Gun Self-Defense and Deterrence

Jens Ludwig


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Abstract

Recent research on the prevalence of defensive gun use has prompted growing concern that government efforts to regulate gun ownership and use may be counterproductive. Estimates of defensive gun use from the National Crime Victimization Survey (on the order of 100,000 per year) appear to be too low. However, estimates from one-time telephone surveys (from 1.5 to 2.5 million per year) appear to be too high; even a modest rate of false positives may lead to substantial upward bias. A more promising approach is to examine the net effects of gun policies on rates of crime and injury directly. Evidence for a substantial deterrent effect of permissive concealed gun–carrying laws comes from a recent study by Lott and Mustard. Reanalysis of their data suggests that the estimated “treatment effects” are due in part or whole to unmeasured variables. More recent studies find no evidence of a significant negative effect of these laws on crime, though the available research remains far from definitive.

On the contentious topic of guns in the United States, there are several points on which most observers can agree. First, gun violence is an enormous problem and should be reduced. Since the early 1970s, the United States has averaged nearly 34,000 fatal firearm injuries annually; approximately 40 percent are homicides, 50 percent are suicides,

Jens Ludwig is assistant professor of public policy, Georgetown University and a member of the National Consortium on Violence Research. Thanks to Dan Black, Christina Clark, Philip Cook, Otis Dudley Duncan, Heath Einstein, Gary Kleck, David Hemenway, Arthur Kellermann, Michael Maltz, David McDowall, James Mercy, Daniel Nagin, Mark Rom, Michael Tonry, Jon Vernick, Garen Wintemute, Daniel Webster, Franklin Zimring, and two anonymous referees for helpful comments and assistance. All opinions and any errors are my own.

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and the remainder are unintentional shootings.\textsuperscript{1} In addition, perhaps another 100,000 people suffer nonfatal gunshot injuries each year (Cook 1985; Annest et al. 1995; Cook et al. 1999).

Second, guns are more lethal than most other instruments of violence. Evidence that the type of weapon used in an attack matters, known as an "instrumentality effect," was first documented by Franklin Zimring (1968). Zimring showed that in most gun assaults the assailant's intent to kill is apparently ambiguous, as suggested in part by the small proportion of cases in which the victim suffers multiple wounds. The case-fatality ratio for assault was also shown to be higher when guns rather than knives are used, even though gun and knife attacks typically have similar circumstances and other characteristics in common. A similar finding for gun and nongun robberies was demonstrated by Philip Cook (1983, 1987, 1991), and most scholars now accept the instrumentality hypothesis, even if there remains some disagreement about the magnitude of the effect (Wright, Rossi, and Daly 1983; Cook 1991; Kleck 1991). As a result, most students of gun violence essentially agree that causing criminals to use knives and other weapons rather than guns will result in fewer lethal injuries.

Whether a similar instrumentality effect holds for suicide is less clear, since highly lethal alternatives to guns are more readily available. While the available case-control research seems to suggest that gun ownership is positively correlated with the risk of suicide (Kleck 1991; Miller and Hemenway 1999; Wintemute et al. 1999), interpretation of this finding is complicated by the possibility of unmeasured differences between those who do and do not own guns that may also affect health outcomes. In any event, the question of whether an instrumentality effect exists for suicide has been less central to debates about gun policies, since most interventions have focused on reducing criminal gun use.

Third, even those who disagree about whether the Second Amendment guarantees the individual's right to own firearms agree that there are some who should not be granted access to guns, a group that includes teenagers, the mentally ill, and those with a history of violent criminal activity. More generally, constitutional scholars Laurence Tribe and Akhil Reed Amar (1999, p. A31) have argued that "the right to bear arms is certainly subject to reasonable regulation in the interest

\textsuperscript{1} Unpublished figures from the National Center for Health Statistics' Vital Statistics Program for fatal gunshot injuries from 1972 onward were provided by James Mercy of the Centers for Disease Control and Prevention.
of public safety. . . . As a matter of constitutional logic, to uphold reasonable regulations is not to say that no right exists, or that anything goes.”

What remains highly controversial is whether enhanced regulation of gun ownership and use will increase or decrease crime. The reason behind this controversy is that, while guns make criminal violence more lethal, guns may also have the beneficial effect of enabling private citizens to defend themselves against criminal attack. Proponents of more restrictive gun regulations argue that such efforts will do little to reduce defensive gun use since most law-abiding citizens will retain access to guns under most policy proposals and that, in any case, private citizens can substitute other means of self-defense when guns are not available. Opponents argue that additional regulations will reduce gun ownership rates among ordinary citizens but not among criminals and, thus, impedes the ability of law-abiding people to defend themselves against better-armed criminal predators and to deter crime. For example, James Q. Wilson (1995, pp. 494–95) has argued that “guns are almost certainly contributors to the lethality of American violence, but there is no politically or legally feasible way to reduce the stock of guns now in private possession to the point that their availability to criminals would be much affected. . . . And even if there were, law-abiding people would lose a means of protecting themselves long before criminals lost a means of attacking them.”

Concern that additional gun regulations may be counterproductive has increased in recent years, motivated in large part by a series of influential studies of defensive gun use and the deterrent effects of such uses. Research by criminologist Gary Kleck and his colleagues suggests that guns are used in self-defense by private citizens around 2.5 million times each year, far higher than previous estimates (Kleck 1988, 1991; Kleck and Gertz 1995; Kates and Kleck 1997). Kleck's findings have been invoked in support of arguments that gun regulations, such as waiting periods for handgun purchases and restrictions on gun carrying in public, will increase rather than decrease crime (e.g., see Lott 1999). Supporting evidence of substantial deterrent effects from gun self-defense use comes from the widely publicized research conducted by John Lott and his colleagues (Lott and Mustard 1997; Bronars and Lott 1998; Lott 1998b; Lott and Landes 1999), which suggests that permissive concealed gun-carrying laws reduce crime. Lott's research findings have figured prominently in recent debates about concealed-carry laws in states such as California, Colorado, Kansas, Minnesota,
and Missouri. Given the substantial policy attention that has been devoted to both research literatures, this essay assesses what is known about the prevalence and deterrent effects of self-defense gun use.

Estimates for the prevalence of defensive gun use come from three sources, none of which is ideal. First, several studies have used data from vital statistics or police administrative records to estimate the number of legally justified defensive gunshot injuries (Kellermann and Reay 1986; Kleck 1988; Kellermann et al. 1995). This approach excludes those cases where the victim uses a gun in self-defense but does not injure the perpetrator. The second source of data is the National Crime Victimization Survey (NCVS), a national panel survey that provides crime victims with the opportunity to report on defensive actions they may have taken (Kleck 1988; Cook 1991; McDowall and Wiersema 1994; Cook, Ludwig, and Hemenway 1997). The NCVS appears to understate the prevalence of defensive gun use because respondents are never directly asked whether they have used a gun in self-defense and are only provided with an opportunity to report defensive gun use for a subset of all crimes (Smith 1997). The third source of data is one-time telephone surveys that directly ask respondents whether they have used a gun in self-defense (Kleck and Gertz 1995; Cook and Ludwig 1996, 1997, 1998). These surveys appear to overstate the prevalence of defensive gun use because of “telescoping” (which occurs when respondents report on events that are outside of the survey-question recall period), self-presentation bias, and other sources of measurement error. Thus relatively little is currently known about the prevalence and incidence of civilian gun use in America.

Even unbiased estimates of the prevalence of defensive gun use, however, may leave many important policy questions unanswered. The benefits that civilian gun uses provide to society are difficult to identify from survey data for several reasons. First, only the gun user is interviewed; whether neutral observers would have classified the gun use as defensive or in society’s best interests is often not clear. Second, almost nothing is known about what would have happened to victims had a gun not been available. Equally important, estimates of the frequency of defensive gun uses under the status quo provide no information about how a specific change in gun policy will affect the number of

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2 Information about the NCVS is available from the Bureau of Justice Statistics at www.ojp.usdoj.gov/bjs/.


socially desirable and undesirable gun uses, which is ultimately the question of interest for public policy.

A more promising approach for learning about the effects of defensive gun use and deterrence is to measure directly the net effects of gun policies on rates of crime and injury. John Lott and David Mustard (1997) follow this strategy to learn about the deterrent effects of defensive gun use by examining the net effects of permissive concealed-carry laws. These laws may reduce crime and injury rates by increasing the opportunities for private citizens to use guns in self-defense, thereby deterring criminal behavior. Crime may increase if those who carry guns misuse them or if criminals are more likely to arm themselves in response to these laws or become more likely to resort to force. Since both positive and negative effects are plausible, empirical evidence is required to determine which effects dominate.

The challenge for researchers is that the crime rates that states with permissive concealed-carry laws would have experienced had they not enacted this legislation cannot be observed. Lott and Mustard (1997) draw inferences about the effects of concealed-carry laws by comparing crime in states with and without such laws. Their evaluation improves on previous criminological work on the effects of gun policies by using national data and sophisticated statistical methods that help control for unmeasured differences across states that may affect crime levels and suggests that concealed-carry laws substantially reduce violent crime.

While the Lott and Mustard analysis has raised the standard for empirical evaluations of gun policies, evidence from a reanalysis of their data by Dan Black and Daniel Nagin suggests that the effects estimated by Lott and Mustard are due in part or whole to unmeasured variables (Black and Nagin 1998). More recent evaluations that use alternative methods to control for unmeasured variables produce no evidence of a significant crime-reducing effect of permissive concealed-carry laws (Black and Nagin 1998; Ludwig 1998), though the available evidence remains far from definitive.

This essay is organized as follows. Section I provides descriptive information about patterns of gun ownership, acquisition, and violence in the United States as background for the ensuing discussion of defensive and deterrent benefits arising from the civilian gun stock. Section II reviews the research literature on defensive gun use, while Section III discusses in detail the Lott and Mustard estimates and other evaluations of the effects of concealed-carry laws on crime. Section IV discusses directions for future research.
I. A Primer on Guns in America

While there are enough guns in the United States to arm every adult, gun ownership is actually quite concentrated. Estimates from survey and production data suggest that there are around 200 million guns in private circulation in America. The majority (around 135 million) are long guns that are kept primarily for hunting and other recreational purposes, in contrast to the 65 million handguns in circulation, which are primarily kept for self-protection. This enormous gun stock is owned by roughly one-quarter of America’s adults, three-quarters of whom have two or more guns (Cook and Ludwig 1996). Those who are most likely to own handguns or long guns are in sociodemographic groups that are, on average, least likely to commit or be victimized by crime: gun ownership rates increase with educational attainment and income, are higher among the middle-aged than the young and in rural areas compared with cities, and are highest in the southern and western regions of the country (Wright, Rossi, and Daly 1983; Kleck 1991; Cook and Ludwig 1996).

Guns also tend to be quite “sedentary” in that most do not change hands very frequently—the average gun was acquired around thirteen years ago by its current owner (Cook and Ludwig 1996). Notwithstanding the 500,000–600,000 guns that are stolen each year (Cook, Molliconi, and Cole 1995; Cook and Ludwig 1996), these statistics suggest that most guns are in the hands of those who are unlikely to misuse them and are likely to stay there for some time. The important exception are those guns kept by juveniles and criminals, which change hands quite frequently (Cook, Molliconi, and Cole 1995; Ash et al. 1996).

The number of new guns that enter into circulation each year in the United States (the “flow” of guns across owners) is equal to only around 3 percent of the total civilian gun stock. In 1994, around 7.2 million new guns were sold in the United States, split about evenly between handguns and long guns. In addition, another 1 million used handguns and 1.5 million long guns change hands in off-the-books transactions among private parties who are not federally licensed firearms dealers, or “FFLs” (Cook and Ludwig 1996).

The distinction between transactions that do versus those that do not involve an FFL (termed the “primary” and “secondary” markets,

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1 Unpublished statistics from the Bureau of Alcohol, Tobacco, and Firearms are reported in Cook and Ludwig (1996).
respectively, by Cook, Molliconi, and Cole [1995]) is important because only licensed dealers are required by law to follow state and federal regulations regarding firearm transactions, such as waiting periods and background checks. The 30–40 percent of all gun exchanges that occur in the secondary market are, not surprisingly, responsible for a disproportionately large share of gun assaults. For example, a survey of incarcerated juveniles found that 79 percent had obtained their most recent handguns from a family member, friend, or street connection; only 12 percent had stolen their last handgun; and 7 percent obtained the weapon from a gun or pawn shop (Sheley and Wright 1993).

Much gun violence in America is perpetrated by young men. Around one-sixth of all people arrested for homicide are under the age of eighteen, and nearly two-thirds are under the age of twenty-five (Cook and Laub 1998). A large share of those arrested for gun homicides have had previous contacts with the criminal justice system. For example, a study of all homicide suspects ages twenty-one and under in Boston for the period 1990–94 found that fully 77 percent had been arrested for other crimes prior to committing their homicides (Kennedy, Piehl, and Braga 1996).

Gun policy in the United States has been motivated in large part by the two central facts documented here: crimes are more likely to result in death when guns are used rather than other weapons, and some population subgroups, particularly teens and convicted criminals, are at disproportionately high risk of criminal gun misuse. The first observation has led to policies that attempt to keep guns out of public spaces where there may be altercations; the hope is not necessarily to reduce the number of fights but, rather, to make them less lethal. The second observation has led to prohibitions on primary-market gun sales to teens and convicted criminals and to efforts to stem the flow of guns from primary to secondary markets (such as “one-gun-a-month” laws) and to reduce gun thefts (e.g., by encouraging gun owners to keep their weapons securely locked up). These policies may produce a modest increase in the money or nonmoney price of guns to proscribed groups. The result will be a modest reduction in gun ownership by members of these groups, so long as the decision to keep guns by some group members rests at least in part on the effective price of doing so.

The sparse evidence suggests that at least some members of proscribed groups are somewhat sensitive to the effective price of guns. For example, one incarcerated juvenile in North Carolina reported that “when [people] are short on 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” (Cook, Molliconi, and Cole 1995). Similarly, in a survey of incarcerated adult felons who committed their crimes without guns, 21 percent reported that the trouble of acquiring a gun was somewhat or very important in their decision not to use a gun, while 17 percent reported “cost” as a somewhat or very important consideration (Wright and Rossi 1994).

The challenge for public policy is that strategies for reducing gun misuse may in principle also impair the ability of citizens to defend themselves against criminal attack, which represents a cost that must be weighed against any beneficial effects from reductions in gun misuse. Whether the benefits of various gun policies outweigh their costs is an empirical question.

II. Defensive Gun Use

The prevalence of defensive gun use in relation to gun crime has figured prominently in debates about gun regulation in recent years. Several empirical questions must be answered in order to draw inferences about gun policies. What number of defensive gun use occurs in the United States each year? What are the benefits of defensive gun use for citizens and for society more generally? How would particular gun policies affect the number of socially desirable and undesirable gun uses?

A. Estimates of Defensive Gun Use

Since both law enforcement and public health agencies collect comprehensive and generally reliable mortality data in the United States, a natural starting point for understanding the relative frequency of defensive gun use versus gun misuse is to compare the number of justifiable shootings with the numbers of homicides, suicides, and unintentional injuries. This approach was employed by Arthur Kellermann and Donald Reay (1986), who analyzed mortality records for King County, Washington, over the period 1978–83. Their analysis found that for every justifiable self-defense homicide with a firearm, there were 1.3 unintentional firearm deaths, 4.6 criminal homicides, and thirty-seven gun suicides.

To many gun control proponents, the Kellermann and Reay findings were decisive: if the number of deaths from gun homicide, suicide, and accidents exceeds the number of justifiable shootings, policies to further regulate access to firearms will save lives. Yet the number of
fatal injuries perpetrators may understate the public health benefits of defensive gun use if, as seems plausible, some gun owners deter potentially fatal criminal attacks but do not fatally injure the perpetrator (Kleck 1991). Mortality data may thus provide an incomplete picture of the benefits of defensive gun uses.

In a follow-up study, Kellermann and his colleagues overcome many of the limitations of using mortality data by focusing on data from police reports. The advantage of law enforcement over mortality data is that the former will include all cases in which gun owners prevent crimes and call the police. Kellermann found that self-defense gun use was reported to the police in only 1.5 percent of all burglaries of occupied homes in Atlanta during a three-month period in 1994, which is far less than the number of domestic gun homicides, suicides, and unintentional shootings that occur in Atlanta (Kellermann et al. 1995). Yet police data may still undercount the number of injuries that are prevented by self-defense gun use if some gun owners do not report their defensive actions to the police, for example because of uncertainty about the legality of the owner’s actions (Kleck and Gertz 1995). More fundamentally, national estimates of defensive gun use cannot be obtained from police reports because the official national compilation of such data—the Federal Bureau of Investigation’s (FBI) Uniform Crime Reporting (UCR) system—does not include information on gun self-defense.

Because of the controversy over the interpretation of mortality records and police data, research on defensive gun use has increasingly relied on population surveys. In principle, surveys have the potential to capture defensive gun uses that do not result in the death of the perpetrator and are not reported to police, cases that would be missed by law enforcement and mortality records. The most notable aspect of this survey literature is the considerable divergence in the published estimates, ranging from a low of 65,000 (McDowall and Wiersema 1994) from the National Crime Victimization Survey (NCVS) to 2.5 million (Kleck and Gertz 1995) from smaller one-time telephone surveys.

The NCVS is a nationally representative survey of between 40,000 and 60,000 households conducted by the Census Bureau for the U.S. Bureau of Justice Statistics. The NCVS attempts to interview everyone in sampled households age twelve and older every six months over a three-year period. Respondents are asked whether they had been the victim of a crime within the past six months. For crimes that involved direct contact with the perpetrator, respondents are also asked, “Did
you do anything with the idea of protecting yourself or your property while the incident was going on?” Respondents are also asked, as a follow-up, “Was there anything you did or tried to do about the incident while it was going on?” If the respondent answers in the affirmative to either question, the interviewer asks, “What did you do? Anything else?” (Rand 1999). Studies that use the NCVS to estimate the prevalence of defensive gun use are reviewed in table 1. The most recent published estimate of 108,000 defensive gun uses per year comes from NCVS data for 1994–96 (Cook, Ludwig, and Hemenway 1997), while estimates using data for 1996–98 suggest 72,000 gun uses annually (Rand 1999).

One concern with the NCVS is that respondents are never directly asked whether they have used a gun in self-defense. This is problematic because, as Tom Smith of the National Opinion Research Center notes, “Indirect questions that rely on a respondent volunteering a

<table>
<thead>
<tr>
<th>Study</th>
<th>NCVS Survey Years</th>
<th>Recall Period</th>
<th>Crimes Defended Against</th>
<th>Estimated Annual Defensive Gun Use</th>
</tr>
</thead>
<tbody>
<tr>
<td>Kleck (1988)</td>
<td>1979–85</td>
<td>6 months</td>
<td>Robbery, assault</td>
<td>68,000</td>
</tr>
<tr>
<td>Cook (1991)</td>
<td>1979–87</td>
<td>6 months</td>
<td>All violent crimes and burglary</td>
<td>80,000</td>
</tr>
<tr>
<td>McDowall and Wiersema (1994)</td>
<td>1987–90</td>
<td>6 months</td>
<td>All violent crimes and burglary</td>
<td>65,000</td>
</tr>
<tr>
<td>Cook, Ludwig, and Hemenway (1997)</td>
<td>1992–94</td>
<td>6 months</td>
<td>All violent crimes and burglary</td>
<td>108,000</td>
</tr>
<tr>
<td>Rand (1999)</td>
<td>1996–98</td>
<td>6 months</td>
<td>All violent crimes and burglary</td>
<td>72,000</td>
</tr>
</tbody>
</table>
specific element as part of a broad and unfocused inquiry uniformly lead to undercounts of the particular of interest” (1997, pp. 1462–63). Also omitted are cases where guns are used in defense against crimes that are not asked about in the NCVS survey, including trespassing, vandalism, and malicious mischief. (Whether the use of a gun to prevent such crimes is in society’s best interests is a different question, which is discussed below.)

The limitations of the NCVS motivated criminologists to seek other sources of survey data on defensive gun use, sources that asked respondents directly whether they had ever used a gun in self-defense. Kleck (1988) reviewed the findings from a number of local or national surveys that had included such questions (see table 2), which taken together suggest that defensive gun use is far more common than data from the NCVS had indicated. While the surveys reviewed by Kleck had the advantage of asking respondents directly about whether they used a gun in defense, each survey suffered from other limitations. For example, in five of the eight surveys respondents are asked whether they have “ever” used a gun in self-defense, which makes it difficult to calculate an estimate for the annual prevalence or incidence of defensive gun use. In half of the surveys the respondent is asked to report on defensive gun uses by anyone within the household, which may lead to underestimates of defensive gun behaviors since many respondents are apparently unable or unwilling to report on gun ownership (and presumably gun use) by others within the home (Ludwig, Cook, and Smith 1998). Further, these surveys did not include follow-up questions that would enable analysts to determine whether the gun use was against an animal or a person or whether it was a function of the respondent’s employment as a police officer or security guard (Kleck and Gertz 1995).

Thus the 1993 survey of 4,977 adults sponsored by Kleck and Gertz (1995) represented a substantial step forward in the measurement of defensive gun use. The Kleck and Gertz survey, which was conducted expressly for the purpose of measuring defensive gun use, intentionally oversampled residents of high gun-owning areas such as the South and West, and also oversampled males within contacted households. Each respondent was asked: “Within the past five years, have you yourself or another member of your household used a gun, even if it was not fired, for self-protection or for the protection of property at home, work, or elsewhere? Please do not include military service, police work, or work as a security guard.” Follow-up questions included: “Was this to protect against an animal or a person?” “How many incidents in-
TABLE 2
Estimates for the Prevalence and Incidence of Defensive Gun Use from One-Time Telephone Surveys

<table>
<thead>
<tr>
<th>Study and Survey</th>
<th>Question Refers To</th>
<th>Recall Period</th>
<th>Annual Number of People Who Use Guns in Self-Defense</th>
</tr>
</thead>
<tbody>
<tr>
<td>Kleck and Gertz (1995):</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>DMIa, 1978 ((N = 1,500))</td>
<td>Household*</td>
<td>Ever</td>
<td>2.1 million</td>
</tr>
<tr>
<td>DMIb, 1978 ((N = 1,010))</td>
<td>Household*</td>
<td>Ever</td>
<td>1.1 million</td>
</tr>
<tr>
<td>Hart, 1981 ((N = 1,228))</td>
<td>Household*</td>
<td>5 years</td>
<td>1.8 million</td>
</tr>
<tr>
<td>Mauser, 1990 ((N = 343))</td>
<td>Household*</td>
<td>5 years</td>
<td>1.5 million</td>
</tr>
<tr>
<td>Gallup, 1991 ((N = 1,002))</td>
<td>Respondent†</td>
<td>Ever</td>
<td>.8 million</td>
</tr>
<tr>
<td>Gallup, 1993 ((N = 1,014))</td>
<td>Respondent</td>
<td>Ever</td>
<td>1.6 million</td>
</tr>
<tr>
<td>L.A. Times, 1994 ((N = 1,682))</td>
<td>Respondent</td>
<td>Ever</td>
<td>3.6 million</td>
</tr>
<tr>
<td>Tarrance, 1994 ((N = 1,000))</td>
<td>Respondent/household</td>
<td>5 years</td>
<td>.8 million</td>
</tr>
<tr>
<td>1993 NSDS ((N = 4,977))</td>
<td>Respondent</td>
<td>1 year</td>
<td>2.5 million</td>
</tr>
<tr>
<td>Cook and Ludwig (1998):</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1994 NSPOF ((N = 2,568))</td>
<td>Respondent</td>
<td>1 year</td>
<td>1.3 million</td>
</tr>
<tr>
<td>Hemenway and Azrael (1997):</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1996 Fact Finders ((N = 1,905))</td>
<td>Respondent</td>
<td>5 years</td>
<td>1.5 million</td>
</tr>
</tbody>
</table>

Sources.—For DMIa and DMIb surveys (separate subgroups), both reported in: Decision/Making/Information (1978); for Hart 1981 survey: Hart Research Associates (1981); for Mauser 1990 survey: Mauser (1993); and for Gallup 1991 and 1993 surveys, L.A. Times 1994 survey, and Tarrance 1994 survey, see Kleck and Gertz (1995, p. 182), which reports that these surveys were “taken from a search of the DIALOG Public Opinion online computer database.” For all remaining surveys, see the studies in which they are cited.

Note.—Information on survey sample sizes was kindly provided by Gary Kleck (personal communication). All defensive gun use estimates above use the Kleck and Gertz (1995) definition of “legitimate” defensive gun uses, in which respondent is not employed by police, military, or private security, and gun use is against a human. The estimates from Kleck and Gertz (1995) and Cook and Ludwig (1998) only include defensive gun uses by respondents who saw a perpetrator and actually used a gun, are not employed as police or security personnel, and are against people rather than animals. The estimates by Hemenway and Azrael (1997) exclude gun uses against animals and gun uses by people employed as police officers. DMI = Decision/Making/Information; NSDS = National Self-Defense Survey; NSPOF = National Survey of Private Ownership of Firearms.

* Survey question refers to whether anyone in the household has used a gun in self-defense.

† Survey question refers to whether respondent has used a gun in self-defense.
volving defensive uses of guns against persons happened to members of your household in the past five years?” and “Did this incident [any of these incidents] happen in the past twelve months?”

Using the detailed information available from their follow-up questions, Kleck and Gertz (1995) kept only those cases for which the respondent used a gun in a “meaningful way” against a person and for which the respondent identified the type of crime against which the gun was used in defense. Kleck and Gertz also excluded cases where the respondent was employed by the military, police, or private security. The results of the Kleck and Gertz survey, as shown in table 2, suggest 2.1–2.5 million defensive gun uses per year. Not surprisingly, considerable research attention has since been devoted to explaining the discrepancy between estimates from the NCVS and those from the one-time telephone surveys of Kleck and Gertz.

B. Convergence from Discrepancy?

What explains the discrepancy between the estimates from the NCVS and the cross-sectional telephone surveys, and which of these estimates is most relevant for public policy? One concern with the Kleck and Gertz (1995) findings raised by David Hemenway (1997b) is whether their survey design actually produced a nationally representative sample of American adults. While the NCVS regularly obtains response rates on the order of 90–95 percent (Bureau of Justice Statistics 1996), the Kleck and Gertz response rate is something less than 61 percent.4 Kleck and Gertz do report that their survey oversampled telephone numbers in the South and West, as well as men from within contacted households, but the procedures they use to project their responses up to population estimates are not clearly specified, and the weighted sample characteristics diverge somewhat from other published estimates (Hemenway 1997b).

As it turns out, sampling error does not appear to explain much of the difference between the NCVS and the Kleck and Gertz survey, since two more recent telephone surveys that used similar defensive gun-use questions to those employed by Kleck and Gertz produced similar results. The National Survey of Private Ownership of Firearms (NSPOF) was conducted in 1994 by Chilton Research Associates for

4 Kleck and Gertz (1995) report that responses were obtained in 61 percent of cases where a person rather than an answering machine answered the phone, which will overstate their response rate since presumably for some proportion of telephone numbers selected into the sampling frame the interviewers could not make contact with a person.
the National Institute of Justice (Cook and Ludwig 1996). The survey instrument was designed by Gary Kleck, Philip Cook, and David Hemenway and uses a sequence of defensive gun-use questions quite similar to those found with the 1993 Kleck and Gertz (1995) survey. Unlike the Kleck and Gertz survey, the NSPOF randomly selects one adult per sampled household, does not oversample telephone numbers from the South and West, and uses standard sample-weighting techniques (Cook and Ludwig 1996). The results support an estimate of 1.3 million defensive gun users each year (table 2), which is within the 95 percent confidence interval of the Kleck and Gertz estimate (Cook and Ludwig 1998). A more recent national survey sponsored by the National Institute of Justice suggests 1.5 million defensive gun uses per year (Hemenway and Azrael 1997, and forthcoming).

The discrepancies between the NCVS and these one-time phone surveys appear to stem instead from different susceptibilities to “false negatives,” cases where the respondent has used a gun in self-defense but conceals this from the survey interviewer, and “false positives,” where respondents falsely report that they have used a gun in self-defense when they have not. For different reasons, estimates from the NCVS are probably too low, while those obtained from the one-time telephone surveys appear to be too high.

1. Measurement Errors with the NCVS. That NCVS respondents are never directly asked whether they have used a gun in self-defense is likely to cause the NCVS to understate the prevalence of defensive gun use. The NCVS may suffer additional downward bias because some crimes such as trespassing are not included among the victimization questions that are followed by questions about defensive behaviors. Neither source of downward bias is present with the one-time telephone surveys reviewed in table 2, which directly ask all respondents whether they have used a gun in self-defense.

Because the NCVS provides an opportunity to report defensive gun use only to those respondents who report a criminal victimization, the NCVS may also omit cases where the respondent used a gun to prevent a crime before it occurred. The one-time surveys may be less susceptible to this problem because they ask respondents directly about gun self-defense and do not limit defensive gun-use questions to those who have reported a criminal victimization. Evidence to support this hypothesis comes from a study by McDowall, Loftin, and Presser (1999), who sponsored a survey in which half the sample is asked the NCVS sequence and then the sequence of questions from the Kleck
and Gertz survey, while the other half is asked these questions in the reverse order. The McDowell survey shows that respondents are much more likely to report defensive gun uses with the Kleck and Gertz questions than with the NCVS questions, regardless of the question order. Similar evidence comes from the 1994 NSPOF survey, in which only one-third of those who reported a defensive gun use also answered yes to questions earlier in the survey about whether the respondent had been the victim of a violent crime (Cook and Ludwig 1998).

Another source of downward bias with the NCVS suggested by Kleck and Gertz (1995) stems from the possibility that respondents may conceal their defensive gun uses from the government personnel who conduct these interviews. Because some respondents may be uncertain about the legality of their defensive gun use, "in the context of a non-anonymous survey conducted by the federal government, a respondent who reports a defensive gun use may believe that he is placing himself in legal jeopardy" (Kleck and Gertz 1995, pp. 155–56). This phenomenon can only contribute to the discrepancy between the NCVS and the one-time telephone surveys if respondents believe that the confidentiality promise made by private polling firms are more credible than those made by the NCVS interviewers. Yet the best available evidence seems to suggest, if anything, that survey respondents believe that interviews by census staff have greater legitimacy than other surveys (Smith 1997).

Forgetting is a source of downward bias with all surveys, though this is probably less of a problem with the NCVS than with the one-time surveys. The reason is that the NCVS asks respondents to report on defensive gun uses only during the past six months. The one-time telephone surveys ask respondents about defensive gun uses over a longer period of time (typically one to five years prior to the survey). Since memory failure is more likely for events that occurred longer ago in the past (Woltman, Bushery, and Carstensen 1984), proportionately more events may be omitted in the one-time surveys than in the NCVS.

Finally, all surveys will suffer from some measurement error (both false positives and false negatives) because a proportion of all respondents to any nationally representative survey may be unreliable reporters. Recent estimates from the U.S. Department of Health and Human Services suggest that 51.3 million American adults suffer from some form of mental or addictive disorder (Bourndon et al. 1994), many of whom are unlikely to provide reliable responses to social science sur-
veys. The possibility of unreliable reporters in the general population does not invalidate the use of social science surveys, but it does suggest that there will be some additional measurement error beyond sampling variability that complicates any effort to estimate very rare events such as defensive gun use. While there is no reason to believe that the proportion of unreliable reporters should be higher in the NCVS than in the one-time telephone surveys, the net effects of unreliable reporters may be greater with the one-time surveys because a larger proportion of the survey sample is asked to report on defensive gun uses (and thus has an opportunity to report unreliably).

2. Measurement Errors with One-Time Telephone Surveys. In contrast to the NCVS, the one-time telephone surveys analyzed by Kleck and Gertz (1995), Cook and Ludwig (1996, 1997, 1998), and Hemenway and Azrael (1997, and forthcoming) are more susceptible to upward bias from false positives. One reason is that the NCVS is less prone than the one-time surveys to false positives from “telescoping”—that is, when respondents report events that occurred outside of the survey question’s recall period—because the NCVS uses a panel design that interviews respondents in selected housing units every six months for a specified period. With the NCVS, each respondent is reminded of the criminal victimizations (including those involving defensive behavior) that he or she reported during the previous interview and is also reminded that these events should not be reported as part of the current series of questions about victimizations that occurred during the past six months. Since one-time telephone surveys obviously cannot bound respondent reports about defensive gun uses, telescoping is more of an issue than with the NCVS. Comparisons of bounded and unbounded estimates for criminal victimization from NCVS data suggest that the latter are approximately 30–40 percent higher than the former (Woltman, Bushery, and Carstensen 1984). Whether the degree of telescoping for self-reported defensive gun use should be higher or lower than for self-reported criminal victimization is not clear.

Another possible source of false positives comes from “social desirability bias,” which stems from the well-known tendency of respondents to present themselves favorably to survey interviewers. Respondents are more likely to report behaviors or attitudes that the survey interviewers may believe to be socially desirable, such as voting or seat belt use, and less likely to report on particulars that may be viewed as socially undesirable (Sudman and Bradburn 1974). If defensive gun use
is viewed as a socially desirable behavior, then social desirability bias works in the opposite direction of respondent concerns about the legality of their gun use. No systematic data are available on public attitudes toward defensive gun use. Yet suggestive evidence that at least some groups view gun self-defense as a socially desirable behavior comes from the fact that the National Rifle Association's publication the *American Rifleman* has published abbreviated newspaper accounts of defensive gun use since 1932. Social desirability bias could lead to false positives in the form of exaggerated accounts of events that actually happened or fabricated accounts of events that did not happen.

A final source of false positives comes, as noted above, from the possible presence of unreliable reporters in any nationally representative survey sample. The propensity of respondents to be unreliable reporters or to misreport because of social desirability bias may not be very different in the NCVS than in one-time surveys, though there is no empirical evidence on this point.

Thus the probability that a given respondent reports a false positive (the false-positive rate) will probably be higher in the one-time surveys than the NCVS because of telescoping. Whether the false-positive rate will differ across the two types of surveys because of social desirability or other reasons is less clear.

The false-positive rate can have a substantial effect on estimates from the one-time surveys, since defensive gun use by any measure is a relatively rare event, and thus even a modest false-positive rate may lead to substantial overestimates (Hemenway 1997a, 1997b). Consider, for example, a survey of 5,000 people in which the true prevalence of defensive gun use is 0.5 percent (i.e., half a percent of all adults use a gun in self-defense each year). With the one-time telephone surveys, every respondent is asked whether they have used a gun in self-defense. In this case, since 4,975 respondents have not used a gun in self-defense, the only reporting error available to this group is a false positive. However, only twenty-five respondents have actually used a gun in self-defense and thus can report a false negative. If the false-positive rate equals, say, 2 percent (because of telescoping, social desirability bias, or other reasons), the one-time survey will overstate the prevalence of defensive gun use by a factor of four even if the false-negative rate is 100 percent (i.e., none of those who have actually used a gun in

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self-defense report their gun use to the interviewers). Put differently, because the net bias in a survey is a function of the false-positive and false-negative rates as well as the true prevalence of the event of interest, a modest false-positive rate can produce substantial overestimates even if the false-negative rate is far higher than the rate of false positives.

The NCVS is less susceptible to this algebra of false positives because only those respondents who report a criminal victimization are given the opportunity to report a defensive gun use (Cook and Ludwig 1998). To continue with the example from above, suppose that none of the twenty-five survey participants who have actually used a gun in self-defense reports their use, so that the false-negative rate is once again 100 percent. Suppose also that 10 percent of the 5,000 survey respondents have been the victim of a crime during the survey recall period. In this case, only 500 respondents are provided with the opportunity to report a defensive gun use, so a false-positive rate of 2 percent only leads to a total of ten reports of defensive gun use. Because only crime victims are asked about defensive gun use in the NCVS, a 2 percent false-positive rate leads to an estimate of only 40 percent of the true number of defensive gun users in the sample, while the same false-positive rate leads to an estimated prevalence of gun self-defense that is four times the true figure in one-time surveys when the gun-use questions are administered to everyone in the sample.

3. One-Time Gun-Use Surveys: Internal and External Comparisons. The key empirical question with the one-time surveys is whether the false-positive rate is far enough from zero to produce a net upward bias in estimates for defensive gun use. Some indirect evidence of a net upward bias with the one-time telephone surveys comes from comparing the results of the 1994 NSPOF data with related phenomena estimated from other surveys or administrative data. For example, it is difficult to reconcile the NSPOF estimates for defensive gun use with crime victimization statistics estimated from the NCVS (Cook and Ludwig 1998). The defensive gun uses reported in the NSPOF imply that guns were used to defend against 322,000 rapes and 527,000 robberies in 1994. By contrast, estimates from the NCVS suggest that there were a total of 316,000 total rapes (including attempts) and 1.3 million robberies in 1994. The comparison suggests that an implausibly high proportion of crimes were defended against by gun owners, which in turn suggests that estimates for the prevalence of defensive gun use from the one-time NSPOF survey may be in error.
Kleck and Gertz (1995, p. 167) argue that comparisons of estimates for defensive gun uses from the one-time surveys and victimization estimates from the NCVS are not useful because “a large share [of defensive gun uses] are probably outside the scope of incidents that realistically are likely to be reported to either the NCVS or police.” In particular, Kleck and Gertz are concerned that the NCVS will not capture cases where the respondent is uncertain about whether the gun use was legal, for example, because the gun was carried or used in a public area (Kleck 1999). This concern can be addressed by focusing on those NSPOF respondents who report a defensive gun use and report that the police found out about the incident, which turns out to be about half of all NSPOF defensive gun-use reporters. The NSPOF thus implies that there were 265,000 attempted rapes and 141,000 attempted robberies in which the victim used a gun in self-defense and reported the incident to the police. By comparison, the FBI’s Uniform Crime Reports imply that the total number of rape and robbery attempts reported to the police in 1994 was 102,000 and 619,000, respectively. This comparison provides further support for the hypothesis that the one-time NSPOF survey overestimates the prevalence of defensive gun use, since the number of rape attempts defended against with a gun and reported to the police cannot logically exceed the total number of rape attempts reported to the police.6

A third test of external validity comes from comparing the number of criminal perpetrators who are shot by someone using a gun in self-defense, equal to 117,000 in 1994 according to respondents in the NSPOF survey (Cook and Ludwig 1996), with the estimated number of people who are shot and receive medical treatment each year in the emergency department, equal to around 150,000 in 1992–93 (Annest et al. 1995). This comparison lends itself to three possible interpretations. First, most of the people who are admitted to emergency departments in the United States who reportedly have been injured during the course of criminal assaults, suicide attempts, or unintentional shootings may in fact be criminals who are wounded during the course of committing crimes. This seems unlikely. Second, it is possible that most of the criminals who are shot by those using guns in self-defense

6 It is possible that the UCR undercounts the true number of crimes reported to the police nationwide for various reasons. Yet data from the NCVS suggest that a total of 719,000 robberies and 137,000 rapes and attempted rapes were reported to the police in 1994, which also suggest that the NSPOF estimates for defensive gun use are implausible.
do not seek treatment in the emergency department and, thus, are not reflected in the emergency department admission data. Yet 92 percent of incarcerated criminals who have been shot in the past report that they went to the hospital for treatment (May et al., forthcoming). The third and most plausible explanation is that the NSPOF-based estimate for the number of perpetrators wounded during the course of self-defense is too high. The Kleck and Gertz survey similarly reveals implausibly large numbers of wounded assailants and other inconsistencies with external benchmarks (Kleck and Gertz 1995; Hemenway 1997a, 1997b).

The internal consistency of defensive gun-use reports has also been cited as a measuring stick for the veracity of these accounts. Kleck and Gertz (1995, p. 179) argue that the false-positive hypothesis is implausible “since we asked as many as nineteen questions on the topic [of defensive gun use, and as a result misreporting] would entail spontaneously inventing as many as nineteen plausible and internally consistent bits of false information and doing so in a way that gave no hint to experienced interviewers that they were being deceived.” While Kleck and Gertz do not report on the details of their internal-consistency checks, there are some indications of internal inconsistencies in about a third of defensive gun-use reports in the 1994 NSPOF data (Cook and Ludwig 1998). For example, in one-fifth of the cases, the respondent indicated that a serious crime (rape, robbery, or attack) was involved but also reported that the perpetrator neither attacked nor issued a threat. These inconsistencies reflect either internal inconsistencies in the accounts of those who report defensive gun uses or coding errors by the interviewers. The latter seems unlikely to explain all of the apparent inconsistencies, given that senior survey staff monitored interviews and response coding for a random tenth of all calls throughout the study and because these inconsistencies arise in such a large proportion of all defensive gun-use cases.

Internal and external comparisons for the NSPOF data thus suggest that at least some of the details reported by respondents about their defensive gun uses are inconsistent with estimates derived from other sources. While estimates for external benchmarks such as the number of criminal victimizations, crimes reported to the police, or patients admitted to emergency departments are certainly subject to some error, these measurement errors would need to be far larger than is currently believed in order for the NSPOF reports to be consistent with these other facts. Similar inconsistencies between the defensive gun-
use reports in the Kleck and Gertz survey and other estimates have also been identified (Kleck and Gertz 1995; Hemenway 1997a, 1997b). These comparisons do not allow us to determine whether respondents are forgetting, exaggerating, or unintentionally distorting details about gun uses that actually happened or whether they are inventing these events altogether.

But whatever the cause, these apparent measurement errors suggest that the available data from one-time telephone surveys cannot be used to support precise estimates for the annual number of gun uses that are in society’s best interests. If the gun uses happened as reported but occurred outside of the survey recall period, estimates for the annual prevalence or incidence of defensive gun use will be overstated. If important details about the gun use are intentionally or unintentionally misreported, then analysts have little way of knowing whether the gun use that actually occurred produced a net benefit or cost from society’s perspective. And if some of these events were invented outright by the respondents, there is obviously no gun use to produce either a benefit or cost to society.

4. Narrowing the Range of Estimates. Analysts continue to debate whether the NCVS or the phone-survey estimates are more useful for public policy. Smith (1997) narrows the difference between the two sets of estimates by speculating on the magnitude of the biases that appear to afflict these figures. Smith inflates the latest NCVS estimate of 108,000 (Cook, Ludwig, and Hemenway 1997) by 50 percent to account for the fact that the NCVS does not directly ask about defensive gun use and, then, by another 16–42 percent to account for the fact that the NCVS excludes gun uses against trespassing and vandalism. Whether this second adjustment should be made is not clear, since the legality of using a gun to prevent a case of trespassing or vandalism is questionable. In any event, these two adjustments bring the revised NCVS estimate up to between 256,500 and 373,000. Smith also discounts the average of the smallest estimates reported by Kleck and Gertz (1995) and Cook and Ludwig (1996)—1.8 million—to account for telescoping, which yields an upper-bound estimate of 1.3 million.

While Smith’s exercise is useful in narrowing the magnitude of the discrepancy, the deeper problem comes from identifying which gun uses are clearly in society’s best interests (however that might be defined), and how these uses should be weighed against the annual number of gun misuses.
C. The Benefits of Defensive Gun Use

Estimates of the prevalence of defensive gun use are routinely compared with the number of gun crimes as a measure of the relative costs and benefits of private gun ownership or more or less restrictive gun policies (see, e.g., Kleck 1991; Kleck and Gertz 1995, 1997; and Lott 1999). Yet the policy implications of civilian defensive gun use will depend in large part on the benefits to society that result from these events, which are often difficult to identify from the available data.

One concern is that survey respondents may report a wide variety of behaviors as defensive gun uses, ranging from legitimate cases of self-defense where life and limb are at stake to more ambiguous cases, including those where the victim provokes the attack. Determining where particular gun uses fall on this continuum is frequently not possible from the available data. One problem is, as noted above, that some respondents may intentionally or unintentionally misreport some details of the events. Another problem is that the information that is available about the event is obtained from only one party to the encounter. For example, in around one-quarter of the gun uses reported in the NSPOF, the respondent indicated that the most serious crime involved in the incident was a fight or attack (Cook and Ludwig 1998). Whether a neutral observer would have classified the survey respondent as the victim in these confrontations cannot be determined from the available data. At a minimum, future surveys of gun self-defense should gather more detailed information about the sequence of events that leads up to the gun use in order to learn more about how the respondent contributes to the escalation of the encounter.

Identifying whether a gun use is in society’s best interests is further complicated by the possibility that some respondents may use their guns in defense in the expectation that someone may be seriously injured, though the actual outcome of the event is ambiguous. For example, consider a case where a car is stopped downtown at a red light late at night, a young man approaches the car and knocks on the window, the driver retrieves his handgun from the glove compartment in response, and the other party flees. Is this a thwarted criminal assault, or was the other party simply requesting directions or a ride to the nearest gas station for mechanical assistance? Determining whether this should be counted as a legitimate case of self-defense would require information about the other party’s intent. Whether a survey of the other party, or the presence of eyewitnesses, could reliably identify what this person was intending to do is by no means obvious, since if
the other party was intending to commit a crime, then he will have obvious incentives to misreport his intentions, while eyewitnesses will be in the difficult position of having to forecast the person’s future behavior on the basis of the set of events that they observe (which may be incomplete).

Another complicated scenario arises in the case of defensive gun uses by residents who move back and forth between the legal and illegal worlds. Such cases may account for a large number of defensive gun uses, since a large proportion of homicide victims have prior criminal histories (Kennedy, Piehl, and Braga 1996). While population-survey samples are likely to underrepresent members of this group (Cook 1985), there is nevertheless the problem that social science surveys do not record information about illegal activities associated with the defensive gun use or the gun user more generally.

Even if defensive gun uses associated with illegal activities could be identified, should they be counted as a benefit to society? Consider, for example, the case of a drug dealer who uses a gun to ward off a “client” who attempts to attack the dealer and make off with his supply of narcotics. Presumably some readers will object to the idea of counting this as a socially beneficial gun use, since it is associated with an illegal activity (drug selling). But what if a recreational drug user, after consuming narcotics within his home, uses a gun to ward off a burglar? What if an otherwise law-abiding citizen is carrying a concealed handgun in violation of local laws and then uses the weapon in self-defense, in order to prevent a robber from taking his wallet? Does the illegal-activity argument imply that both of these gun uses produce no benefit to society? If gun defenses by some “criminals” are judged to be benefits and others are not, what rules guide these distinctions?

Finally, the notion that a gun use produces some benefit to society implies that the outcome of the event is better in some sense than what would have happened to the respondent had he not used a gun. Yet nothing is known about what would have happened to the survey respondent had a gun not been available. Uncertainty about the counterfactual scenario can be divided into two components: Had the survey respondent not had access to a gun, how would the outcome of the event be affected once a hostile confrontation with the perpetrator was unavoidable? And, how does access to a firearm influence the likelihood that a would-be victim comes into contact with a perpetrator or escalates a confrontation? Because these questions cannot be answered from the available survey data, how many of those who report defen-
sive gun uses would have been victimized had a gun not been available remains unclear.

While there is a large research literature that examines the consequences of victim resistance with a gun once contact with a perpetrator is unavoidable, far too little is known on this point. Even the best available dataset for studying victim resistance, the NCVS, is inadequate for identifying what would have happened to the victim had some alternative defensive strategy been employed. One problem is that the NCVS does not provide detailed data about the sequence of robber and victim behavior, which makes it difficult to determine whether the victim’s actions improve the outcome of the event (Cook 1986; Kleck and DeLone 1993). For example, the victim may use force only in response to a robber’s attack or may try to resist in response to the robber’s threat and provoke an attack. Both cases will be recorded in the NCVS as ones in which both the victim and the robber use force.

More generally, when and how victims choose to resist is almost surely based in part on aspects of the encounter that are not measured by the NCVS. As a result, comparisons of victim outcomes will confound the causal effects of the victim’s behavior with the unobserved aspects of the event that contribute both to the victim’s decision to resist and to the probability that the resistance will be successful. For example, consider Kleck and DeLone’s (1993) state-of-the-art analysis of victim resistance in robberies using NCVS data for 1979–85. Kleck and DeLone use a multivariate regression approach to control for two dozen potentially confounding variables, including the victim’s age, whether the victim was employed by the police or otherwise trained in the use of a gun, whether the incident occurred “when it was dark,” and whether the incident occurred in a “private location.” Now consider the following scenarios:

A thirty-year-old economist, standing 5’9” and 150 pounds, confronts a physically imposing, determined perpetrator in a dark alley at 11:30 p.m. The perpetrator attacks, the economist resists by yelling and punching the perpetrator but is nevertheless injured as a result of the assault.

A thirty-year-old graduate student who works part time as a bouncer, standing 6’9” and 350 pounds, confronts a slightly built perpetrator shortly after dusk near a heavily trafficked entrance in a shopping mall parking lot. The perpetrator is nervous, scared, and inebriated. The perpetrator attacks, the bouncer fends off the
attacker while retrieving a handgun from the glove compartment of his car, and the perpetrator flees.

Despite the relatively large number of covariates included in the Kleck and DeLone (1993) regression models, their analysis nevertheless treats the two cases described above as observationally equivalent in all respects other than the defensive behavior of the victim and the outcome of the event. Yet presumably most readers will agree that there is some ambiguity about how the presence of a gun contributed to the outcome of the second case.

The literature on victim resistance described above ignores the more general question of victimization avoidance. Studies of victim resistance typically examine the effects of victim behavior conditional on there being a hostile confrontation between the victim and the perpetrator. Yet whether such a confrontation occurs may stem in part from the decisions made by the victim and may be influenced by the availability of a firearm. For example, armed citizens may escalate verbal arguments that otherwise might be defused or ignored or may choose to walk dark streets that would otherwise have been avoided. While the avoidance of certain parts of town at certain parts of the day or night surely imposes some cost on citizens, these costs are very different from those associated with a criminal victimization.

The hypothesis that guns may induce "compensating risks" or "offsetting behavior" is motivated in part by research showing similar effects in other contexts. For example, improvements in automobile safety design have been found to lead to riskier driving (Peltzman 1975; Crandall and Graham 1984; Traynor 1993), the introduction of child-resistant packaging for aspirin leads to more careless storage of aspirin bottles by parents (Viscusi 1984), and the frequency of unsafe sex practices among homosexual men seems to increase when the incidence of sexually transmitted diseases decreases (Philipson and Posner 1993). Some direct support for offsetting behavior in the case of gun use comes from the 1994 NSPOF survey data. In one-third of the defensive gun use cases reported in the NSPOF, the respondent indicates that the encounter occurred "near the respondent's home" and also indicates that, when he first wanted to use his gun for protection, the gun was somewhere in the home (Cook and Ludwig 1998). These figures suggest that one-third of all gun defenders had the option of staying inside and calling the police rather than confronting the perpetrator.
D. Conclusions

Surveys of defensive gun use, as with any survey, will almost surely contain both false positives and false negatives. But because defensive gun use by any measure is a relatively rare event, far more respondents have the opportunity to report a false positive than a false negative in the one-time telephone surveys that ask every respondent about defensive gun use. As a result, a relatively low false-positive rate can lead to substantial overestimates of defensive gun use with these surveys even if the false-negative rate is far higher. Some comparisons of the results from the one-time surveys analyzed by Cook and Ludwig (1996, 1997, 1998) and Kleck and Gertz (1995) with other estimates provide some evidence to suggest the point estimates for the prevalence of defensive gun use suffer from an upward bias. However, since only respondents who have reported a criminal victimization are asked about defensive gun use in the NCVS, the relative influence of false positives is far less than in the one-time surveys. False negatives are thus likely to be more important with the NCVS and may lead to a downward bias in estimates for defensive gun use derived from this data source.

The larger difficulties arise in determining how gun uses contribute to public well-being. In some cases, the information necessary for determining whether the survey respondent is the victim or the aggressor in the gun use is not available. More generally, the consequences of gun use cannot be understood without some understanding of whether gun use is more effective than other means of resistance against criminal attack, about which little is currently known. Access to firearms may also affect the probability that a confrontation occurs, as is suggested by evidence from the NSPFO survey. And surveys of defensive gun use provide no information about perhaps the most important benefit of this behavior—the deterrent effect on criminals.

Determining whether a gun use is in the public’s best interest also requires answers to difficult normative questions that cannot be answered by social science. Most people will agree that the use of a gun in self-defense when a serious injury is clearly imminent is uncontroversially beneficial to society, assuming that the victim did not contribute to the confrontation in some way. But what if the gun user initiated the incident by engaging in aggressive driving? What if the other party only threatened to attack, or the gun user inferred that the party might attack? What if a gun is used in response to a nonfatal or avoidable attack? What if the gun is used to prevent trespassing or to protect property rather than life and limb? These are difficult questions to an-
answer, as evidenced by the ambivalence with which the public frequently receives the use of guns by law-enforcement officers during the course of their duties, even in cases where police report that they were concerned for their safety.

Estimates for the number of defensive gun uses that are in society's best interests are frequently used as measures of the potential costs of implementing different gun regulations. But these regulations may also produce benefits that must be weighed against these costs, which include any reductions in fatal and nonfatal injuries that may result. The benefits of gun regulations may also include reductions in the number of hostile or inappropriate gun brandishings, which may exceed both the number of gun crimes reported in the NCVS and the number of defensive gun uses reported in one-time telephone surveys (Hemenway and Azrael 1997, and forthcoming).

Moreover, estimates of the frequency of socially desirable and undesirable gun uses under the status quo provide little information about how the prevalence of these behaviors would change in response to specific public policies. The answer is not obvious for most gun policies. For example, requirements that all handgun sales involve a three- or five-day waiting period may prevent some impulsive suicides but may also prevent some people who are being stalked from obtaining guns for their personal protection. No one knows how waiting periods affect the prevalence of either event, even though it is the change in these behaviors produced by a policy rather than their prevalence under the status quo that is most relevant for evaluating gun policies. In other words, a gun policy that prevents 1,000 crimes and suicide attempts each year and has no effect on defensive gun use or deterrence can arguably be judged to be successful, regardless of whether the number of defensive gun uses is currently 100,000 or 2.5 million. The most promising approach for understanding the implications of defensive gun use and deterrence for public policy is to examine directly the net effects of gun policies on the ultimate outcomes of interest—rates of crime and injury.

III. The Effects of Gun-Carrying Laws on Crime

The burgeoning research literature on the defensive and deterrent value of private gun ownership has motivated substantial scholarly interest in the effects of permissive concealed-gun carrying laws (or, equivalently, "concealed carry laws"), which seek to enhance the ability of citizens to use guns in self-defense. In particular, the influential
evaluations conducted by John Lott and his colleagues (Lott and Mustard 1997; Bronars and Lott 1998; Lott 1998b; Lott and Landes 1999) have contributed to the growing perception that the deterrent effects of defensive gun use are substantial and that concealed-carry laws produce substantial reductions in violent crime.

A. Permissive Concealed-Carry Laws and Their Potential Effects

Within the past five years, ten states have enacted permissive concealed-gun carrying laws, bringing the total number of states with such laws on the books to thirty-one.7 These laws require local law enforcement authorities to issue a permit to carry a concealed handgun in public to any adult who meets a minimal set of criteria related to criminal background, mental competence, and, in some states, training in the handling of firearms. (These are also known as “shall-issue” laws because of the requirement that local police issue permits to any qualified adult.) Permissive concealed-carry laws represent a sharp departure from the prior legal regime that had been in place in most of these states, under which local police were given considerable discretion over the issuance of concealed-carry permits (“may-issue”). Since police had typically been quite restrictive in the issuance of concealed-carry permits (McDowall, Loftin, and Wiersema 1995), the more permissive state-level concealed-carry laws have the potential to increase substantially the proportion of adults who are legally licensed to carry a concealed handgun in public areas.

The degree to which the prevalence of gun carrying actually increases in response to these laws remains somewhat unclear. One recent review suggests that in twelve of sixteen permissive concealed-carry states that were studied, fewer than 2 percent of adults had obtained permits to carry concealed handguns (Hill 1997). However, the change in the number of concealed-carry permits issued will be different from the change in the overall prevalence of gun carrying, since many people apparently take guns into public areas in the United States without benefit of a permit. For example, survey data from the 1994 NSPOF suggests that 7.5 percent of American adults carried a firearm on their person or in a motor vehicle at some point during the

7 The ten states are Alaska, Arkansas, Kentucky, Louisiana, Nevada, North Carolina, Oklahoma, South Carolina, Texas, and Virginia. The total number of states is taken from the National Rifle Association Institute for Legislative Action, Compendium of State Firearm Laws, www.nraila.org, downloaded November 20, 1999.
year (Cook and Ludwig 1996). Because many people who obtain permits have already been carrying guns in public, the change in the prevalence of gun carrying may be smaller than the number of permits issued. For example, 85 percent of those with concealed-carry permits in North Carolina who carry a gun in their car and 34 percent of those who carry on their person did so even before they obtained a permit, and a majority of both groups indicate that the frequency of their gun carrying did not increase with the acquisition of the permit (Robuck-Mangum 1997). Thus the data that are available suggest that permissive concealed-carry laws may have only a modest effect on the overall prevalence of gun carrying.

Assuming that permissive concealed-carry laws increase the prevalence of gun carrying, one consequence may be an increase in the frequency of defensive gun use. The net effects of more gun self-defense on injury rates will depend, in part, on the advantages of using a gun rather than other methods of avoiding criminal victimization and injury and, in part, on the degree to which law-abiding citizens change their risk-avoiding behaviors. Perhaps the most important benefit from an increase in gun carrying may be a general deterrent effect on criminal behavior (Lott and Mustard 1997). Some support for the plausibility of a deterrent effect comes from Wright and Rossi's (1994) interviews of incarcerated felons. Around 80 percent of the respondents agreed with the statement that "a smart criminal always tries to find out if his potential victim is armed," and around two-fifths of those who agreed with this statement also report that at some point in their lives they had decided to not commit a particular crime because they "knew or believed that the victim was carrying a gun" (Wright and Rossi 1994, p. 147). These findings are consistent with evidence that the threat of apprehension and punishment from the criminal justice system has a deterrent effect on criminal behavior (Nagin 1998).

Even if there is little or no change in actual gun carrying from the implementation of a permissive concealed-carry law, crime may decrease if criminals perceive the law to have increased the risks of encountering an armed victim. This could happen if criminals update their perceptions of the prevalence of gun carrying in response to the publicity surrounding the implementation of a permissive concealed-carry law, what Zimring and Hawkins (1997) term an "announcement effect." The possibility that the perceived probability of punishment may diverge from the actual probability, at least in the short run, is
suggested by evidence that reductions in drunk driving persist through a "residual deterrent" effect even after local police crackdowns have ended (Sherman 1992).

The most obvious potential cost of permissive concealed-carry laws comes from the possibility that those who obtain concealed-carry permits will misuse their guns. Another potential cost that is frequently ignored in policy debates comes from the possibility of undesirable changes in the behavior of criminals in response to these laws. For example, nearly two-thirds of the incarcerated criminals interviewed by Wright and Rossi (1994) who used guns to commit their crimes reported that the prospect of encountering an armed victim was very or somewhat important in their decision to carry a gun themselves. Since guns are currently used in "only" 21 percent of all robberies, 4 percent of sexual assaults and rapes, and 7 percent of assaults (Rennison 1999), permissive concealed-carry laws may have the negative consequence of increasing the proportion of criminals who carry guns to commit their crimes. Increases in the proportion of crimes committed with a gun may in turn increase the proportion of crimes that result in fatal injuries (Cook 1991). Another possibility is that if the probability of encountering an armed victim increases, criminals may be more likely to use their weapons, whether they be guns, knives, or something else entirely, in order to gain a first-mover advantage.

Because both positive and negative effects are plausible from concealed-carry laws, empirical research is required to determine whether the net effects of these laws are to increase or decrease rates of crime and injury. While the sign of the effect from these laws is ambiguous, the magnitude of the effect is probably modest given that concealed-carry laws may have only nominal effects on the prevalence of gun carrying in public spaces. Moreover, increases in gun carrying appear to be concentrated disproportionately in areas where crime rates are quite low, among people who are already at relatively low risk of victimization. Data from the NCVS suggest that victimizations from violent crime are most frequent among low-income, never-married residents of urban areas (Rennison 1999). Yet in Texas and North Carolina, around three-quarters of those who obtain a concealed-carry permit are over the age of forty, almost all are white, and over half live in rural areas (Hill 1997). Permit holders in North Carolina are more likely to be married or college educated relative to other state residents (Robuck-Mangum 1997). The effects from concealed-carry laws may
be limited if the prevalence of gun-carrying changes primarily in areas away from where most crimes occur.

B. The Evaluation Problem

The causal effect of an intervention such as a permissive concealed-carry law is defined as the difference between the crime rate that is actually observed in a jurisdiction with a permissive concealed-carry law and the crime rate that the jurisdiction would have experienced had the law not been implemented. The challenge for researchers is that the crime rate under the counterfactual scenario in which the state does not enact the law is not observed. (This is true by definition, since “counterfactual” refers to outcomes that could potentially have occurred in place of the events that were actually observed but did not.) Researchers in medicine, psychology, and other disciplines frequently try to infer the counterfactual outcomes that a particular treatment group would have experienced by conducting randomized clinical trials, known in social science circles as randomized experiments. Since participants who volunteer for an experiment are randomly assigned into the treatment or control group, the baseline health measures and other characteristics of those in the two groups should be similar. As a result, the health outcomes observed for the controls at the end of the experiment should provide an unbiased estimate of what the health outcomes for the treatment group would have been had they not been given the treatment. The contrast in health outcomes between the treatment and control groups provide an unbiased estimate for the effects of the treatment. Thus the keys to valid inference with randomized clinical trials is that analysts choose who receives the treatment, and because of random assignment the outcomes observed for the control group are good proxies for what would have happened to the treatment group had they not received the treatment.

Of course in some situations experiments are not possible because of ethical, financial, or other practical considerations. In such cases, analysts attempt to draw inferences about the outcomes the treatment group would have had if they had not received the treatment using different statistical procedures. The difficulties of this task can be seen by considering the case of using nonexperimental data to evaluate the effects of doctors’ office visits on health outcomes for people with influenza. The problem is that for flu victims who visit a doctor, we do not observe what their health outcomes would have been had they not
sought treatment, while for those who do not see a doctor we do not know what their health outcomes would have been had they done so. The goal of statistical analysis is thus to draw inferences about what would have happened to each observational unit under the alternative scenario of treatment (or nontreatment). The two most common methods for doing this are “pre/post” comparisons, where the analyst assumes that the person’s health outcomes without the treatment will equal (or can be forecast from) her health status before she seeks treatment, and comparisons of people who do and do not voluntarily seek treatment.

One problem with pre/post comparisons is that if patients tend to visit the doctor only after they are sick, we may mistakenly conclude that visiting the doctor causes the flu (depending on when the pre- and post-health measures are recorded). A related challenge stems from the fact that influenza spells are typically of finite duration; since patients will eventually recover whether or not they seek medical treatment, we may mistakenly conclude that doctor visits “cure” the flu, even if they have no effect. (This alternative hypothesis is expressed by the old saying, “A cold will last seven days if one visits the doctor, and otherwise will last a week.”)

Comparing health outcomes of people with the flu who have visited versus those who have not visited the doctor can help control for the fact that flu spells are of finite duration, though such comparisons may still be biased if the reasons that cause some people but not others to visit the doctor are related to health outcomes. For example, patients who seek out the doctor may have more severe cases of the flu than those who do not, which might lead us to conclude that doctors exacerbate the severity of the illness. Alternatively, visits to the doctor may be more likely among patients who care about their health and, thus, may recovery more quickly because of their general fitness or because they initiate supplemental treatment activities on their own (such as extra bed rest or time off from work).

Evaluating the effects of concealed-carry laws involves almost all of the same complications raised in the example above. As with influenza patients, states choose whether and when to implement the treatment (concealed-carry legislation). These decisions may in part be a consequence of the outcome measure of interest, since states may enact concealed-carry laws in response to changes in local crime rates. And as with the flu, increases in crime rates are of finite duration—data for the United States as a whole show that periods of increasing crime are
regularly followed by periods in which crime rates decrease (Blumstein 1995; Philipson and Posner 1996), and similar patterns can be observed at the state level as well. States that choose to enact permissive concealed-carry laws may be different from other states in ways that are relevant for the local crime rate (e.g., with respect to demographic or economic characteristics of the local population) or may supplement concealed-carry laws with other anticrime measures that will confound attempts to isolate the effects of the gun-carrying intervention. Thus the evaluation of concealed-carry laws requires the difficult task of disentangling the causal effects of a self-imposed treatment that may be both cause and consequence of the outcome of interest.

C. Pre/Post Comparisons

One of the first systematic scholarly attempts to evaluate the effects of concealed-carry laws was conducted by David McDowall, Colin Loftin, and Brian Wiersema (1995). Their study uses data from 1973 through 1992 for five urban counties in Florida, Mississippi, and Oregon, each of which implemented a permissive concealed-carry law during the sample period (October 1, 1987; July 1, 1990; and January 1, 1990, respectively). The key outcome variables for the evaluation are gun and nongun homicides, as recorded by the vital statistics system of the National Center for Health Statistics, and thus should be fairly reliable measures of the rate of lethal violence in these treatment areas.

McDowall and colleagues (1995) attempt to measure the effects of concealed-carry laws by comparing the crime rate in each county after the area's concealed-carry law has gone into effect with the crime rate that the county would have had if the law had not gone into effect, as predicted from the area's crime rates before the law had gone into effect. Their evaluation approach thus assumes that, had these concealed-carry laws not gone into effect, each county's crime rate would have followed the same historical trend as was observed during the prelaw period. The results of the analysis suggest large increases in gun homicides in four of the five counties that they examine. In three of the four counties where gun homicides increased, the effects are enormous (on the order of 20–75 percent) and statistically significant at the conventional 5 percent level. They find no systematic evidence of changes in nongun homicides in these areas.

The central concern with the evaluation by McDowell, Loftin, and Wiersema (1995) is the possibility that the crime rates in these cities may deviate from their historical trends for a number of reasons other
than the implementation of concealed-carry laws. For example, beginning in the mid-1980s the use and distribution of crack cocaine is thought to have increased substantially in American cities. This increase in crack is thought to have contributed to an increase in gun violence, as more young people became involved in the distribution of drugs and began to use guns to enforce the terms of these drug transactions (Blumstein 1995; Blumstein and Cork 1996; Cork 1999). McDowall and colleagues attempt to control for the effects of unexpected changes in crime rates by replicating their analysis in different cities, with the expectation that "if similar outcomes occur in several different places after the laws, historical events become a less plausible explanation of the change" (1995, p. 199). Yet the timing of the concealed-carry laws in Florida, Mississippi, and Oregon coincide almost exactly to periods when crack arrests (and juvenile gun homicides) surged (Cork 1999).^8

While crack may or may not explain the findings by McDowall and his colleagues, the surge in crack activity around the time of these laws highlights the potential problems of drawing inferences about the effects of concealed-carry laws using only crime data for counties that implemented such laws. Information about crime trends in nearby areas that did not enact permissive concealed-carry laws would help eliminate alternative explanations for the authors' pattern of findings. An example of the advantages of using data for control jurisdictions that did not enact whatever legal intervention is being studied comes from the evaluation of a law enacted in Gainesville, Florida, in the mid-1980s that required convenience stores to employ two clerks per shift to deter robberies (Sherman 1992). Initial evaluations found that convenience-store robberies decreased substantially in Gainesville after these laws were enacted. Yet a reanalysis found similar reductions in convenience store robberies in a nearby county that had not enacted such a law, suggesting that some factor other than the two-clerk requirement was responsible for the change in robberies in both Gainesville and the nearby county.

In sum, it is difficult to draw causal inferences from the McDowall

^8 McDowall, Loftin, and Wiersema (1995) also attempt to control for unexpected changes in each treatment area's crime rate over time by including in their regression models a control for the overall U.S. homicide rate. But this will only partially control for the effects of innovations like crack if, as seems plausible, these innovations have greater effects on crime rates in cities than in suburbs and rural areas, so that the urban areas examined by McDowall, Loftin, and Wiersema experience increases in crime rates relative to the nation as a whole.
et al. findings without information about what happened in similar jurisdictions that did not implement concealed-carry laws during the sample period. This unresolved identification problem, together with their provocative findings that concealed-carry laws may substantially increase rates of gun violence, helped generate considerable demand for additional empirical work on this topic, which the academic research market soon filled.

D. The Lott and Mustard Study

An important step forward came with the study by Lott and Mustard (1997), who provided the first attempt to examine the effects of concealed-carry laws using national data on both treatment and control jurisdictions. Their study also exploits the repeated cross-section nature of the dataset (multiple observations for each state over time) in an attempt to control for difficult to measure differences between treatment and control areas. While the Lott and Mustard study has in many ways set a new standard for the empirical evaluation of concealed-carry legislation (and other anticrime interventions), there nevertheless remains some question about whether their analysis has identified the causal effects of these laws.

Lott and Mustard’s (1997) dataset consists of county-level observations for the entire United States for the period 1977–92. The authors analyze the effects of concealed-carry laws on different types of crime (violent and property crime rates, as well as disaggregated crime rates for murder, rape, aggravated assault, robbery, burglary, larceny, and auto theft) because concealed-carry laws are likely to have their greatest effect in deterring crimes that involve contact between the perpetrator and the victim, most of which are violent crimes. The dependent variables in the analysis equal the natural logarithm of the county’s crime rate per 100,000 people, which has the advantage of ensuring that the predicted crime rates produced by their linear regression models are nonnegative. In counties with no crimes of a particular type in a given year, Lott and Mustard define the value of the dependent value to equal the logarithm of 0.1 since the log of zero is undefined. The choice of substituting 0.1 for 0 before taking the log of the crime rate for these counties is arbitrary and could in principle have some non-trivial effect on the empirical results. In practice this seems to have little effect on the Lott and Mustard estimates, since Black and Nagin (1998) obtain similar results when they restrict the analytic sample to
large counties (with populations of 100,000 or more) where zero crime
counts are probably not very common.

The crime rate data come from information that local police depart-
ments voluntarily report to the FBI's UCR system. The UCR suffers
from a number of well-known sources of measurement error, including
nonreporting, incomplete reporting, or underreporting by some juris-
dictions for particular years (Biderman and Lynch 1991; Maltz 1999).
For these and other reasons, the Lott and Mustard (1997) dataset does
not include any crime data for Florida for 1988 (the year after the
state's concealed-carry law went into effect) or violent crime figures for
a number of other large urban areas for a number of years. Other
problems with the UCR data include variation across jurisdictions and
time in the definitions that police departments use to classify different
crimes and variation in the propensity of victims to report crime to the
police (Biderman and Lynch 1991). Because there is little discretion in
how police may classify homicides, and because police are made aware
of most homicides (Cook and Laub 1998), Lott and Mustard's analysis
is probably more reliable for homicide than for other crimes.

Lott and Mustard (1997) draw inferences about the effects of
concealed-carry laws by comparing the outcomes of counties having
concealed-carry laws with those in counties without such legislation
while controlling for state "fixed effects" as well as a number of other
sociodemographic and other variables that may differ across states.\footnote{While there
remains some ambiguity about whether Lott and Mustard (1997) have
correctly identified the timing of the concealed-carry laws implemented by different
states during the 1977–92 period (Webster et al. 1997), the findings generally appear
to be robust to alternative decisions about the timing of these laws (Lott and Mustard
1997).}
The use of state fixed effects is designed to control for difficult-to-
measure, time-invariant factors that cause some states to have persist-
tently higher crime rates than others and essentially involves compar-
ing the crime-rate trends between treatment and control areas. The
intuition behind this approach can be seen by considering the case of
Idaho, a state that enacted a permissive concealed-carry law in 1990,
and California, a nearby state that did not enact such a law. In 1990,
the first year in which Idaho's law was in effect, the homicide rate per
100,000 residents equaled 12.4 in California and 2.5 in Idaho. Part of
the difference in homicide rates between California and Idaho may be
due to Idaho's permissive concealed-carry law, but presumably part of
the homicide disparity is also due to other differences between the
states. In 1989, the year before Idaho’s concealed-carry law went into effect, the homicide rates in California and Idaho equaled 10.9 and 2.9, respectively. The fixed-effects analysis helps overcome the problem that crime rates may be higher in California than Idaho for reasons that have nothing to do with Idaho’s gun laws by comparing the changes in the crime rates in the two states over the same period of time. A fixed-effects analysis thus essentially consists of comparing the change in California’s homicide rate over this period \((12.4 - 10.9 = 1.5)\) with the change observed in Idaho \((2.5 - 2.9 = -0.4)\), for an estimated treatment effect of \((-0.4 - 1.5 = -1.9)\).

The use of state and year fixed effects in Lott and Mustard’s (1997) analysis represents a substantial improvement over previous evaluations, since this procedure has the potential to control for the effects of at least some kinds of unmeasured variables that cause crime rates to differ across counties and states. The critical assumption is that California and Idaho would have had similar trends in homicide rates during this period had Idaho not enacted a concealed-carry law. This assumption will be violated if some criminogenic factors changed more substantially in California than in Idaho around the time of Idaho’s concealed-carry law. To control for this possibility, Lott and Mustard include in their fixed-effects regression models a number of variables that measure sociodemographic and policy conditions that may change over time. The control variables thus play a critical role even with the fixed-effects research design, since Lott and Mustard assume that whatever differences in crime trends remain between treatment and control jurisdictions after adjusting for these other variables must reflect the effects of concealed-carry laws. If there are unmeasured factors that would have caused California and Idaho to have different crime trends even if Idaho had not enacted a concealed-carry law, Lott and Mustard’s estimates will be biased.

Their controls for sociodemographic and economic variables include population density (people per square mile), per capita income, and the proportion of the county’s population that falls into different sex/age/race groups (e.g., the proportion of people who are white females between the ages of thirty and thirty-nine). Since poverty-rate information is not available at the county level, Lott and Mustard try to proxy for the local poverty rate by including measures of county spending on social programs (unemployment insurance, income maintenance, and retirement payments) per county resident. These spending variables are unlikely to be useful proxies for the degree of material deprivation
within a county, since per capita social spending could be either positively or negatively correlated with the local poverty rate. To see this, consider a county with 100 residents and per capita expenditures on social programs of $500. This level of social spending is consistent with conditions in both an affluent county in which only one family is eligible for assistance and receives $50,000 in benefits and a destitute county in which all 100 residents live in poverty and receive $500 each. Lott and Mustard (1997, p. 24) also experiment with a measure of local cocaine prices to control for the prevalence of drug market activity but report that using this variable "removes observations during a couple of important years during which changes are occurring in concealed handgun laws."

The decision whether and when to implement a concealed-carry law is ultimately a political one, and the same public concern about crime that contributes to the enactment of a gun-carrying law may also lead to other anticrime measures. Lott and Mustard attempt to control for concurrent changes in the criminal justice system by including in their regression models variables for whether states have sentencing add-ons for crimes committed with deadly weapons and waiting period requirements for handgun purchases.

To proxy for unmeasured differences in the effectiveness of local law enforcement, Lott and Mustard (1997) also include the county's arrest rate (number of arrests divided by number of crimes reported to the police) as a control variable. The problems of using the arrest rate as a right-hand-side control variable are well-known: unmeasured variables that affect the dependent variable (the number of crimes reported to the police) will produce a spurious negative correlation with the arrest rate, since the number of crimes reported to the police serves as the denominator for the arrest rate (Nagin 1978). An upper bound on how much influence the arrest rate has on the results is provided by Lott and Mustard's reanalysis of their regression models using only state and year fixed effects, excluding the arrest rate as well as all of the other control variables. Omitting all of these control variables reduces the magnitude of the concealed-carry effect on violent crime by around 50 percent, though this effect is still close to statistically significant with a $t$-statistic of 1.66 (Lott and Mustard 1997, p. 19).

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10 For example, suppose that the number of crimes in a county increases for some reason that is not modelled by the regression analysis. As the number of crimes (the dependent variable in the regression) increases, the arrest rate (a right-hand-side variable) will decrease, since the increase in the number of crimes serves to inflate the denominator of the arrest rate.
The estimates for which Lott and Mustard present cost-benefit calculations (which presumably are thus their preferred results) suggest that permissive concealed-carry laws reduce homicides by nearly 8 percent, rapes by 5 percent, and aggravated assaults by 7 percent and increase property crimes by around 3 percent. Each of these estimated effects is statistically significant at the conventional 5 percent threshold. The authors attribute the observed increase in property crimes to decisions by criminals to substitute property for violent crimes, in order to reduce the probability of encountering a potentially armed victim. Using estimates for the costs of crime calculated by Miller, Cohen, and Wiersema (1996), Lott and Mustard (1997) suggest that if states without concealed-carry laws had enacted such laws, the result would have been an improvement in social welfare equal to $5.7 billion in 1992 dollars.

Lott and Mustard conduct sensitivity analysis that is far more extensive than is usually found within criminology, and conclude that their findings are generally robust to changes in their statistical procedures (such as including burglary and robbery rates as explanatory variables to control for unmeasured criminogenic factors in their analysis of other property and violent crimes).

The one exception comes from Lott and Mustard’s attempt to estimate a more elaborate “instrumental variables” model, which requires them to identify factors that explain why some states enact concealed-carry laws and others do not. In order for the resultant estimates to be valid, these factors must otherwise be uncorrelated with crime rates. Problems with some of the factors identified by Lott and Mustard have been discussed in Nagin (1978). While standard diagnostic statistics can be easily calculated to test whether these assumptions are met (Hausman 1983; Newey 1985), such statistics are not included in the Lott and Mustard study. Some evidence that the conditions for unbiased estimation are not met comes from the implausibly large magnitude of the resultant estimates: Lott and Mustard’s two-stage least squares models suggest that permissive concealed-carry laws reduce homicides by 67 percent and rapes by 65 percent (Black and Nagin 1998). Lott and Mustard do not indicate whether they believe these two-stage estimates to be plausible or whether these estimates should be preferred to the other fixed-effects estimates that are one-tenth as large.

Lott’s 1998 book also reports on two extensions to his study with Mustard (Lott 1998b). The first extension, also published as a separate
article with Stephen Bronars (Bronars and Lott 1998), examines the possibility that the deterrent effects of concealed-carry laws will cause criminals to relocate their activities to neighboring jurisdictions where the risks of encountering an armed victim are presumably lower. Bronars and Lott test this hypothesis by examining whether crime rates changed in counties located within fifty miles of states that adopted concealed-carry laws, using a slight modification of the fixed-effects regression approach used in Lott and Mustard (1997). Their analysis finds a positive correlation between implementation of a concealed-carry law within a given state and the homicide, robbery, and rape rates in neighboring counties and a negative correlation with rates of aggravated assault.

The second extension by Lott and William Landes (1999) also uses the same fixed-effects model specifications employed by Lott and Mustard (1997), but now focuses on multiple-victim shootings as the dependent variable of interest. Lott and Landes use a Lexis-Nexis search of newspaper accounts for the period 1977–95 to identify the number of “multiple victim shootings” in each state per year, defined as shootings in which two or more people are wounded in a church, business, bar, street, workplace, park, or other public place. They exclude shootings that occurred as part of another crime (such as a robbery or drug deal), as well as shootings associated with gang activity, a serial-killing spree (where the shootings occur over the span of two or more days), or “professional hits.” As it turns out, the events that are both recorded in Lexis-Nexis and meet the Lott and Landes criteria turn out to be relatively rare. For example, a number of states (including Delaware, Nevada, and Tennessee) had no multiple-victim shootings according to the Lott and Landes data during the nineteen-year sample period, while Washington, D.C., had a total of two such shootings.

The Lott and Landes (1999) fixed-effects regressions suggest that implementation of a permissive concealed-carry law reduces the rate of murders and injuries in multiple-victim shootings by 0.111 murders/injuries per 100,000 residents. For comparison, the average rate of murders and injuries from multiple-victim shootings per 100,000 people during the 1977–95 period in the Lott and Landes dataset is 0.038 for the nation as a whole, and 0.042 in those states that do not enact concealed-carry laws. Thus the Lott and Landes study suggests that permissive concealed-carry laws have a treatment effect that is equal to around 300 percent of the mean murder and injury rate from multiple-victim shootings in the control states. On the basis of these findings,
Lott has suggested that state policy makers should encourage teachers to carry concealed handguns in schools in order to prevent mass shootings (Lott 1998c).

E. Critiques and Reassessments of Lott and Mustard

The primary concern with the Lott and Mustard (1997) study is whether those authors have isolated the causal effects of concealed-carry laws on crime or whether, instead, their estimates are due in part or whole to other difficult-to-measure factors that cause the treatment and control states to have different crime trends. Evidence on this question comes from a reanalysis of Lott and Mustard’s data by Dan Black and Daniel Nagin (1998), who examine the central assumption behind the Lott and Mustard study—that after controlling for the variables included in the multivariate regression models, the treatment and control states would have had similar trends in crime rates had the treatment areas not enacted permissive concealed-carry laws. This assumption cannot be tested directly, since we cannot observe what the crime trend would have been in treatment states had they not enacted their concealed-carry laws. But the assumption can be tested indirectly, by examining whether the treatment and control states have similar crime trends before the treatment states enact their laws. Put differently, Black and Nagin test whether the Lott and Mustard models provide evidence of a “treatment effect” between treatment and control states even before the treatment is implemented. This kind of pretreatment difference would suggest that part or all of the difference in crime trends during the postprogram period are a result of factors that are not captured by Lott and Mustard’s model.

Black and Nagin’s (1998) reanalysis found that violent crime rates were increasing more rapidly in treatment than in control states before the treatment states enacted concealed-carry laws and that these pre-concealed-carry-law differences are statistically significant.11 The Black and Nagin findings suggest, perhaps not surprisingly, that treatment states enact permissive concealed-carry laws in response to increases in crime. Because crime rates in the United States are cyclical, with periods of increasing crime regularly followed by periods in which crime decreases (Blumstein 1995; Philipson and Posner 1996), the unmea-

11 While Black and Nagin (1998) focus on the basic Lott and Mustard (1997) fixed-effects specification, similar findings of bias are found with extensions to the basic model, including those that include robbery or burglary rates as additional controls for unmeasured variables. (E-mail from Dan Black to me.)
sured factors that cause crime rates to increase more sharply in treatment than in control states before the concealed-carry laws go into effect may be responsible for part or all of the relative decrease in crime rates in treatment states after the laws are implemented.

The Black and Nagin (1998) findings also raise questions about the interpretation of the results on multiple-victim shootings in Lott and Landes (1999) and crime spillover effects from concealed-carry laws in Bronars and Lott (1998). The multiple-victim shooting study by Lott and Landes uses the same fixed-effects regression models as those used in the Lott and Mustard analysis. Since Black and Nagin provide evidence suggesting that the fixed-effects regression models may be biased when UCR homicide and other crime rates are used as the dependent variables of interest, the same biases will presumably arise when the models are estimated using data for a specific subgroup of homicides (multiple-victim shootings) as the dependent variable of interest. Empirical evidence on this point could be easily obtained by applying the model-specification test employed by Black and Nagin to the multiple-victim-shooting data.

The finding in the Bronars and Lott (1998) paper that crime rates are correlated across counties seems uncontroversial, since whatever policy and sociodemographic factors affect crime rates are likely to be correlated across areas. Less clear is whether the correlation in crime rates between permissive concealed-carry areas and nearby counties necessarily reflects the movement of criminal activity across jurisdictions in response to concealed-carry laws, as Bronars and Lott claim, or instead are correlated for other reasons. Black and Nagin’s (1998) findings suggest that part or all of the changes in crime rates observed within the states that enact concealed-carry laws are due to factors that are unmeasured or poorly measured in the Lott and Mustard (1997) study. Correlations in crime rates across jurisdictions may thus be due either to the effects of the concealed-carry laws or, instead, to the effects of the unmeasured variables suggested by Black and Nagin’s reanalysis. One way to test these competing hypotheses is to examine the correlation between crime rates in states that pass concealed-carry laws and nearby counties during the period before the concealed-carry laws go into effect. A correlation across areas in crime rates before the concealed-carry laws are implemented would suggest that correlations in crime rates after the laws are enacted are due at least in part to unmeasured variables.

While the Black and Nagin (1998) reanalysis provides some evi-
dence to suggest that unmeasured or poorly measured variables bias the estimates derived by Lott and Mustard (1997), little is currently known about which variables are responsible for this bias. Obvious candidates include poverty, gangs, drug-market activity, and the level and deployment of local police resources, though this list is speculative. A better understanding of the factors that produce bias with the Lott and Mustard study should be a priority for criminological research, since this knowledge would also be useful for attempts to evaluate a broad array of other criminal justice interventions.

One way to control for the unmeasured variables that appear to produce bias with the Lott and Mustard analysis is to model each state’s crime trend as some nonlinear function of time. This approach captures the pattern that the crime rate within each state increases and then decreases over time, even if the specific factors that are responsible for these changes are not known or not properly measured. When Black and Nagin model local crime rates as a quadratic function of time (i.e., as a function of the year and the year squared, plus the other control variables included in the Lott and Mustard multivariate model), the only statistically significant effect of concealed-carry laws is to increase the rate of assaults. One obvious limitation of the quadratic function used by Black and Nagin is that this approach imposes some specific assumptions on how the unmeasured variables affect each state’s crime trend over time.

In my own study of the effects of permissive concealed-carry laws, I control for the time-varying unobserved variables that appear to bias Lott and Mustard’s study and produce a positive (but not statistically significant) estimate for the effects of concealed-carry laws on homicide rates (Ludwig 1998). My study applies a difference-in-difference-in-difference (DDD) model to state-level panel data for 1977–94 that relies on juveniles as a within-state control group to control for time-varying state-specific unobserved variables. I focus on homicide victimization rates obtained from vital statistics records to avoid the measurement problems associated with data on victimization rates for robberies, assaults, and other nonlethal crimes. My study uses state-level rather than county-level data to ensure that there are enough

12 In Lott’s (1998a, p. 230) reply to Black and Nagin (1998), he argues that a “problem with using state-specific quadratic trends . . . [is that] allowing a separate quadratic time trend for each state results in the time trend picking up both the upward path before the law and the downward path thereafter.” Yet this is the central advantage of the quadratic time-trend approach, not a limitation.
adult and juvenile victimizations for each observational unit in each year to permit reliable estimation.

The estimation approach exploits the fact that even states with permissive concealed-carry laws require applicants to be eighteen years old (or in some cases twenty-one) in order to obtain a concealed-carry permit. While concealed-carry laws produce some crime-reducing benefit for juveniles—for example, because adults in public spaces may deter crimes against juveniles or because some teens may look older than eighteen or twenty-one—any deterrent benefit from these laws should nonetheless be greater for adults than juveniles (since only adults can obtain permits) and as a result should be revealed by a reduction in the adult victimization rate relative to that for juveniles. So long as unmeasured, time-varying variables affect both adult and juvenile victimization rates, these omitted variables will be controlled for with this empirical approach.

The estimated coefficient for the concealed-carry law variable in my analysis is equal to +0.15 homicide victimizations per 100,000 adults, an effect that is not statistically significant. Alternative ways of defining treatment and control states or pre- and post-treatment periods produce qualitatively similar estimates. While these estimates will be unbiased in the presence of time-varying state-specific unobserved variables that have similar effects on both juveniles and adults, there remains the possibility of bias from unobserved variables that have disproportionate effects on a state's juvenile or adult population (e.g., from changes in a state's juvenile justice system). Nevertheless, the results of my analysis together with the quadratic-trend model of Black and Nagin (1998) suggest that concealed-carry laws are as likely to cause crime to increase as to decrease.

Three other reanalyses of the Lott and Mustard (1997) data have also been conducted, though none examines the central question of whether the Lott and Mustard estimates reflect the causal effects of concealed-carry laws or, instead, the effects of unmeasured variables. Bartley and Cohen (1998) examine whether the Lott and Mustard findings are robust to decisions about which of Lott and Mustard's control variables to include in the multivariate regression specification. Their reanalysis suggests that the estimated effects of concealed-carry laws do not change substantially when different combinations of the available control variables are included in the model, though this exercise is not informative about whether other variables that are not in-
cluded in the Lott and Mustard dataset cause bias in the estimated effects.

While Lott and Mustard (1997) estimate all of their treatment effects using some variant of a linear ordinary least squares regression, Plassmann and Tideman (1998) reestimate the treatment effects using a maximum-likelihood approach that models county-level crime rates as having a Poisson distribution. The central advantage of the Poisson-regression approach is that the standard errors should be smaller than those obtained from ordinary least squares. The disadvantage is that because the Poisson regression model is more computationally intensive, Plassmann and Tideman must exclude many of the control variables that are included in the Lott and Mustard ordinary least squares models, thereby exacerbating the omitted-variables problem that poses the central challenge to valid inference. Thus while Plassmann and Tideman’s estimates suggest even larger crime-reducing effects from concealed-carry laws than those obtained by Lott and Mustard, these findings are likely to be unreliable given Black and Nagin’s findings that even the more richly specified regression models estimated by Lott and Mustard are biased.

Finally, Dezhbakhsh and Rubin (1998) use the basic linear-regression approach of Lott and Mustard but allow the effects of concealed-carry laws to vary according to the state’s sociodemographic and other characteristics. Their reanalysis produces smaller negative effects of concealed-carry laws on homicide than those obtained by Lott and Mustard and mixed (positive and negative) effects of these laws on other crimes such as rape, robbery, and aggravated assault.

F. Summary

Permissive concealed-carry laws have the potential to reduce crime, by enhancing the ability of citizens to use guns in self-defense and thereby deter crime, or to increase crime, through gun misuse by permitted gun carriers or increased weapon carrying and use by criminals. Since both positive and negative effects are plausible, empirical evidence is required to determine which effects dominate.

The widely publicized study by Lott and Mustard (1997) represents a substantial improvement over previous evaluations of concealed-carry laws along several dimensions. First, previous studies have relied on comparisons of crime rates before and after concealed-carry laws went into effect in jurisdictions that implemented such laws, which
may confound the effects of gun-carrying laws with other changes in
the environment that affect local crime rates. Lott and Mustard instead
use national data and draw inferences about the effects of concealed-
carry laws (the "treatment") by comparing crime in treatment and
control states, which helps eliminate rival hypotheses that may explain
crime patterns in the treatment states. Second, Lott and Mustard use
fixed-effects analysis to focus on relative trends in crime rates between
treatment and control states, which controls for unmeasured or diffi-
cult to measure factors that may cause treatment and control states to
have different crime rates year after year for reasons that are unrelated
to concealed-carry regulations.

While Lott and Mustard find evidence to suggest that permissive
concealed-carry laws produce substantial reductions in violent crime,
their estimates rest on the assumption that treatment and control states
would have had similar trends in crime rates had the treatment states
not implemented their concealed-carry laws. A reanalysis of the Lott
and Mustard data by Black and Nagin (1998) finds evidence that treat-
ment and control states had different crime trends even before the
treatment states' concealed-carry laws went into effect for reasons that
are not captured by the statistical model used by Lott and Mustard.
The implication is that the differences in crime trends between treat-
ment and control states following the implementation of concealed-
carry laws may be due in part or whole to unmeasured, time-varying
factors rather than to the effects of the laws themselves. More recent
evaluations have used different methods to control for these time-
varying unmeasured variables and find no effects or modest positive
effects of permissive concealed-carry laws on crime (Black and Nagin
1998; Ludwig 1998). Because even these recent studies will be unbi-
ased only if some assumptions with the data are met, future research
that employs alternative strategies for controlling for unmeasured vari-
ables would be a useful addition to this literature.

IV. Gun Research and Public Policy
Recent studies of defensive gun use and deterrence have been influen-
tial in scholarly and policy debates about gun policy in the United
States. The possibility that civilian defensive gun use is prevalent and
has substantial deterrent effects on criminal behavior has raised con-
cerns that additional government regulation of gun ownership and use
may increase rather than decrease the rate of serious crime and injuries
by impairing the ability of private citizens to defend themselves against criminal attacks.

One research strategy that has been used to evaluate this possibility is to measure the prevalence of defensive gun use directly. To date, reliable estimates for the number of defensive gun uses that occur each year are not available. The best available evidence suggests that the number of defensive gun uses is probably somewhere between 300,000 and 1.3 million per year, though there is considerable variation across reported gun uses in the degree to which the events may contribute to the public interest. The range of socially desirable gun uses could be narrowed further by conducting additional survey research that asked all respondents directly whether they have used a gun in self-defense, employed a panel design to reduce telescoping, and asked detailed follow-up questions that might help identify the nature of the gun use (including how the respondent might have contributed to the escalation of the event) and screen out false positives.

Even with such a survey in hand, identifying the benefits to society from reported gun uses will be difficult since only one party to the encounter is being interviewed and very little is known about alternative courses of action the respondent might have taken had a gun not been available and the consequences of these alternatives. Such surveys in any case provide no information about the deterrent benefits of defensive gun use. Determining whether these gun uses are in society’s best interests also require answers to difficult normative questions that have yet to be fully resolved in America, as evidenced by the public’s ambivalence about many cases of defensive gun use by law enforcement personnel. More generally, the key question for policy makers is how the number of desirable and undesirable gun uses change in response to changes in public policy, effects that are not implied by estimates for the prevalence of defensive gun use under the status quo. A more promising approach for learning about the consequences of defensive gun use and deterrence is to examine the net effects of gun policies on more ambiguous outcomes such as crime and injury rates.

One research literature that follows this strategy seeks to estimate the net effects of permissive concealed-carry laws, which are intended to enhance the ability of citizens to use guns in self-defense and deter criminal activity. The central challenge in evaluating these laws is that we do not observe what rates of crime and injury would have been in states that enact concealed-carry laws had they not enacted these laws. Simply observing the crime trends in these states before and after the
law may be misleading. Increases in crime following implementation of a law do not necessarily imply that the intervention was ineffective, since crime may have increased even more rapidly in the absence of the law. Similarly, reductions in crime rates following enactment of a law do not imply that the law was effective, since crime may have decreased by the same amount or more had the law not gone into effect.

The widely publicized study by Lott and Mustard (1997) attempts to rule out alternative explanations for observed crime changes in states that enact concealed-carry laws by comparing the relative changes in crime rates between states with and without such laws, using multivariate regression analysis to control for other factors that may have changed differentially between treatment and control states. While Lott and Mustard find evidence suggesting that concealed-carry laws substantially reduce violent crime, a reanalysis of their data by Black and Nagin (1998) suggests that this estimated treatment effect is due in part or whole to factors that are not adequately captured by the statistical model used by Lott and Mustard. More recent evaluations that control for unmeasured variables in more sophisticated ways suggest that concealed-carry laws are as likely to cause crime to increase as to decrease, though this evidence is far from definitive.

What are the implications for public policy? Most criminologists agree that the number of fatal injuries in America would be lower by some amount if fewer criminals used guns to commit their crimes (Cook 1991). The research literatures on defensive gun use and deterrence have raised important and useful questions for criminologists and public policy makers about the costs of additional gun regulations in the form of reductions in civilian defensive gun use. Nevertheless, the best available evidence suggests that the crime-reducing benefits of policies such as restricting the number of guns carried in public spaces are not outweighed by the costs of fewer defensive gun uses and a reduction in the deterrence threat to criminals.

Learning more about the net effects of concealed-carry and other gun regulations on rates of crime and injury should be an important priority for criminological research. I conclude with some suggestions for how future research might usefully address these questions. First, Lott and Mustard’s (1997) use of national data and fixed-effects analysis sets a new standard for the empirical evaluation of gun policies. Wherever possible, future evaluations should strive to use national data with multiple observations over time for both “treatment” and “control” jurisdictions. Moreover, information about whether the treat-
ment and control states have similar crime trends before the interventions go into effect provides useful information about whether differences in trends around the time of the intervention reflect the causal effects of these laws or, instead, are caused by other factors that have not been properly modeled.\textsuperscript{13} Similarity in pretreatment crime trends between treatment and control states provide an objective minimum standard for the reliability of future evaluations of gun policies.

Second, if some groups but not others are affected by a policy, analysts may further control for unmeasured state-specific factors by using the empirical approach outlined in Ludwig (1998). Changes in within-state differences in outcomes over time between affected and unaffected (or less-affected) groups may help identify the causal effects of the interventions of interest.

Third, evaluations of policies to restrict access to firearms should focus on gun suicides as well as homicides. Reductions in gun suicides represent an important effect of gun regulations, and ignoring these effects will produce a misleading picture of the net benefits and costs of a given policy. Moreover, gun suicides may be less susceptible to the unobserved criminogenic factors that may confound the analysis of gun crimes. (This suggestion will be less useful for gun policies such as concealed-carry restrictions that should have little effect on suicides but may hold some promise for evaluations of other interventions.)

Finally, criminologists should search for institutional and other factors that produce variation in gun policies that is independent of local criminogenic and criminal justice conditions; this kind of "exogenous" policy variation forms the basis for "instrumental variables" analysis (see Angrist, Imbens, and Rubin 1996). While the instrumental variables estimates presented in the Lott and Mustard (1997) study appear to be biased, as evidenced by their implausibly large magnitudes, a number of other studies have employed the instrumental variables procedure to good effect. Some examples of exogenous policy variation that have been exploited in previous studies include the timing of local election cycles that affect police hiring (Levitt 1997), which has been used to examine the effect of police resources on crime, and prison overcrowding lawsuits that produce sharp changes in prison populations (Levitt 1996), which have been used to study the effects of incar-

\textsuperscript{13} If multivariate regression models are used that include controls for other covariates, analysts should compare the regression-adjusted trends (i.e., the trends in the regression residuals) during the preintervention period. Formal statistical tests for differences in such trends are outlined in Heckman and Hotz (1989) and Black and Nagin (1998).
ceration policies on crime. This kind of instrumental variable analysis holds enormous potential for overcoming many of the empirical problems that arise in evaluating the effects of gun policies, though the strategy to date has been underused within criminology.

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