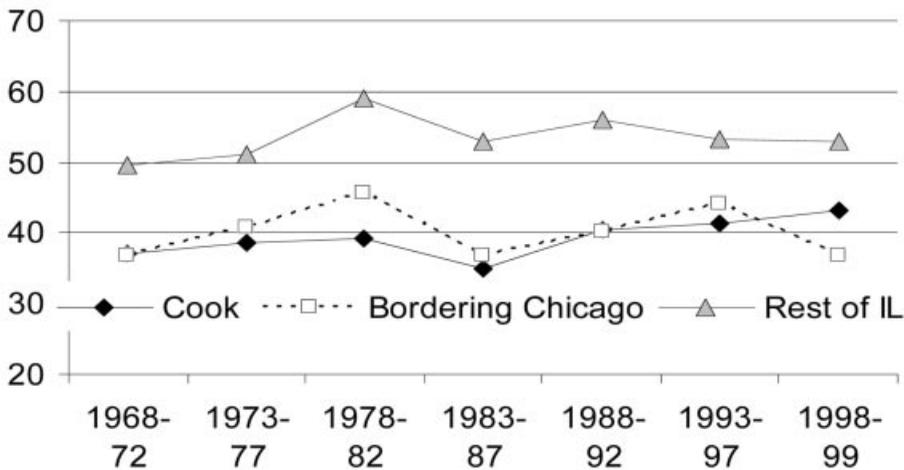
Still controversial is the question of how much of this decline can be attributed to the handgun ban rather than to other factors. In an influential article published in the *New England Journal of Medicine*, criminologist Colin Loftin and his colleagues showed that homicides and suicides declined in Washington, and by more than in the city's Maryland and Virginia suburbs (Loftin et al., 1991). A challenge to the use of affluent suburbs as a control group for the city (Britt, Kleck, & Bordua, 1996) led to additional research using Baltimore data. Like D.C., Baltimore also experienced a decline in firearm homicides around 1976. But unlike Washington, Baltimore experienced a reduction in non-gun as well as gun homicides, suggesting some general change in Baltimore around this time that was not specific to guns. Further, Baltimore did not experience a decline in gun suicides (McDowall, Loftin, & Wiersema, 1996).

Further complicating the interpretation of this evidence is that D.C. did not seem to experience a decline in overall household gun ownership rates (as proxied by the fraction of suicides committed with guns, or FSS), either relative to the city's pre-1976 levels or compared to the trend over this period in Baltimore (Figure 1). Similarly in 1982 Chicago essentially banned private ownership of guns, with a grandfather exception enabling those already in possession of handguns to register them with the city. Figure 2 shows that FSS declined somewhat in Cook County (which is dominated by Chicago) briefly after the 1982 ban was enacted, but then reverted back to pre-ban levels (see also Cook & Ludwig, 2003c).



Notes: Chart presents 5-year averages for percent suicides with guns, a proxy for household gun ownership rates (see text).

Figure 1. Percent suicides with guns, Washington, D.C., and Baltimore, MD.



Notes: Figure presents 5-year averages of percent suicides with guns, a proxy for household gun ownership rates (see text). Counties bordering Chicago are DuPage, Lake, McHenry, and Will. Missing data in VS for IL counties for 1992.

Data taken from CDC Wonder for Residents Only; rates are possibly slightly higher.

Figure 2. Percent suicides with guns, Cook County and rest of Illinois.

Gun “buyback” programs may seem to offer another opportunity to learn more about the effects of gun prevalence on crime. In practice, American buyback programs have had little effect on prevalence because they are brief and voluntary, and leave open the possibility of owners buying new guns to replace those they turn in.²¹ Further, the sellers in these buyback programs have been shown to be people at low risk for criminal offending, and the guns that are turned in are often broken or quite different from those that are used in crime.

In contrast, the buyback program launched by Australia in 1996 had sustained, compulsory elements that provided every chance for success. Following a rampage shooting in Tasmania that killed 35 people, the national government banned semi-automatic weapons (with certain exceptions). Owners were compelled to turn in their weapons but were offered compensation during the first few months of the program. In the event, 650,000 guns were turned in and destroyed. The gun homicide count declined but, given the small numbers involved (on the order of 50 homicides per year for the entire nation), it is not possible to reach a confident conclusion about the effectiveness of the program (Reuter & Mouzos, 2003).

More Guns, More Violence

Taken together, this research suggests that within the generally gun-rich context of the United States, higher gun prevalence is associated with more homicides while having little effect on other types of crime. The evidence is weaker for suicide but points tentatively in the direction of more guns, more suicide. It is reasonable to note that anything short of a true randomized experiment leaves some room for doubt about the causal interpretation of such findings, since whatever causes people and jurisdictions to have different rates of gun ownership may also affect their involvement with crime and violence. Unfortunately, local policy shifts that might provide useful “natural experiments” for research are not very informative about the effects of gun prevalence on violence because even outright bans on private ownership have surprisingly little effect on gun prevalence. But some qualified support for panel-data findings of more guns, more homicide, comes from the fact that gun prevalence is not associated with other types of violent crime, which would seem to rule out at least the most obvious types of confounding from general omitted criminogenic factors.

Summary: Prevalence of Gun Ownership

Treatment	Effect of Treatment on Access to Guns by Violent People (Output)	Effect of Treatment on Homicide Rates (Outcome)	Confidence in This Conclusion
Handgun bans in some cities	Small	Small	Medium
Occasional local gun buyback programs	Trivial	Trivial	High
Natural variation in ownership prevalence	Medium	Medium	Medium

²¹ Callahan, Rivara, and Koepsell (1994); Rosenfeld (1996); Romero, Wintemute, and Vernick (1998). Gun buyback programs also face conceptual challenges. For example, if the trade-in price is set too low, no one will participate. But a sufficiently high price can increase overall gun ownership by reducing the cost to owners whose need for a gun is of temporary or uncertain duration (Mullin, 2001).

GUN TRANSACTIONS AND POSSESSION

In practice, most “supply-side” regulations in the United States are not intended to have much effect on the overall prevalence of guns, but rather to reduce criminal and reckless use of guns by banning possession by certain groups, such as youths and felons. Fortunately, an effective program to deny guns to those likely to misuse them does not require a house-to-house search; it would be enough to regulate transactions effectively. The reason is that criminal misuse usually follows rather quickly after gun acquisition. In other words, the millions of current gun possessors will account for little of the violent crime five years from now. A reasonable goal, then, is to increase the effective price of guns to the high-risk segment of the market.

The most important federal firearm law since 1968, the Brady Act, provided a natural experiment that lent itself to evaluation using a strong design. The result, which we believe is reliable, is that the effect of the Brady Act on gun homicide is not discernibly different (in a statistical sense) from zero (Ludwig & Cook, 2000). Based on this evaluation, we can be confident that if there was an effect, it was only to reduce (or increase) the homicide rate by a few percentage points. A larger effect would not be expected in any case, because the law left untouched a massive loophole, the largely unregulated secondary market. Other studies that have looked at state-level regulations have found some evidence for beneficial impacts on “outputs,” such as whether crime guns originate in or out of state, but there is little evidence for effects on homicide or other outcome measures of more direct policy interest. The one exception in this regard is a study of regulations to ban possession by domestic batterers (Vigdor & Mercy, 2003). Additional enforcement rather than regulation of the supply side of the gun market might also make a difference, at least based on the limited evidence currently available on the structure of underground gun markets.

Gun Markets

To some people, the notion of trying to keep guns away from a small subset of the population with over 200 million guns already in circulation seems hopeless. But targeted regulation in an environment of widespread availability is not always futile, as suggested by the analogy to minimum-drinking-age laws. There is consensus among scholars that minimum-age laws, while routinely violated by a majority of older teens, are nonetheless effective; the quasi-experimental evidence of numerous changes in state minimum-age laws during the 1970s and 1980s provides evidence that this partial prohibition lowers alcohol abuse, traffic accidents, and crime (Wagenaar & Toomey, 2002).

Whether restrictions on gun acquisitions are or could be similarly effective is not clear, although the prospect is somewhat less daunting when we recognize that the stock of guns in America probably matters less than the flow. Most of our country's guns are in the hands of relatively low-risk people and are likely to remain there (theft notwithstanding) for many years. The large majority of gun crimes are committed by a small group of criminally active people whose criminal “careers” are typically fairly short in duration. Regulation might be effective if it makes it harder for each new cohort of criminally active young people to acquire guns (Cook, 1991). And it is surely relevant to note that most gang members and violent criminals do not own guns, as evidenced by survey data and by the fact that only a small minority of robberies involve guns—despite the evident advantage of using one.

Considering that the secondary market is the proximate source for the vast majority of crime guns, one obvious intervention point is the movement of guns from the primary to secondary markets. Most of what is known about the structure of these markets comes from an intensive case study of Chicago (Cook, Ludwig, Venkatesh, & Braga, 2005). Professional traffickers seem to play an important role in moving guns across markets, as suggested by both ethnographic field interviews in one high-crime part of Chicago as well as by Bureau of Alcohol, Tobacco, and Firearms (ATF) investigation files and crime-gun trace data (see also Cook & Braga, 2001). In principle, other “traffickers” may simply be girlfriends or relatives who engage in one or two straw purchases to provide guns to someone with a disqualifying criminal record. However, these types of straw purchases appear to be relatively rare in Chicago. As one of the informants interviewed as part of the Cook, Ludwig, Venkatesh, and Braga (2005) study noted:

Most of us, we never been outside these four or five blocks, our neighborhood. Now how can you bring the guns here if you don't even know how to get to other places? . . . Even if we go to jail, we really spend most of our time around where we live, where we work.

Some licensed gun dealers are willing accomplices to gun trafficking or straw purchases, or are selling to criminals off the books (Wachtel, 1998). One ATF investigation of the relatively small subset of dealers who account for the original retail sale of most crime guns submitted for tracing found that 75 percent were in violation of at least one federal regulation. While most of these were for minor violations, 20 percent of dealers in this sample were recommended for license revocation (ATF, 2000b).

Regardless of the actual frequency of dealer malfeasance, the ability of ATF to monitor dealers under the current regulatory system is quite limited. As a practical matter there are so many retail licensees—currently about 80,000 (ATF, 2000a)—that ATF can only inspect a few percent of them in any one year. Even when ATF investigators determine that a dealer is in serious violation of the law, it can be very difficult to take effective action, thanks in part to federal legislation (the McClure-Volkmer Act, or Firearm Owners Protection Act of 1986) that limits regulatory actions and establishes a near-impossible evidentiary requirement for successful prosecution (Cook & Ludwig, 2002; Butterfield, 2001).

If regulation or enforcement could reduce the flow of guns from primary to secondary market, standard economic analysis suggests that the resulting decline in supply would increase the price of guns in secondary markets. Diverting high-risk buyers from the primary to the secondary market (by, for example, improving background checks or cracking down on rogue dealers) would further increase prices in the latter by increasing demand (Cook & Leitzel, 1996).

Enforcement might also help increase the price of guns to high-risk people by targeting “retailers” in the underground gun market. Cook, Ludwig, Venkatesh, and Braga (2005) found that the underground gun market in Chicago is characterized by high transaction costs. Prices are high relative to the legal “primary” market, although because quality is often low in this market, prices can be low in an absolute sense: The median price paid among one sample of Chicago arrestees was \$150, with one-quarter of guns bought for less than \$100. Information about buyers and sellers (including the trustworthiness of trading partners) in the underground gun market is scarce, perhaps in part because guns, unlike drugs, are durable goods and so the underground gun market is quite “thin,” with a relatively

small number of transactions. At least in Chicago, many gun transactions are navigated by local brokers who draw on their social networks to match up buyers and sellers, and advertise their services in part through word of mouth throughout the local underground economy. The costs of doing business for these local brokers and the higher-level suppliers that provide them with guns could presumably be increased through buy-and-bust or sell-and-bust activities by the local police, as well as by offering arrestees incentives to provide information about retailers and sellers in the gun market.

The frictions found in Chicago's underground gun market lead some youth to join gangs in part to facilitate access to firearms (Cook, Ludwig, Venkatesh, & Braga, 2005). The fact that gangs have important economic interests provides law enforcement with leverage to encourage gangs to restrict access to members, which seems to be what happens in Chicago to some degree. Less desirable from a social policy perspective is the role that gangs play in encouraging violent behavior and other forms of criminal malfeasance by members. We return to this point below as part of our discussion of gun-oriented enforcement.

Whether price increases in the underground gun market translate into decreased gun misuse depends on how price-sensitive teens and criminally inclined adults are. Surprisingly little is known on this point, although scattered survey evidence suggests that criminals are not entirely immune to the financial and other costs of getting guns. In one survey of incarcerated adults, 21 percent of those who chose not to use a gun to commit their crimes said that the trouble of getting a gun played a "very" or "somewhat" important role in their decision; 17 percent cited the financial cost (Wright & Rossi, 1994, pp. 128–129). In a survey of incarcerated teens in North Carolina, one said that "when [people] are short of money, they have no choice but to sell [their guns]," while another remarked that he had "traded a .22 for a Super Nintendo and some other guns for a VCR and for my waterbed. I got other stuff for my room, like a phone with lights and a copy [fax] machine for a twenty-gauge" (Cook, Molliconi, & Cole, 1995). With higher prices, we would expect cash-strapped youths to be less inclined to buy a gun and more inclined to sell whatever guns come their way. Further, higher prices would provide an incentive for those who do have a gun to exercise greater caution against theft and confiscation by law enforcement by, for example, leaving it at home.

The Brady Act

One sign of the 1968 GCA's effectiveness comes from the fact that surveys of prisoners from the 1980s show that only around one-fifth obtained their guns directly from a licensed gun dealer (Wright & Rossi, 1994), even though dealers in most states were not required to conduct background checks to verify the buyer's eligibility. The GCA's restrictions were strengthened beginning in 1994 by the Brady Handgun Violence Prevention Act, which required gun dealers in states without background-check requirements to begin to conduct such checks on prospective buyers. Around 3% of potential handgun buyers have been denied handguns following Brady as a result of background checks (Manson & Gilliard, 1997), which is to say hundreds of thousands have been denied since the Brady Act went into effect. Descriptive statistics like these have led many to conclude that the Brady Act has had a substantial effect on crime and suicide.

More direct evidence on the Brady Act's effects on public safety comes from comparing mortality trends in the 32 states that were required to abide by Brady's background check and waiting period requirements with the 18 states (plus the District of

Columbia) that already had sufficiently stringent policies in place, and as a result were exempt from the Brady provisions. Our analysis, published in the *Journal of the American Medical Association*, exploited this natural experiment using a “difference-in-difference” analysis. That approach nets out the permanent differences among states in conditions that contribute to homicide, and presumes that the “experimental” and “control” states would have followed the same trajectory on average in the absence of the law. That presumption is reasonable both because of our finding that the trajectories were similar *prior* to the law, and because the law in question was exogenous to the individual states—there is no self-selection problem here, as might arise if we were evaluating laws that were changed by act of individual state legislatures.²²

We find no statistically discernible difference in homicide trends between the “Brady” (treatment) and “non-Brady” (control) states among people 21 and older (Ludwig & Cook, 2000). Given the standard error of this estimate, it is plausible that there was a true effect on homicide ranging from an increase of around 8% to a reduction of 13%. Considering that a reduction in homicide of even a few percentage points would have made the law highly worthwhile, we cannot rule out the possibility that this policy furthers the public interest. Indeed, that conclusion would be quite attractive if it were possible to rule out a *perverse* effect *a priori*—which is to say, rule out the possibility that some people who needed a gun quickly in the face of a specific threat were delayed by *Brady* regulations, and as a result ended up being killed. While we know of no specific examples, it is at least a logical possibility.

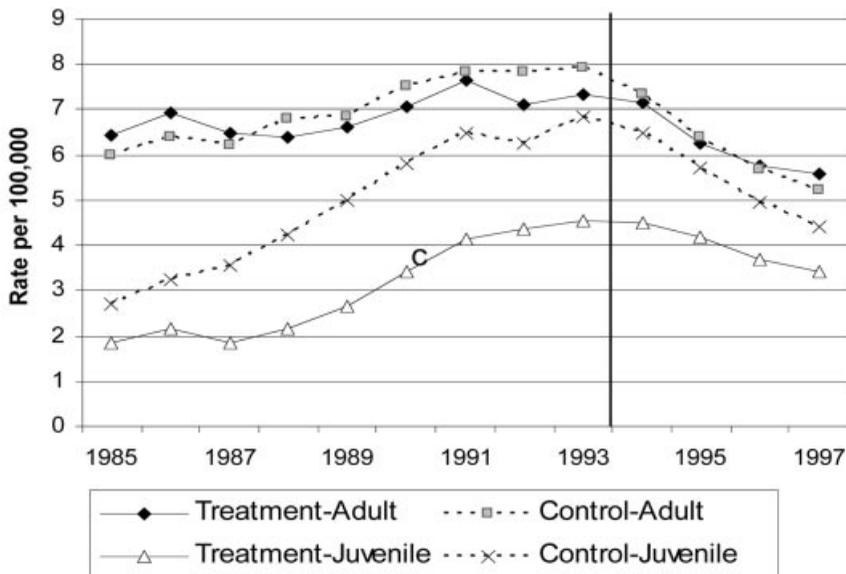
Our focus on *adult* mortality rates is motivated by the different trajectories that juvenile homicides follow in treatment and control states even before the Brady law went into effect (see Figure 3). As a result, any differences in juvenile homicide trends following implementation of the Brady Act cannot be confidently attributed to the effects of the law itself. Excluding juvenile victims is not particularly problematic, since most of them were shot by those who would have been too young to be directly affected by the Brady background check requirement (Cook & Laub, 1998).

Our methodological point is that when evaluating discrete policy interventions, one check on the validity of the “control” group is whether it follows a trajectory similar to the “treatment” group *prior* to the intervention. If not, then the resulting estimates of the treatment effect may well be biased.²³ This type of objective specification test provides the basis for a rejoinder to the common complaint that statistics can be used to “say anything” and argue either side of an issue.

The Brady case provides an illustration. While our analysis finds no statistically significant effect of the Brady Act on homicides or other violent crime, John Lott asserts that Brady increased the number of rapes and perhaps assaults as well (Lott, 2000, pp. 90, 200). The contradiction results from the fact that Lott’s evidence comes from comparing crime rates in treatment and control states following Brady’s implementation for people of *all* ages, including juvenile as well as adult perpetrators. Since juvenile crime trends in the Brady treatment and control states diverge even before Brady goes into effect, Lott’s analysis is likely to confound the effects of the Brady Act with those of whatever unmeasured factors cause juvenile trends to differ across the two groups of states during the pre-Brady period.

²² The individual state legislatures in the experimental group had “chosen” to not regulate gun transactions, indicating that they have a different political climate in this respect than the control states. But that difference is part of the permanent cross-section difference among states that is netted out by the difference-in-difference procedure. The change in regulations was imposed by the federal law and hence did not reflect a shift in state political climates, which would have confounded our estimates.

²³ See Bassi (1984); Heckman and Hotz (1989); Black and Nagin (1998); Ludwig (2000); Smith and Todd (2005).



Source: Ludwig and Cook (2000).

Figure 3. Gun homicide rates, Brady Treatment vs. Control States, adults (over 21), and juveniles (under 21).

A distinct concern in evaluating the effects of the Brady Act is that the new law may have reduced gun running from the treatment to control states, in which case comparing the two groups of states might understate the overall effects of the law. In a nutshell, the concern here is that the “control” states were in fact affected by the intervention. Some support for this concern comes from ATF trace data in Chicago showing that the fraction of crime guns in the city that could be traced to the Brady treatment states declined dramatically following implementation of the law (Cook & Braga, 2001).²⁴ However, the proportion of homicides in Chicago committed with guns did not change over this period, despite the substantial changes in gun-trafficking patterns (Cook & Ludwig, 2003c). One explanation of these results is that traffickers can adapt easily to changes in the larger environment. If correct, they suggest that any bias introduced into comparisons of Brady treatment and control states due to changes in across-state gun-running is minor.

Of course the Brady Act may affect outcomes other than crime. Comparing trends in treatment and control states suggests that Brady may have reduced gun suicide rates among those 55 and older and that the waiting period requirement of the law may have been responsible. However, these gains were at least partially offset by an increase in non-gun suicides, so whether the waiting periods reduced overall suicides among this age group is unclear (Ludwig & Cook, 2000).

²⁴ Cook and Braga analyzed the age and point of first sale of all guns confiscated in Cook County in 1999. Guns first sold prior to 1994 were much more likely to come from out of state. The change in 1994 is large and abrupt.

State Regulations of Gun Acquisition

Many states have supplemented the minimum requirements of the 1968 Gun Control Act with additional restrictions and regulations for gun transactions. The available evidence on the effects of these state-level regulations in general relies on weak research designs, and yields stronger evidence for regulatory impacts on immediate output measures, but not on outcomes that are of more direct policy interest.

For example, a study by Weil and Knox (1996) using ATF firearms trace data found that the fraction of crime guns confiscated in Northeastern states that were first purchased in Virginia declined after that state's "one gun a month law" went into effect. While persuasive on its own terms, the net effect of a change in crime-gun source states on the effective price of guns and ultimately on criminal gun use is far from clear. Other studies analyze outcome measures but using weaker designs that leave room for substantial uncertainty about the proper interpretation (Webster, Vernick, & Hepburn, 2001; Kleck & Patterson, 1993; Webster, Vernick, & Hepburn, 2002).

Gun Possession by Violent Misdemeanants

Perhaps the exception to the generally bleak literature on gun regulation regards initiatives that move the boundary between who is and is not eligible to purchase a firearm. Two recent federal examples include the 1994 ban on gun possession by people under a restraining order for domestic violence, and the 1996 Lautenberg Amendment that extended that ban to anyone convicted of a domestic violence misdemeanor. Encouraging evidence for the effects of these laws comes from study of similar state-level laws.

Elizabeth Richardson Vigdor and James Mercy find that state laws that prevent those who are subject to a restraining order from owning or purchasing a handgun reduce rates of intimate partner homicides (by about 10 percent), while there are no clear effects for prohibitions directed against those with prior misdemeanor convictions for domestic violence. Their use of repeated cross sections of state-level data enable them to control for at least some of the unmeasured state or period effects that may confound evaluation of these laws. Presumably, the difference in the effects of the two laws is due to some combination of the inability of available data systems to identify all those with domestic violence records, the close timing between state and federal laws that keep guns from those convicted of domestic violence misdemeanors, and the fact that there are likely to be more people subject to restraining orders than with prior convictions for domestic violence misdemeanors. The results for the restraining order laws are also more likely to reflect causal policy effects than those for domestic violence misdemeanors. Prohibitions on those with restraining orders are consistently related to intimate-partner homicides and unrelated to other crimes that should less clearly be affected by gun regulations; the reverse is true for the domestic violence laws. The data also suggest that states with restraining order prohibitions experience rates of intimate-partner homicides quite similar to those observed in other areas before these gun laws go into effect, at least up to one year before passage.²⁵

²⁵ Wintemute, Wright, Drake, and Beaumont (2001) find encouraging results from an examination of California's law, although the research design employed leaves the estimates susceptible to bias from omitted variables (see Cook & Ludwig, 2003a).

Concluding thoughts about regulating transactions

What do these results imply for the prospects of restricting gun access to high-risk people? The apparent fact that the Brady Act did not have a substantial effect on homicide may reflect the gaping loophole, the secondary market, left unregulated by this act. Still, the evaluation results, while reliable, are not precise enough to rule out the possibility that the Brady Act’s costs are exceeded by its benefits. Generally speaking, if mild, inexpensive regulations save even just a few lives, they may well be justified. That principle applies to the case of banning possession by domestic batterers, where there appears to be a small but discernible effect.

While most of the policy attention has been devoted to access regulations or use enforcement (discussed next), new evidence on the structure of underground gun markets suggests the potential of access-reducing enforcement. Transaction costs in the underground gun market might be increased by targeting the retailers and wholesalers that seem to play an important role in at least tight-control, low-gun jurisdictions like Chicago, although perhaps with the undesirable side effect of steering more youth toward street gangs that help facilitate gun access. More information about how gun markets operate in other jurisdictions would have substantial value in refining both enforcement activities and regulations, and determining why some regulations appear to be effective in saving lives while others seem to be less so.

Stepping back, we note that the uneven evidence on the effectiveness of gun-control measures stands in seeming contrast to the relatively strong evidence that gun availability has a positive effect on homicide rates. There are a variety of possible explanations, one of which is that, in practice, most gun-control measures have not had much effect on gun availability to dangerous people—certainly not as much as would a substantial reduction in the prevalence of gun ownership.

Summary: Regulations on Gun Transactions and Possession

Treatment	Effect of Treatment on Access to Guns by Violent People (Output)	Effect of Treatment on Homicide Rates (Outcome)	Confidence in This Conclusion
Brady Act (background checks)	Small	Small	High
Ban on possession by domestic batterers	Medium (for relevant group)	Medium	Medium

TARGETING CRIMINAL GUN USE

While “gun control” entails regulations on gun commerce and possession, “gun policy” is a broader term that also incorporates laws and programs to reduce misuse more directly. Here we discuss the evidence on sentence enhancement and gun-oriented enforcement.

Sentence Enhancements for Criminal Gun Use. The idea of meting out longer prison terms for those who use guns in crime seems appealing because guns make crime more lethal, and in general we are persuaded by a growing body of evidence that criminal behavior can be deterred by increases in expected punishment (Nagin, 1998; Levitt, 2001). However, to date there is not strong evidence that sentence enhancements deter gun use in crime, perhaps because these laws may not be

implemented as intended (see Marvell & Moody, 1995), or because any effects might be too modest for available data and evaluation techniques to detect.

Most of the studies of sentence enhancements rely on time-series models, some of which find evidence for an effect of such laws on gun involvement in at least some types of crime, but none of these essentially pre-post comparisons are entirely convincing (Loftin & McDowall, 1981; Loftin, Heumann, & McDowall, 1983; Loftin & McDowall, 1984; McDowall, Loftin, & Wiersema, 1992).²⁶ The main problem with these time-series analyses is the possibility of other changes in these jurisdictions over time that can also affect gun involvement in crime. The fact that similar sorts of changes are observed in multiple jurisdictions that enact a policy at different times does not rule out the possibility of confounding factors, since enactment of sentence enhancement laws may be systematically related to local crime trends. For example, if these types of “get tough” laws tend to be enacted in response to increasing rates of gun crime (as seems plausible), and if local crime rates are cyclical (as they tend to be), then subsequent declines in gun crime following sentence enhancements may simply be the result of mean reversion—even if similar patterns are observed in different jurisdictions that enact such laws at different points in time. Even the more sophisticated state-level panel data study by Marvell and Moody (1995) is susceptible to this type of bias to some degree, as we discuss further below within the context of state laws that regulate concealed gun carrying.

Gun-Oriented Enforcement. Boston’s Operation Ceasefire is one of the most widely cited and conceptually appealing gun-oriented enforcement efforts enacted to date (Braga, Kennedy, Waring, & Piehl, 2001; Kennedy, Piehl, & Braga, 1996). 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. Of particular interest are the program’s activities targeted at gun use, in which 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 inter-gang violence would provide a “cooling off” period that would break the dynamic of violence fueled by “gang beefs” and retaliatory attacks. While the quantitative evidence on Ceasefire is mixed and somewhat difficult to interpret, qualitative evidence from Cook, Ludwig, Venkatesh, and Braga (2005) provide at least suggestive complementary support in favor of the idea.

Any formal evaluation of Ceasefire as it was implemented in Boston must confront two complications.²⁷ 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

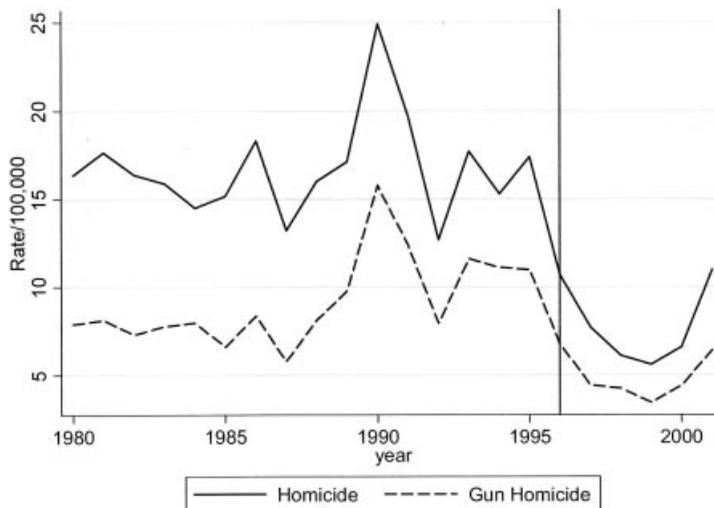
²⁶ Other studies using the same basic research design have found evidence of some decline in gun use in crime in at least some cities in Arizona (McPheters, Mann, & Schlagenhauf, 1984) but not California (Lizotte & Zatz, 1986). Kleck (1991) uses a cross-sectional analysis of data from 170 cities and finds no effect on gun use in crime.

²⁷ In principle, a third 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, McRoberts, & Winship, 2000; Berrien & Winship, 2003; and Winship, 2002).

before Ceasefire went into effect in summer 1996 (Braga et al., 2001, p. 205). When Piehl, Cooper, Braga, and Kennedy (2003) search the time series for the “optimal break” in trend starting with the observations for January 1996 the data point to summer 1996 (when Ceasefire was in fact initiated), 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 4. 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 & 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 1993–95—despite the trend in “fundamentals” that Levitt (2004) argues drove crime rates down everywhere during the 1990’s—before resuming their decline.²⁸ Most studies use this 1993–95 period as the “pre-treatment” benchmark period for evaluating Ceasefire. However, if the 1993–95 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 to 2001. Under this approach, Boston’s decline in homicides was (in proportional terms) about average compared to 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



Source: Ludwig (2005).

Figure 4. Homicide trends in Boston, MA.

²⁸ Youth homicides show a similar pattern; see Cook and Ludwig (2004).

appropriate comparison group for Boston have led to competing claims about the efficacy of the Ceasefire intervention.²⁹

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 “pulling levers” comes from ethnographic fieldwork from Chicago’s South Side reported in Cook, Ludwig, Venkatesh, and Braga (2005). The Chicago police already employ some components of Ceasefire’s gang-oriented pulling levers approach, and as a result many gang leaders seek to regulate gun use by current and former members. One gang leader explained why he tries to limit gun use by the gang’s local affiliates: “If they don’t have guns, they don’t cause a lot of trouble, nobody [from the police] comes down on them, and things just flow [and we make our money]. And, if they need a gun, we’ll give it to them.” Police typically assume that gang members or alumni caught in possession of a gun obtained the weapon from the gang and so crack down on the gang accordingly. As one police officer noted:

Look, I’ll be honest with you. There will always be drugs, drug dealing, and drug dealers. The reason we get tight on guns is that it’s better that there be drugs and no one gets killed than if someone gets killed. We love guns! We love getting them because it makes the job easier on the street. So, when we find one, yes, we really go after them [gang leaders] because they know the rules. They know the agreement, and if we get a gun, that means they broke it.

While Ceasefire’s gang-deterrence strategy holds considerable conceptual appeal and enjoys some support from qualitative research, the limits of the available quantitative evaluations leave us uncertain about the program’s actual impacts (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 (Braga, 2002; Tita et al., 2003).

Summary: Criminal Gun Use

Treatment	Effect of Treatment on Access to Guns by Violent People (Output)	Effect of Treatment on Homicide Rates (Outcome)	Confidence in This Conclusion
Sentence enhancements for gun use in crime	Medium	Small	Medium
Gun-oriented enforcement	Difficult to generalize	Possibly quite large	Low

²⁹ When Braga et al. (2001) use monthly data to compare Boston’s experience before and after Ceasefire with what was observed in 39 other large cities across the country, they conclude that Boston experienced an unusually pronounced decline in youth homicides. Applying slightly different methods to annual data using a comparison group of 95 cities, Rosenfeld, Fornango, and Baumer (2005) find no detectable effect of Ceasefire. Fagan (2003) 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 as large or larger in other townships throughout Massachusetts compared to Boston.

GUN CARRYING

One step back from gun use in crime is illegal carrying, and policies to deter carrying by dangerous people may be an efficient strategy for reducing misuse. As Lawrence Sherman notes, “To the extent that homicide frequently occurs spontaneously among young men in public places, it is the *carrying* of firearms, rather than their ownership, that is the immediate proximate cause of criminal injury” (Sherman, 2000, p. 1193).³⁰ Others, impressed by the potential value of an armed public in deterring street crime, have successfully advocated for relaxing restrictions on carrying by adults who can pass a criminal-record check.

The available empirical evidence in support of policing against illegal gun carryings is currently much stronger than what is available in support of longer prison terms for carrying guns illegally. The net impact of state laws that make it relatively difficult for normal citizens to get permits to carry guns legally remains very unclear. But since just a small share of the population gets concealed-carry permits, even in states with lax permit requirements, and since those who get permits are at very low risk of either criminal offending or victimization, the net effects of restrictive gun-carry laws are likely to be modest in any case.

Policing against Illegal Guns

The most straightforward way to keep people from carrying guns illegally is to arrest them when they do so. The widespread belief in the effectiveness of police patrols against illegal gun carrying is motivated in large part by findings from the Kansas City Gun Experiment, in which patrol resources were added in one high-crime neighborhood to search pedestrians and motorists for guns. Lawrence Sherman and his colleagues calculate that gun seizures increased by 65 percent in the target neighborhood during the program, while gun crime declined by 49 percent. In contrast, there was little change over this period in either outcome in a comparison neighborhood several miles away (Sherman, Shaw, & Rogan, 1995; Sherman & Rogan, 1995).

Despite the promise of the Kansas City Gun Experiment, it is important to recognize that this program was not an “experiment” in the true sense of the term. There were just two neighborhoods involved, and they experienced different levels and trends in firearm offenses even *before* the policing program was put into place (Sherman, Shaw, & Rogan, 1995). As we have argued above, that difference should make for caution in drawing inferences from differences in crime rates after the program was put into place. While policymakers in New York City and elsewhere have implemented police patrols against illegal guns, convincing evidence on the effects of this strategy is lacking.

Cohen and Ludwig (2003) provide stronger evidence in support of the effects of such patrols by evaluating a model program implemented in Pittsburgh. Their evaluation exploits the fact that gun-oriented patrol was implemented in some parts of the city but not others, and that in the targeted areas the extra patrols were focused on just four evenings each week (Wednesday through Saturday). Their main finding is that during the targeted nights of the week, the target neighborhoods experienced much larger declines in gunshot injuries and citizen reports of shots fired

³⁰ James Q. Wilson extends the argument: “Our goal should not be the disarming of law-abiding citizens. It should be to reduce the number of people who carry guns unlawfully, especially in places—on streets, in taverns—where the mere presence of a gun can increase the hazards we all face” (Wilson, 1994).

compared to control areas. Evidence that, at least for gunshot injuries, the control neighborhoods provide a reasonable estimate for what *would have* happened in the treatment areas had the program not been enacted comes from the fact that there was little difference in injury trends between treatment and control neighborhoods on days in which the new anti-gun patrols were *not* scheduled (Sunday through Tuesday). In addition, the treatment and control neighborhoods have similar trends in gunshot injuries *before* the policing program was implemented. However, the treatment and control neighborhoods did have significantly different experiences with shots-fired reports (the other outcome they examine) even before the program was in effect, so we should be more confident in the results for gunshot injuries than shots fired.

This evaluation supplements existing evidence that police programs targeted against illegal gun carrying may *reduce* gun violence. Given the substantial costs of gun violence to society—on the order of \$1 million per gunshot injury (Cook & Ludwig, 2000; Ludwig & Cook, 2001)—these policing programs easily generate benefits to society in excess of their operational costs. Of course, aggressive police patrols may generate other costs, impinging on civil liberties and straining police-community relations. In the Pittsburgh case, at least, the police appear to have been mindful of these concerns, and quite restrained.

Enhanced Punishment

Another approach to deterring illegal gun carrying is to enhance the threatened severity of punishment for those who are caught. In the 1970s, this approach was used with apparent success in Massachusetts, which enacted the Bartley-Fox Amendment mandating a one-year prison sentence for unlicensed gun carrying. The new law prohibited plea bargaining and was widely advertised; the law was subsequently evaluated in several careful studies, with somewhat contradictory results (Pierce & Bowers, 1981; Wellford, Pepper, & Petrie, 2005).

In recent years, the most highly touted example of this approach is Richmond, Virginia's, Project Exile, which diverted convicted felons who are arrested for gun possession from state courts into the federal system, where penalties are more severe. Exile now serves as one model for the Bush administration's nationwide Project Safe Neighborhoods (PSN) initiative. Advocates for Project Exile often point to the 40 percent reduction in gun homicides in Richmond between 1997 and 1998 as evidence.³¹ Skeptics point out homicides actually increased during the last 10 months of 1997 following Exile's launch in February, and that the homicide rate during 1997 as a whole was around 40 percent higher than in 1996. Neither of these simple before-and-after claims is very convincing because without a control group, there remains the obvious question of what Richmond's crime trajectory would have been in the absence of this "Project." After all, violent crime rates were declining dramatically across the country during the 1990s (Blumstein & Wallman, 2000; Cook & Laub, 2002).

The first rigorous evaluation of Project Exile is by Raphael and Ludwig (2003), which offers no evidence that the program effected a reduction in homicides or other types of crime in Richmond. They show that Richmond's crime trajectory (even removing 1997 data from the picture) in the late 1990s is not notably better

³¹ "Have Gun? Will Travel," by Elaine Shannon, *Time Magazine*, August 16, 1999, 154(7); and "Remarks by the President on Project Safe Neighborhood," The White House, Office of the Press Secretary, May 14, 2001.

than other cities that had experienced similarly volatile homicide rates since 1980. This null finding is robust to a variety of methodological adjustments, including a check for omitted variables bias that uses juveniles (who are generally exempt from the federal felon-in-possession charges that make up the bulk of Exile prosecutions) as an additional within-city control group. Levitt (2003) notes that expectations of large impacts were probably unrealistic from the start, since Exile engendered a small objective increase in the threat of punishment. Greenwood (2003) suggests that the program did not focus sufficiently on the most dangerous group of offenders.³²

Restrictive Gun-Carrying Laws

While many big-city police departments devote substantial resources to keeping guns off the street, over the past several decades state governments across the country have made it easier for people to carry guns legally in public. More than 30 states have eliminated their restrictive gun-carrying laws, and a number of others, such as Missouri, Minnesota, Ohio, and Wisconsin, are considering such changes (Dvorak, 2002). These legislative changes are not necessarily in conflict with police patrols against illegal gun carrying, because there is not much overlap in the population characteristics of those who apply for permits to carry and those who are targeted in police patrols.

Opponents of restrictive gun-carrying laws hope that the increased likelihood of encountering an armed victim will deter criminals, a possibility that receives some support from prisoner surveys: 80 percent in one survey agreed with the statement that “a smart criminal always tries to find out if his potential victim is armed” (Wright & Rossi, 1994). But the same data also raise the possibility that an increase in gun carrying could prompt an arms race: Two-thirds of prisoners incarcerated for gun offenses reported that the chance of running into an armed victim was very or somewhat important in their own choice to use a gun. Currently, criminals use guns in only around one-quarter of robberies and one of every 20 assaults (Rennison, 2001). If increased gun carrying among potential victims causes criminals to carry guns more often themselves, or become quicker to use guns to avert armed self-defense, the end result could be that street crime becomes more lethal.

In a provocative series of research papers and books, economist John Lott has argued that the deterrent effects of eliminating restrictive gun-carrying laws dominate: “Of all the methods studied so far by economists, the carrying of concealed handguns appears to be the most cost-effective method for reducing crime” (Lott, 2000, p. 20). Lott and fellow economist David Mustard improved on earlier research by comparing crime changes in states that enact concealed-carry laws with changes in other jurisdictions (Lott & Mustard, 1997). Lott has now performed this analysis in a variety of ways, reaching differing conclusions about the effect on property crime (Cook et al., 2002), but always finding that ending restrictive gun-carrying laws reduced homicide rates (Lott, 2000, pp. 90, 100).

Economist John Donohue (2003) argues that, while Lott’s analysis improves upon previous research on this topic, in the end Lott’s findings cannot support the conclusion that ending restrictive concealed-carry laws reduces crime. Donohue shows

³² Rosenfeld, Fornango, and Baumer (2005) reach a different conclusion about Exile’s effects in Richmond. But their analysis only considers crime data back to 1992, and so omits the most important variable explaining why Richmond had an above-average decline in homicide after Exile went into effect—the city’s above-average increase in homicides from the mid-1980s to the mid-1990s.

that Lott's estimates are sensitive to the correction of several coding errors and to reasonable changes in the model specification. More important, Donohue's re-analysis of the Lott data shows that states that eventually ended restrictive concealed-carry laws had systematically different crime trends from the other states even before these law changes went into effect—violating what we have argued is a minimum necessary condition for deriving unbiased estimates of policy impacts. The violation of this condition implies that the estimated treatment effect may in fact be due to whatever unmeasured factors caused crime trends to diverge before the laws are enacted.

In a response to Donohue, Lott's co-author, David Mustard, notes that their work tries to address this apparent omitted-variables problem in a number of ways. In our own judgment, as well as that of the NRC panel report, none of these approaches is persuasive.³³ The puzzling pattern of results for robberies and property crimes in this literature is one manifestation of this issue.³⁴ Another is the finding, by Donohue and the NRC panel's own re-analysis of Lott's data, that "right-to-carry" laws in the 1980s seemed to reduce crime, while those adopted in the 1990s appear to have the opposite effect.³⁵ Manning (2003) notes in his commentary that few of the estimates reported in this literature may be statistically significant anyway, once one correctly calculates standard errors and the relevant statistical tests.³⁶

Whether the net effect of relaxing gun-carry laws is to increase or reduce the burden of crime, there is good reason to believe that it is not large. One recent study found that in 12 of the 16 permissive concealed-carry states studied, fewer than two percent of adults had obtained permits to carry concealed handguns (Hill, 1997). The actual change in gun-carrying prevalence will be smaller than the number of

³³ For example, the instrumental variables (two-stage least squares) estimates presented by John Lott and David Mustard yield implausibly large estimates for the effects of right-to-carry laws on crime; see Donohue's chapter as well as Ludwig (1998, 2000). Using nonlinear state-specific trends may yield evidence for right-to-carry laws when separate trends are included for the pre- and post-law periods, but not when each state's crime trend over the entire sample period as a whole is modeled using a linear and quadratic term (Black & Nagin, 1998). Because crime rates follow the same types of cyclical patterns as do many economic indicators, and these right-to-carry laws are adopted during periods of increasing crime, isolating their causal effects is quite difficult in practice. That the post-law crime levels are below the pre-law levels does not rule out the influence of whatever factors drive these crime cycles over time.

³⁴ Mustard (2003) argues that the net effect of right-to-carry laws on a county or state's robbery rate is ambiguous because not all robberies occur in public places, and right-to-carry laws may cause some criminals to substitute from robbing people in public places to committing such crimes in private areas instead. But the proportion of criminal events that occur in public areas is higher for robbery than for murder, rape, and other violent crimes. Why we should see substitution from public to private areas suppressing the right-to-carry effect on robbery more than for other violent crimes is unclear.

³⁵ Mustard (2003) argues that compared to right-to-carry laws enacted in the 1980s, the laws adopted in the 1990s involved higher fees, more stringent training requirements, and more restrictions on where those with permits can legally carry their firearms. While this argument offers some hypotheses about why the crime-reducing effect of the laws adopted in the 1990s might be muted compared to those enacted in the 1980s, it cannot explain why Donohue finds that right-to-carry laws adopted in the 1990s seem to increase crime. A similar sort of "treatment heterogeneity" argument is offered by James Q. Wilson in his dissent to the NRC panel report's conclusions about Lott's research (see Appendix A in Wellford et al., 2005). A more likely explanation in our view for the conflicting results between the laws of the 1980s and 1990s is that both sets of estimates are driven by confounding factors that are not captured by the regression model.

³⁶ The key issue here is that annual observations drawn from counties within the same state may not be statistically independent (for example, if shocks to the state government's budget affect the provision of criminal justice or social services statewide). The NRC panel report is correct that if we are willing to assume that the only unmeasured state "shocks" have constant effects on crime outcomes over the entire sample period, then they will be absorbed in the county fixed effects included in all of the models that have been employed in this literature (see Wellford et al., 2005, p. 138).

permits issued would suggest, because many of those who obtain permits were already carrying guns in public (Robuck-Mangum, 1997). Moreover, the change in gun carrying appears to be concentrated in rural and suburban areas where crime rates are already relatively low, among people who are at relatively low risk of victimization—white, middle-aged, middle-class males (Hill, 1997). The available data about permit holders also imply that they are at fairly low risk of misusing guns, consistent with the relatively low arrest rates observed to date for permit holders (Lott, 2000). In sum, changes to state laws governing legal gun carrying are likely to induce only modest changes in the incentives facing criminals to go armed themselves, or to avoid potentially armed victims.

Concluding Thoughts about Gun Carrying

The available results on the effects of restrictive gun-carrying regulations are mixed. If advocates on either side of the debates about gun-carrying regulations expect to generate noticeable changes in crime in their states, they are likely to be disappointed.

On the other hand, strategies to reduce gun carrying by youths and felons deserve consideration. Among the strategies that have made it onto the current policy “menu,” directed police patrol is quite promising, while the threat of more severe punishment appears to be less reliably effective.

Summary: Gun Carrying

Treatment	Effect of Treatment on Access to Guns by Violent People (Output)	Effect of Treatment on Homicide Rates (Outcome)	Confidence in This Conclusion
Directed patrol against illicit carrying (Kansas City, Pittsburgh)	Unknown	Possibly quite large	Medium
Enhanced punishment for illicit carrying (Project Exile)	Unknown	Small	Medium
Permissive gun-carrying laws	Small	Small	Medium

IMPLICATIONS FOR POLICY AND FUTURE RESEARCH

There are no feasible policies that would reduce the rate of gun violence in the United States to that of Western Europe. But we believe there are ways to make a substantial dent in the problem. As we have seen, for example, targeted police patrols against illegal gun carrying appear more promising than extending prison sentences for those who use or carry guns illegally. So we could increase the chances that current government spending in this area will reduce gun violence by shifting resources from PSN’s efforts to impose more lengthy prison terms for illegal gun carrying or use toward directed patrol strategies.

One of our goals for this essay has been to encourage reconsideration of the traditional “scientific” standard; that is, the usual 95% standard: the equivalent of direct proof beyond a reasonable doubt that the intervention works. In the context of policy development, where failure to act is as much a policy as adopting an innovative intervention, we believe that an expected-value standard (of costs as well as

benefits) is more apt. Further, research can inform the policy debate in a variety of ways other than simply assessing the potential effectiveness of each item on a list of possible interventions. In particular, we have emphasized the evidence that gun violence places a substantial burden on the community standard of living, and that the misuse of guns is to some extent under the influence of rational incentives.

This framework, together with the descriptive and analytical information summarized above, opens the door to favorable consideration of a variety of interventions. For example, we would identify as promising gang-oriented deterrence strategies designed to reduce gun misuse by gang members. Formal quantitative evaluation evidence for this approach as in Boston's Operation Ceasefire and elsewhere falls short of definitive. But descriptive statistics about the importance of gangs to gun violence in many cities, together with quantitative evidence that many street gangs have important economic interests (Levitt & Venkatesh, 2000) and qualitative evidence that gangs regulate gun use by members to protect these economic interests (Cook, Ludwig, Venkatesh, & Braga, 2005), suggest to us that this is an approach that is worth pursuing.

Beyond specific interventions, we think it is a useful exercise to consider what an entire portfolio of interventions should look like. For example, if we accept the "rational violence" notion as a guide, then we want to organize a portfolio around making guns a liability to criminals. The goal is to increase the (perceived and actual) likelihood and severity of negative consequences for misusing a gun. If that can be done without much affecting gun access for self-defense by generally law-abiding people, then the expected impact would be beneficial. Included on the list of potentially helpful measures are the following:

- Improve the gun-registration system so that guns confiscated by the police can be more reliably traced to their owners;
- Increase the use in police investigations of the available technology to analyze the ballistic "fingerprints" on shell casings left at the scene of crimes in order to help investigators match confiscated guns to crimes, or to match violent events with each other;
- Launch intensive police patrol directed against illicit gun carrying in high-violence neighborhoods;
- Offer rewards for information leading to the arrest of people carrying or possessing a gun illegally; and
- Institute a gun emphasis policy in investigations and prosecutions of violent crimes.

While only one of these interventions has been subjected to a formal impact evaluation (police patrols), all of them receive general support from the evidence on the potential of deterring criminal behavior by increasing the probability of punishment.

Another promising approach is stepped-up enforcement against the underground gun market to reduce access by criminals. Qualitative evidence from Chicago and survey evidence from arrestees in 22 cities suggest that in many cities guns are not readily available to most criminals (Cook, Ludwig, Venkatesh, & Braga, 2005). This qualitative evidence also suggests that the underground market is structured in ways that may be vulnerable to regulatory enforcement against scofflaw licensed dealers and buy-and-bust undercover operations against illicit brokers.

With respect to gun design, we have to believe that we are better off with the 1934 federal regulation on machine guns, hand grenades, sawed-off shotguns, and other

such weapons, despite the logical possibility that they may be useful in self-defense against invading armies. It is really quite remarkable that Congress has so far resisted banning .50-calibre sniper rifles, which are capable at a great distance of piercing armor, shooting down helicopters, and posing substantial risk to industrial and other terrorist targets.

With respect to suicide, it seems like the scope for enforcement is limited. On the regulatory side, it may help for the states to legislate waiting times for gun sales, an idea that receives some support from Ludwig and Cook's (2000) study of the Brady Act. Perhaps the most important feasible action is to ensure that acutely suicidal people be deprived of ready access to guns by their families, medical providers, and police. That approach has not been subject to a rigorous scientific evaluation for efficacy, but has obvious potential for reducing the chance that a suicidal impulse will result in a successful suicide.

Is more research required before policymakers can take any useful steps to reduce gun violence? No. There are lessons to be learned from currently available research. But of course more research is always welcome. One secondary benefit of our conclusion that enforcement activities directed at both access and use seem promising is the possibility for rigorous evaluation. If some law enforcement agencies accept the importance of "piloting" new enforcement programs before taking them to scale, there would be important scientific opportunities to randomly assign the neighborhoods or townships that receive these pilot programs. In the meantime, wisdom and prudence require better use of existing evidence in formulating gun policy.

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