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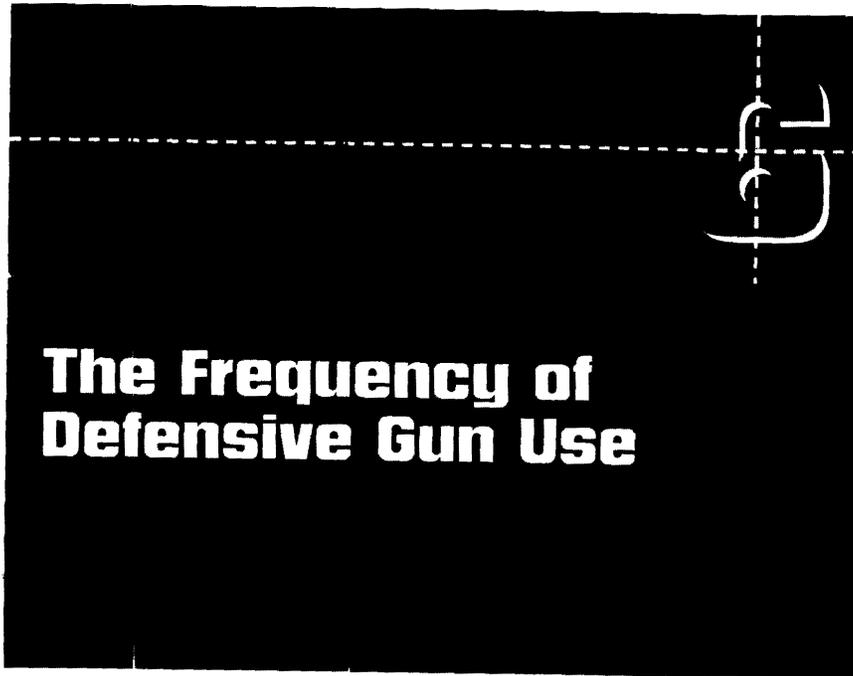
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An ounce of evidence outweighs a ton of speculation

Abbreviations Frequently Used in Chapter Six:

- DGU = defensive gun use
- NCVS = National Crime Victimization Survey
- NSDS = National Self-Defense Survey
- NSPOF = National Survey of the Private Ownership of Firearms¹
- R = respondent (in a survey)

Gun control advocates who argue for laws that would reduce gun availability in various ways often assert that such a measure would be worthwhile even if it could save just one life. Thus, even if nearly all violent people managed to evade a given control mechanism, it might nevertheless be beneficial. The unstated premise of this argument, however, is that there are no countervailing costs attributable to any benefits of gun ownership and use that might be reduced by the control. Suppose, for example, that passing a gun control law indeed saved one life, but also cost

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two lives by disarming prospective victims who would have used guns defensively to prevent the loss of life. Then the law would actually cost lives, and, other things being equal, would be a bad idea.

To simply assume that gun control measures can only reduce violence, or at worst have no effect, is an irresponsible simplification. Gun control policies should be subject to the same critical standards that apply to other policies, which means that analysts must take seriously—not merely give lip service to—the possibility that the policies could have unintended harmful effects. Whether this is true for gun control depends to a great extent on the frequency and effects of defensive uses of guns. Thus, this chapter, and the next one, address the frequency, nature, and effects of owning, carrying, and using guns for protection.

We begin with the issue of how often Americans use guns for protection. This chapter is as much about the plague of advocacy scholarship, sagecraft, and “junk science” that afflicts the study of guns and violence, and particularly the issue of guns and self-defense, as it is about the methods and results of research in this area.

Early Surveys with Defensive Gun Use Questions

Before any scholarly survey research focused on defensive gun use (DGU), a number of public opinion polls, many focusing on gun ownership, crime, or related issues, asked one or two defensive gun use questions of probability samples of the general adult population. The results and other noteworthy features of these surveys, referred to hereafter as the “gun surveys,” are summarized in table 6.1. The surveys differ in many important respects. Some asked about uses of all types of guns, while others were confined to handguns. Some covered a specific time period, asking if the respondent used a gun in, e.g., the past five years, while others asked whether the respondent had ever used a gun defensively at any time in the past. Given the widely varying ages of those questioned, the former method of asking the question is clearly more informative. Some of the survey questions asked about “self-defense,” which may narrowly suggest defense of one’s own bodily safety, while others asked more broadly about “protection,” which could include protection of other people and of property.

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Some questions asked only about the respondent's own experiences, while others asked about defensive uses by anyone in the respondent's household. Most surveys asked the defensive use questions of all respondents, but three of them "prescreened" respondents asking the question only of those who reported currently having a handgun or gun in the household. Most surveys specifically excluded uses in the course of military or police duties, but some did not. Some surveys did not distinguish defensive uses against animals from uses against human threats. The better surveys covered a national population, asked about defensive uses during a specific time period, asked the question of all respondents, distinguished civilian use from other uses, and distinguished uses against humans from uses against animals.

Like serious crime victimization, the absolute prevalence of defensive gun use, i.e., the percent of the population that has had this experience, is low. However, when translated into raw numbers of events, as crime figures are commonly reported, the percentages imply large numbers of defensive uses. All of the surveys imply at least 700,000 annual defensive gun uses. The surveys all differed from one another with respect to exactly what set of events was being estimated, but it is possible to adjust estimates from almost all of the surveys to make them somewhat more comparable with each other. Two surveys (Cambridge Reports; *Time/CNN*) cannot yield comparable estimates because the defensive gun use questions were asked only of current gun owners.³ This is important because a substantial share of defensive gun uses are by persons in households that do not currently own (or at least do not report) guns, because the user employed either a gun belonging to someone else or one that was previously in the household.⁴ Surveys that ask the defensive gun use question only of current gun owners will therefore substantially underestimate the frequency of defensive gun uses. The appendix to this chapter explains the computation of adjusted estimates for the thirteen earlier surveys.

The National Self-Defense Survey

The National Self-Defense Survey (NSDS), conducted in the spring of 1993, was the first survey ever devoted to the subject of armed self-defense.⁵ It was carefully designed to correct all of the known correctable or avoidable

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Table 6.1. Frequency of Defensive Gun Use Based on Thirteen Early Surveys²

Survey:	Field	Bordua	Cambridge Reports	DM1a	DM1b	Hart	Ohio
Area:	California	Illinois	U.S.	U.S.	U.S.	U.S.	Ohio
Year of Interviews:	1976	1977	1978	1978	1978	1981	1982
Population covered:	Noninst. adults	Noninst. adults	Noninst. adults	Registered voters	Registered voters	Registered voters	Residents
Gun Type Covered:	Handguns	All guns	Handguns	All guns	All guns	Handguns	Handguns
Recall Period:	Ever/1.2 yrs.	Ever	Ever	Ever	Ever	5 yrs.	Ever
Excluded Uses Against Animals?	No	No	No	No	Yes	Yes	No
Excluded Military, Police Uses?	Yes	No	No	Yes	Yes	Yes	No
Defensive question asked of:	All Rs	All Rs	Protection hgun owners	All Rs	All Rs	All Rs Households	Rs, Hgun
Defensive question refers to:	Respondent	Respondent	Respondent	Household	Household	Household	Respondents
% Who Used	1.4/3/8.6 ^a	5.0	18	15	7	4	6.5
% Who Fired Gun	2.9	n.a.	12	6	n.a.	n.a.	2.6
Implied number of def. gun uses ^b (millions)	3.1	1.4	n.a.	2.1	1.1	1.8	0.8

Table 6.1. (continued) Frequency of Defensive Gun Use Based on Thirteen Early Surveys²

Survey:	Time/ CNN	Mauser	Gallup	Gallup	L.A. Times	Tarrance
Area:	U.S.	U.S.	U.S.	U.S.	U.S.	U.S.
Year of Interviews:	1989	1990	1991	1993	1994	1994
Population covered:	"Firearm owners"	Residents	Noninst. Adults	Noninst. Adults	Noninst. Adults	Noninst. Adults
Gun Type Covered:	All guns	All guns	All guns	All guns	All guns	All guns
Recall Period:	Ever	5 years	Ever	Ever	Ever	5 years
Excluded Uses Against Animals?	No	Yes	No	No	No	Yes
Excluded Military, Police Uses?	Yes	Yes	No	Yes	Yes	Yes
Defensive question asked of:	Gun owners	All Rs	Rs in hgun hshlds	Gun owners	All	All
Defensive question refers to:	Respondent	Hsehold	Respondent	Respondent	Respondent	Respondent/ Household
% Who Used	n.a.	3.79	8	11	8 ^d	1/2 ^e
% Who Fired Gun	9-16 ^c	n.a.	n.a.	n.a.	n.a.	n.a.
Implied number of def. gun uses ^b (millions)	n.a.	1.5	0.8	1.6	3.6	0.8

Notes:

- 1.4 percent in past year, 3 percent in past two years, 8.6 percent ever.
- Estimated annual number of defensive uses of guns of all types against humans, excluding uses connected with military or police duties, after any necessary adjustments were made, for U.S., 1993. Adjustments are explained in detail in Appendix.
- 9 percent fired gun for self-protection, 7 percent used gun "to scare someone." An unknown share of the latter could be defensive uses not overlapping with the former.
- Covered only uses outside the home.
- 1 percent of respondents, 2 percent of households.

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flaws of previous surveys. The authors used the most anonymous possible national survey format, that of the anonymous random digit dialed telephone survey. They did not know the identities of those who were interviewed, and made this fact clear to the respondents. They interviewed a large, nationally representative sample (1,977 completed interviews) covering all adults (age eighteen and over) in the lower forty-eight states who lived in households with telephones, including those with unlisted numbers. They asked defensive gun use questions of all respondents in our sample, asking them separately about both their own defensive gun use experiences and those of other members of their households.

They used both a five-year recall period and a one-year recall period, inquired about uses of both handguns and other types of guns, and excluded occupational uses of guns and uses against animals. Finally, they asked a long series of detailed questions designed to establish exactly what respondents did with their guns, whether they confronted other humans, and with what type of crime each defensive gun use was linked.

A professional telephone polling firm, Research Network of Tallahassee, Florida, carried out the sampling and interviewing. Only the firm's most experienced interviewers were used on the project. Interviews were monitored at random by survey supervisors. All interviews in which an alleged defensive gun use was reported by the respondent were validated by supervisors with call-backs, along with a 20 percent random sample of all other interviews. Of all eligible residential telephone numbers called where a person (rather than an answering machine) answered, 61 percent resulted in a completed interview. Interviewing was carried out from February through April of 1993.

The quality of sampling procedures was likewise well above the level common in national surveys. The sample was not only large and nationally representative, but also stratified by state. That is, forty-eight independent samples of residential telephone numbers were drawn, one from each of the lower forty-eight states, providing forty-eight independent, albeit often small, state samples. Given the nature of randomly generated samples of telephone numbers, there was no clustering of cases or multistage sampling, as there is in the National Crime Victimization Survey (NCVS), and thus no inflation of sampling error due to such procedures.⁶ To gain a larger raw number of sample defensive gun use cases, the authors oversampled in the South and West regions, where previous surveys had indicated gun own-

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ership was higher.⁷ They also oversampled within contacted households for males, who are both more likely to own guns and to be victims of crimes in which victims might use guns defensively (p. 56). Data were later weighted to adjust for oversampling.

Each interview began with a few general "throat-clearing" questions about problems facing the respondent's community and crime. The interviewers then 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." Respondents who answered yes were then asked: "Was this to protect against an animal or a person?" Those who reported a defensive gun use against a person were then asked: "How many incidents involving defensive uses of guns against persons happened to members of your household in the past five years?" and then: "Did this incident [any of these incidents] happen in the *past twelve months*?" At this point, respondents were asked: "Was it *you* who used a gun defensively, or did someone else in your household do this?"

All respondents reporting a defensive gun use were then asked a long, detailed series of questions establishing exactly what happened in the defensive gun use incident. Respondents who reported having experienced more than one defensive gun use in the previous five years were asked about their most recent experience. When the original respondent was the one who had used a gun defensively, as was usually the case, interviewers obtained their firsthand account of the event. When the original R indicated that some other member of their household was the one who had the experience, interviewers made every effort to speak directly to the involved person, either speaking to them immediately, or obtaining times and dates to call them back. Up to three callbacks were made in attempting to directly contact the defensive gun use-involved person.

There were 222 completed interviews with respondents reporting defensive gun uses. Questions about the details of defensive gun use incidents permitted us to establish that a given defensive gun use met all of the following qualifications before it would be defined as a genuine defensive gun use: (1) the incident involved defensive action against a human rather than an animal, but not in connection with police, military, or security guard duties, (2) the incident involved actual contact with a person, rather

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than merely investigating suspicious circumstances, etc.; (3) the defender could state a specific crime that they thought was being committed at the time of the incident, and (4) the gun was actually used in some way—at minimum it had to be used as part of a threat against a person, either by verbally referring to the gun (e.g., “get away—I’ve got a gun”) or by pointing it at an adversary. No effort was made to assess either the lawfulness or morality of the defensive gun uses.

An additional step was taken to minimize the possibility of defensive gun use frequency being overstated. Kleck went through interview sheets on every one of the interviews in which a defensive gun use was reported, looking for any indication that the incident might not be genuine. A case would be coded as questionable if any of four problems appeared to characterize it: (1) it was not clear whether the respondent actually confronted any adversaries; (2) the respondent was a police officer, member of the military, or a security guard, and thus might have been reporting, despite instructions not to do so, an incident that occurred as part of his or her occupational duties; (3) the interviewer did not properly record exactly what the respondent had done with the gun, so it was possible that he or she had not used it in any meaningful way, or (4) the respondent did not state, or the interviewer did not record, a specific crime that the respondent thought was being committed against him or her at the time of the incident. There were a total of twenty-six cases where at least one of these problematic indications was present.

It should be emphasized that the authors did not know that these cases were *not* genuine defensive gun uses; rather, they simply did not have as high a degree of confidence on the matter as with the rest of the cases designated as defensive gun uses, usually because of missing information. Thus, these were *not* cases where there was affirmative information indicating they were not genuine defensive gun uses, but rather cases with less information and thus less basis for confidence about their status. Cases where there was affirmative information indicating that the event did not fit the definition of a defensive gun use were excluded altogether from all estimates. Estimates using all of the apparently genuine defensive gun use cases were labeled A estimates, while the more conservative estimates based only on cases devoid of problematic indications were labeled B estimates.

THE FREQUENCY OF DEFENSIVE GUN USE 221**National Self-Defense Survey Results**

Table 6.2 displays a large number of estimates of how often guns are used defensively. These estimates are not inconsistent with each other, but rather measure different things in different ways. Some estimates are based only on incidents that respondents reported as occurring in the twelve months preceding the interview, while others are based on incidents reported for the preceding five years. Both telescoping and recall failure should be lower with a one-year recall period, so estimates derived using it should be superior to those based on the longer recall period. Some estimates are based only on incidents that respondents reported as involving themselves (person-based estimates), while others were based on all incidents which Rs reported as involving anyone in their household (household-based estimates). Because of their more firsthand character, the person-based estimates should be better. Finally, some of the figures pertain only to defensive gun uses involving use of handguns, while others pertain to defensive gun uses involving any type of gun.

The methods used to compute the table 6.2 estimates are simple. Prevalence (“% Used”) figures were computed by dividing the weighted sample frequencies (in the top two rows of numbers) by the total weighted sample size of 4,977. The estimated number of persons or households who experienced a defensive gun use (in the third and fourth rows) was then computed by multiplying these prevalence figures by the appropriate U.S. population base—the number of persons age eighteen and over for person-based estimates, and the number of households for household-based estimates. Finally, the estimated number of defensive uses was computed by multiplying the number of defensive gun use-involved persons or households by an estimates of the number of all-guns defensive gun use incidents per defensive gun use-involved person or household. Applying the more generous definitions used with the A estimates, the survey data indicated that each defensive gun use-involved person had been involved in an average of 1.478 defensive gun uses in the previous five years, while each involved household had experienced an average of 1.531 uses. Applying the more conservative definition used with the B estimates, the data indicated that each involved person experienced an average of 1.472 defensive uses in the preceding five years, while each involved household experi-

ceeded an average at 1.535 uses. The authors did not establish how many defensive gun uses occurred in the past year, and, for past-five-years defensive gun uses, did not separately establish how many of the defensive gun uses involved handguns and how many involved other types of guns. Therefore, for all past-year estimates, and for past-five-years handgun estimates, it was necessary to conservatively assume that there was only one defensive gun use per involved person or household.

The most technically sound estimates presented in table 6.2 are those based on the shorter one-year recall period and which rely on respondents' firsthand accounts of their own experiences (person-based estimates). These estimates, shown in the first two columns, indicate that each year in the United States there are about 2.2 to 2.5 million defensive uses of guns of all types by civilians against humans, with about 1.5 to 1.9 million of the incidents involving use of handguns.

These estimates were larger than those derived from the best previous surveys, indicating that technical improvements in the measurement procedures, contrary to the expectations of Cook, Reiss, and Roth, and McDowall and Wiersema, *increased* rather than decreased estimates of the frequency with which defensive gun uses occur.³ Defensive gun use is thus just another specific example of a commonplace pattern in criminological survey work (victimization surveys, self-report surveys of delinquency, surveys of illicit drug use, etc.): the better the measurement procedures, the higher the estimates of controversial behaviors.⁹

These estimates may seem remarkable in comparison to expectations based on conventional wisdom, but are quite modest in comparison to various gun-related phenomena. There were probably over 235 million guns in private hands in the United States by the end of 1994, implying that only about 1 percent of them are used for defensive purposes in any one year.¹⁰ In a December 1993 Gallup survey, 49 percent of U.S. households reported a gun, and 31 percent of adults reported personally owning one (p. 99). These figures imply about 17.6 million households with a gun, with perhaps 93 million (49 percent of the adult U.S. population) adults living in households with guns, and about 59.4 million adults personally owning a gun. Again, it hardly seems implausible that 3 percent (2.5 million/93 million) of the people with immediate access to a gun could have used one defensively in a given year. Finally, while the NSDS implied about 670,000 to

Table 6.2 Prevalence and Incidence of Civilian Defensive Gun Use, U.S., 1988-1993^a (National Self-Defense Survey)

Recall Period:		Past Year				Past Five Years			
Base:		Person		Household		Person		Household	
Gun Types:		All Guns	Handguns	All Guns	Handguns	All Guns	Handguns	All Guns	Handguns
Weighted	A: ^c	66	49	79	55	165	132	194	148
Sample	B: ^c	56	40	68	46	148	115	172	129
Cases									
% Used ^b	A:	1.326	0.985	1.587	1.105	3.315	2.652	3.898	2.974
	B:	1.125	0.804	1.366	0.924	2.974	2.311	3.456	2.592
Persons/ Households	A:	2549862	1893079	1540405	1072434	6316811	5053449	3782767	2885822
	B:	2163519	1545371	1325918	896945	5665988	4402626	3353794	2515345
Annual Uses	A:	2549862	1893079	1540405	1072434	1867249	1493800	1158283	883639
	B:	2163519	1545371	1325918	896945	1668067	1296133	1029615	772211

Population Bases: Estimated resident population, age 18 and over, U.S., April, 1993: 192,282,770; estimated households (assuming the 1992-1993 percentage increase was the same as the 1991-1992 increase): 97,045,525 (U.S. Bureau of the Census 1993, pp. 17, 55).

Notes:

- a. Defensive uses of guns against humans by civilians (i.e. excluding uses by police officers, security guards or military personnel). All figures are based on weighted data. Some of the estimated DGU counts in the "Past Five Years" section have been revised from those in Kleck and Gertz (1995, p. 184). However, no conclusions, here or elsewhere, are based on these estimates.
- b. Percent of persons (households) with at least one defensive gun use during the five years (one year) preceding the interview.
- c. A estimates are based on all reported defensive gun uses reported in the survey. B estimates are based on only cases with no indications the case might not be a genuine defensive gun use.

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1,570,000 defensive gun uses linked with carrying guns in public places, it also found there were 1.8 to 2.3 billion person-days of carrying guns in public places.¹¹ Thus, defensive gun uses in public places occurred in less than one in a thousand instances of carrying guns in public places.

Not only do huge numbers of Americans have access to guns, but the overwhelming majority of them are, if one can believe their own statements, willing to use a gun defensively. In a December 1989 national survey, 73 percent of American gun owners stated that they would not only be willing to use a gun defensively in some way, but would be willing to *shoot* a burglar.¹² The percentage willing to use a gun defensively in some way, though not necessarily by shooting someone, would presumably be even higher than this.

Nevertheless, having access to a gun and being willing to use it against criminals is not the same as actually doing so. The latter requires experiencing a crime under circumstances in which the victim has access to a gun. It is unknown how many such opportunities for crime victims to use guns defensively occur each year. It would be useful to know how large a fraction of crimes with direct offender-victim contact result in a defensive gun use. Unfortunately, a large share of the incidents covered by defensive gun use surveys are probably outside the scope of incidents that are likely to be reported to either the National Crime Victimization Survey (NCVS) or police. If the defensive gun use incidents reported in the present survey are not entirely a subset within the pool of cases covered by the NCVS, one cannot meaningfully use NCVS data to estimate the share of crime incidents that result in a defensive gun use. Nevertheless, in a ten-state sample of incarcerated felons interviewed in 1982, 34 percent reported having been "scared off, shot at, wounded or captured by an armed victim."¹³ From the criminals' standpoint, this experience was not rare.

This phenomenon, regardless of how widespread it really is, is largely an invisible one as far as governmental statistics are concerned. The defender/victim has little incentive to report this sort of event to the police, and may have strong reasons to not do so, such as the possibility of being arrested for either the violent use of the gun or for a violation of gun laws. Consequently, many of these incidents never come to the attention of the police, while others may be reported, but with victims omitting any mention of their own use of a gun. And even when a defensive gun use is reported, it would rarely be recorded by the police, who ordinarily do not keep sta-

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istics on such matters other than defensive gun uses resulting in a death), since police record-keeping is largely confined to information helpful in apprehending perpetrators and making a legal case for convicting them.

It is also clear that virtually none of the victims who use guns defensively tell interviewers about it in the NCVS. The NSDS estimates imply that less than 5 percent of defensive gun uses among NCVS respondents are reported to interviewers (the 116,000 defensive gun uses estimated from the NCVS, divided by the 2.55 million estimate derived from the NSDS equals .045).¹⁴

Contrary to unsubstantiated speculations that the NSDS estimates of defensive gun use frequency are too high,¹⁵ there is a considerably firmer basis for believing they are too low, because they do not count any of the following:

1). defensive gun uses people are unwilling to tell strangers about on the phone, often because they involved gun law violations by the respondent (e.g., the gun was carried illegally);

2). defensive gun uses people forgot or wrongly thought were not serious enough to fit into the set of events about which the interviewer was inquiring;

3). defensive gun uses among persons under age eighteen (the NSDS, like other gun surveys, covered only persons eighteen or older, in contrast to the NCVS, which covers persons twelve or older). Persons age twelve to seventeen account for about 24 percent of violent victimizations, and carry guns for protection at about the same rate as adults, so the NSDS may have missed about one quarter of defensive gun uses for this reason alone;¹⁶

4). additional defensive gun uses in the one-year recall period, beyond the most recent one (NSDS one-year estimates assume a single defensive gun use per defensive gun use-involved person).

Also, the NSDS could not contact persons in households without telephones, who tend to be poorer than the rest of the population. And like most surveys, the NSDS underrepresented African Americans. Since both poor people and minorities are more likely to be crime victims, the effect is that the NSDS underrepresented persons likely to have had a need to use a gun for self-protection.

Later Surveys

At least five more national surveys have been conducted since the NSDS (for a total of nineteen altogether, as of mid-2000), and all yielded DGU estimates supportive of those from the NSDS. The best of these surveys, the Police Foundation's National Survey of the Private Ownership of Firearms (NSPOF), was sponsored by the U.S. Justice Department.¹⁷ The NSPOF was based on a sample only half the size of the NSDS's; was conducted by interviewers and staff with no prior experience with crime surveys, and, unlike the NSDS, apparently did not make callbacks with respondents claiming a defensive gun use. Nevertheless, it was modeled after, and otherwise generally comparable to, the NSDS. Further, it included even more questions about details of alleged defensive gun uses that might disqualify them as genuine defensive gun uses.

The NSPOF strongly confirmed the results of the NSDS, yielding estimates, where comparable, of annual DGU prevalence that were within sampling error of those obtained by Kleck and Professor Marc Gertz. Phillip Cook and Jens Ludwig, however, obscured just how strong the confirmation was by making an erroneous comparison of estimates.¹⁸ Like the NSDS, the NSPOF yielded both A estimates based on all reports that fit the definition of a defensive gun use, and more conservative B estimates that excluded reports based on less complete information. Based on person-based self-reports covering the preceding one year, the NSDS yielded an A estimate of 2.55 million and a B estimate of 2.16 million (table 6.2). Cook and Ludwig claimed that their A estimate, which they said was "directly comparable to the well-known Kleck and Gertz estimate of 2.5 million," was just 1.5 million.¹⁹ While this was within sampling error of the 2.55 million figure, it was nevertheless far lower.

The problem is that Cook and Ludwig reversed their A and B estimates, comparing their more conservative B estimate with the more inclusive Kleck-Gertz A estimate. The estimate from their survey that most closely corresponded to the Kleck-Gertz A estimate of 2.55 million was 2.73 million.²⁰ Cook and Ludwig's discussion made the difference look like a 70 percent discrepancy, but it was in fact a remarkably small 7 percent difference. Ludwig has since repeated this erroneous comparison.²¹

The journal article version of the Cook-Ludwig report only confused

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things further, comparing two more estimates derived from the NSPOF that were also not comparable with the Kleck-Gertz A estimate. The first was not comparable because it included defensive gun uses linked with law enforcement, security work or military service, while the second was not comparable because it excluded cases that were included in the Kleck-Gertz A estimates.²² Ignoring these noncomparable estimates, the Cook-Ludwig results were a remarkably strong confirmation of the Kleck-Gertz NSDS results.

The federal Centers for Disease Control and Prevention (CDCP) fielded a large-scale national survey (n=5,238 completed interviews) in 1994 that asked only about one particular kind of DGU, victims retrieving guns in response to intruders attempting to enter the victim's home.²³ This would roughly correspond to burglary-related defensive gun uses in the NSDS. The NSDS indicated that there were 2.55 million annual DGUs, 33.8 percent of them connected with burglaries (table 6.2), implying 861,853 annual burglary-linked defensive gun uses.²⁴ The CDCP survey estimated that each year in the United States there are 1,001,127 incidents in which a victim saw an intruder in, or trying to get into, their home and retrieved a gun to deal with the situation. Cases where the person retrieved a gun but never saw the potential intruder are excluded from this estimate. Thus, the CDCP estimate was just 16 percent higher than the NSDS estimate, and well within sampling error. Further, even this modest difference would be still smaller if some of the cases where the respondent did not see the potential intruder were in fact burglaries and the respondent verbally threatened the burglar with the gun. Once again, a large professionally conducted national survey strongly confirmed the NSDS results.

A much smaller-scale survey of 1906 respondents was fielded in 1996. Although its designers did not report any national defensive gun use estimates, it utilized a national probability sample of adults and did ask a defensive gun use question. Deborah Azrael and Professor David Hemenway asked their Rs: "In the past five years, have you used a gun to protect yourself from a person or people?"²⁵ The phrase "protect yourself" excludes defensive uses on behalf of others. The authors excluded cases involving police, security guard, or military duties (apparently excluding off-duty uses as well as on-duty uses, unlike other researchers). This question had been preceded by the question "In the past five years, has anyone displayed or brought out a gun in a hostile manner, even if this event did

not take place as part of commission of a crime?" As the authors acknowledged (p. 290), an unknown share of the positive responses to this somewhat ambiguous question may also reflect defensive gun uses, i.e., experiences in which other people used guns defensively against the respondent. Some defenders who answered yes to the ambiguous first question might have thought it was unnecessary to answer affirmatively to the self-protection question, believing that they had already reported the relevant event.

Nevertheless, it is possible to compute a conservative national defensive gun use prevalence estimate implied by this survey's results for just the "used a gun to protect" question. Of 1,906 respondents, fourteen answered yes to this question, pertaining to the past five years (p. 239). No past-year figures were reported, but both the NSDS and NSPOF found that the ratio of past-year defensive gun uses over past-five-years defensive gun uses was 0.10, so we can estimate this survey would have yielded 5.6 defensive gun uses with a one-year recall period, indicating an annual prevalence of 0.294 percent (95 percent confidence interval: 0.00291 +/- 0.002130, assuming simple random sampling, and an even wider interval without it).²⁶ Applying these figures to the 1996 U.S. population age eighteen and over of 235,113,000, the estimated annual defensive gun uses would be 690,798 (95 percent confidence interval: 119,885 to 1,262,111). This is a conservative estimate because it excludes the "hostile uses" of guns reported by respondents in this study that could also have been defensive gun uses directed at the respondent. Thus, Hemenway's own survey indicated at least six times as many defensive gun uses as the NCVS estimates that he favors.²⁷ He and his coauthor did not, however, report this estimate to their readers.

In May of 2000, the Gallup survey organization asked 1,031 U.S. adults: "Not including military combat, have you ever used a gun to defend yourself, either by firing it or threatening to fire it?" and 7 percent responded yes.²⁸ This survey used a lifetime recall period, failed to exclude uses against animals or uses linked with law enforcement or security work, while arbitrarily excluding defensive gun uses to defend others ("... to defend *yourself*..."). McDowall et al. found that of 155 respondents who initially reported a defensive gun use without being told to exclude such cases, 30 turned out to involve uses linked with law enforcement or security work.²⁹ Thus, one can roughly adjust for the failure to exclude such cases by multiplying the estimate by 125/155. Multiplying 7 percent by

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125/155, and also applying the adjustments described in the appendix to adjust for use of a lifetime recall period and the inclusion of uses against animals, yields an estimated past-year defensive gun use prevalence of 0.836 percent (95 percent confidence interval: 0.280-1.392 percent, assuming simple random sampling). This is not significantly different from the NSDS estimate of 1.326 percent and implies, based on a projected U.S. population of 203,852,000 residents age eighteen or over, 1,704,449 annual defensive gun uses.

Finally, a *Washington Post* poll fielded in late June 2000 found that 8 percent of U.S. adults had used a gun in self-defense or to protect their property sometime in their lives.³⁰ Applying the appendix adjustments for the lifetime recall period and the failure to exclude uses against animals yields a one-year prevalence estimate of 1.185 percent, not significantly different from the 1.326 percent estimate from the NSDS. Given about 203,852,000 U.S. adults in 2000, this implies 2,415,448 persons with a defensive gun use in the previous year.

To summarize, there are now at least nineteen professional surveys, seventeen of them national in scope, that indicate huge numbers of defensive gun uses in the United States each year.

Explaining the Deviant National Crime Victimization Survey Results

The only national survey to ever indicate less than about 700,000 annual defensive gun uses is the National Crime Victimization Survey (NCVS), which indicated only about 116,000 defensive gun uses per year from 1993 to 1994.³¹ This is less than one twentieth of the 2.55 million estimate yielded by the 1993 National Self-Defense Survey or the 2.73 million estimate from the 1994 National Survey of the Private Ownership of Firearms. How might one account for the extraordinarily deviant estimates produced by the National Crime Victimization Survey?

Some have assumed that the NCVS *must* somehow yield at least approximately reasonable estimates because so much money and technical expertise has gone into developing the survey, and therefore all the other surveys are wrong. For example, Cook accurately, if not very relevantly,

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noted that "the National Crime Survey [the original name of the NCVS] is a much larger and more sophisticated effort [than the gun surveys], based on questionnaires that have been devised and refined through a program of extensive testing."³² This is not very relevant because none of this considerable technical refinement was aimed at producing estimates of defensive uses of guns or of other forceful self-protective actions. Further, none of the "extensive testing" has ever addressed whether the NCVS accurately estimates the frequency of these behaviors. External evidence from at least nineteen other surveys strongly indicates that it does not.

The NCVS was designed to produce national estimates of criminal victimizations. Estimates of forceful self-protection actions are merely an incidental byproduct of the considerable effort devoted to achieving this primary goal. The task of estimating forceful defensive actions is, however, so fundamentally different from that of estimating criminal victimizations in general that many of the undoubted virtues of the NCVS for the latter task have no necessary bearing on the former. Ironically, many of the NCVS's features which are advantageous for producing crime estimates, such as being conducted by the federal government, are probably handicaps for estimating DGUs.

Any forceful act of self-protection is a violent act, since by definition it involves either an attack or a threat against another human being. Even when the victim/defender is confident that the act was morally justified, he or she cannot be so certain about how an interviewer would morally assess their actions. A well-recognized problem of survey research is that respondents often do care about what interviewers think and adjust their answers to not raise questionable matters. Further, regardless of whether the act was in fact lawful, few respondents could be certain about its legality. The law of self-defense is extremely complex, and even legal experts are frequently uncertain whether a given forceful act was lawful.³³ Therefore, uncertainty among defensive actors themselves is also likely to be widespread. Finally, leaving aside the legality of the defensive act, a person who defended themselves with a firearm is likely to have carried the gun illegally or otherwise violated gun control laws.

Thus, if an respondent in the NCVS was the victim of a crime in which she/he used defensive force, when the NCVS interviewer asks about self-protective acts, she/he is in effect asking the respondent to confess to an act of violence, an act that may itself be criminal, and that in any case the

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respondent might think the interviewer would disapprove of. Thus, in connection with forceful self-protection, the NCVS becomes a survey covering controversial and possibly illegal acts committed by the respondent, rather than merely a survey of criminal acts committed against the respondent. The goals of a survey of self-reported deviance are fundamentally different from those of a victimization survey, and are best served by significantly different technical features.

There obviously are some hugely consequential factors responsible for the enormous discrepancy in defensive gun use results between the NCVS and all other surveys. Many features of the NCVS are likely to contribute to an underestimation of defensive gun use frequency. First, interviewers never directly ask respondents about defensive gun use. One could argue that the NCVS cannot even yield a defensive gun use estimate, since one cannot use a survey to estimate the frequency of an experience that is never directly asked about. Instead, after the respondents report that they had been the victim of a crime, the NCVS interviewer merely asks the respondents, "Was there anything you did or tried to do about the incident while it was going on?" and Rs who wish can then volunteer that they used a gun.³⁴

Discussing this aspect of the NCVS, Tom Smith, director of the National Opinion Research Center, noted that "indirect questions that rely on a respondent volunteering a specific element as part of a broad and unfocused inquiry *uniformly lead to* undercounts of the particular of interest."³⁵ In contrast, the gun use surveys directly asked respondents specifically about defensive gun use.

Second, the NCVS is conducted by one branch of the federal government, the U.S. Census Bureau, on behalf of another branch, the U.S. Justice Department. Respondents are told these things at the beginning of the interview, so when respondents are later asked about self-protection actions, those who used guns are aware that they are talking to employees of the federal government.³⁶ This might not matter much if they were reporting on innocuous topics like how much they spent on home repairs or the number of rooms in their home, but most defensive gun uses involve illegal behavior on the part of the victim, most commonly unlawful carrying of a firearm.³⁷ Federal government conduct of interviews has a very different significance when respondents are asked to confess to a crime, or gun owners are asked about illegal things they have done with their guns, than when respondents

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are asked the mostly innocuous questions common in the census. In contrast, most of the gun use surveys were conducted by private survey firms.³⁸

Third, respondents in the NCVS are not anonymous. Although respondents are promised confidentiality, interviews begin with interviewers obtaining the respondent's name, address, and telephone number.³⁹ Thus, questions are answered in the context of the respondent's knowledge that their identities are known to the surveyors. The lack of anonymity might be of little consequence if respondents did not know that they were speaking to employees of the federal government, but gun owners' fears of gun confiscation by the federal government gives a very different significance to nonanonymity in this context.⁴⁰ NCVS respondents know that they are speaking to federal employees and that the government knows who they are and where they live. Thus, the combination of government conduct of the survey with nonanonymity could be a powerful deterrent to reporting defensive gun uses. In contrast, in gun use surveys respondents are told the interviews are anonymous.

Fourth, in the NCVS, interviewers ask where the crime occurred before asking questions about self-protection.⁴¹ This means that a respondent who had used a gun for self-protection in a public place knows, by the time the self-protection questions are asked, that he has already revealed where he was when the crime occurred. Almost all of the U.S. population lives in states where it is either completely forbidden for any civilian to possess a gun in a public place, or forbidden to all but the handful who possess a carry permit.⁴² The crime of unlawful carrying is generally defined as a felony, frequently results in arrest, and is even subject to mandatory minimum prison sentences of one year or more in at least ten states.⁴³ Thus, if a victim had used a gun in self-protection in connection with a crime that occurred in a public place, it would usually be impossible for the victim to report the defensive gun use without also confessing to having committed a serious crime.

The NCVS for 1994 indicated that only 14.4 percent of violent crime incidents occurred "at or in respondent's home."⁴⁴ The other 85.6 percent occurred in some location where, in order to possess a gun during the incident, the victim would have had to have carried the gun through public spaces. In contrast, gun use surveys ask about details of the incident, including its location, only after asking the defensive gun use questions.

Fifth, for many respondents, gun possession would be unlawful regardless of location. This would apply with special force to the subset of the

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population most likely to be criminally victimized, criminals. Persons with criminal convictions are prohibited under federal law and most states' laws from possessing guns anywhere, and a common condition of probation or parole is to not possess weapons.⁴⁵ In addition, millions of otherwise non-criminal persons possess guns illegally because they lack a permit or license legally required under local or state law. In two Illinois surveys of the general population, Professor David Bordua and his colleagues found that even among respondents willing to report gun ownership, 28 percent did not have the license required of all Illinois gun owners.⁴⁶ For all such persons, to admit use of a gun for self-protection or for any other purpose would necessarily entail confessing to unlawful possession of a gun.

Sixth, in the NCVS, no respondents are even asked the self-protection questions unless they are first willing to report a criminal incident and provide some details about it. Thus, any tendency of respondents to underreport personal-contact crimes would necessarily also cause an underreporting of defensive gun uses. There is convincing evidence that the NCVS underestimates crime frequency. For example, Cook showed that the NCVS radically underestimates the number of criminal incidents involving gunshot woundings.⁴⁷ Based on his best estimate of the number of gunshot woundings reported to police, Cook's data indicated that the NCVS captured only about one-third of the gunshot wound incidents (p. 96). Since all of his proposed explanations for this problem would apply at least as well to other forms of violence, there is reason to expect that victims also radically underreport violent incidents in general. And indeed, based on other comparisons of alternative survey estimates of violent events with NCVS estimates, University of Maryland researchers Colin Loftin and Ellen MacKenzie noted that rapes could be thirty-three times as frequent as NCVS estimates indicate, while spousal violence could easily be twelve times as high.⁴⁸ Less serious incidents are underreported still more, since memory failure is greater with less serious offenses.⁴⁹

A specific variant of nonreporting in the NCVS is especially problematic for defensive gun use estimation. The NCVS often fails to elicit positive responses to crime questions where a genuine crime involved no harm to the victim, i.e., the crime was attempted but not completed. Because the vast majority of defensive gun uses involve neither property loss nor injury to the victim, crimes with defensive gun use will often not be reported at all

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in the NCVS, never mind reported as having involved a defensive gun use.⁵⁰ This problem is discussed in greater detail in a later section.

In sum, in order for respondents to report a defensive gun use in the NCVS, they must be willing to confess, to an employee of the federal government, gathering information for the law enforcement branch of that government, to having committed a serious crime, and to do so in the context of a nonanonymous interview, by volunteering the information in response to a general question that does not even directly ask about gun use. It is hard to imagine survey conditions less congenial for gaining meaningful estimates of defensive gun use frequency.

Nevertheless, none of these conspicuous problems prevented University of Maryland criminologist David McDowall from insisting that there is no "solid reason for expecting errors" in NCVS respondents' responses to the self-protection questions.⁵¹ Nor have the problems been regarded as serious enough to prevent the Bureau of Justice Statistics from continuing to disseminate its defensive gun use "estimates" as if they were valid, without a single caveat to potential users.⁵² And these seemingly grave flaws did not deter McDowall and Wiersema from stating, based solely on the NCVS, that "armed self-defense is extremely uncommon," or discourage Cook from concluding that he could, with minor qualifications, "accept the NCS-based estimate of 50,000 defensive uses per year against rape, robbery, and assault" and conclude that "the National Crime Survey estimates are a reasonable approximation of reality."⁵³ Nor were Kellermann and his colleagues prevented from relying solely on the NCVS estimate in drawing the conclusion that "fewer than two crimes in a thousand (are) resisted with a gun."⁵⁴ Likewise, writing a report for the National Institute of Justice, Urban Institute researcher Jeffrey Roth showed no qualms about citing only a 70,000 NCVS estimate to support his claim that defensive gun uses are rare, mentioning none of the problems with NCVS, and withholding from his readers any hint that every single other survey had yielded far higher estimates of defensive gun use frequency.⁵⁵

McDowall and others have explored whether the differences between defensive gun use estimates based on the NCVS and those based on other surveys such as the NSDS were due to (1) differing question order (asking about victimization first, then self-protective methods rather than the reverse) or to (2) differing domains of behavior covered by the differing ques-

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tions in the surveys.⁵⁶ To test these ideas, respondents were either asked the NCVS questions first, followed by the gun survey questions, or the reverse.

The most noteworthy of the findings pertained to Hemenway's (1997) speculation that large numbers of respondents fabricate nonexistent defensive gun use experiences. McDowall and his colleagues stated that Hemenway's fabrication hypothesis "requires an interaction between question content and question order," and the authors reported that they "did not find such an interaction."⁵⁷ Thus, the results clearly failed to support Hemenway's fabrication hypothesis. The authors, however, declined to draw this conclusion. After having previously stated that they could test this hypothesis with their methods (p. 9), once they obtained results unresponsive of Hemenway's falsification hypothesis, they concluded that their interaction test had "low statistical power" and called for "additional study" (p. 17).

Their results also indicated that using the NCVS questions, and asking them in the NCVS sequence, both yield fewer defensive gun use reports than using the questions and sequence found in the other surveys. In an earlier draft of their paper, the authors had claimed that incidents reported using the gun survey methods but not with the NCVS methods were not really defensive gun uses, but rather were gun uses in connection with "crimes that had not yet occurred."⁵⁸ I served as a referee assessing this paper for a journal and suggested, in comments to the authors, a different interpretation. These cases may instead have been defensive gun uses that occurred early in a genuine crime that was in fact already underway. When early defensive gun use averted any harm to the victim, and did so in such a way that it never became certain that the defender's adversary was indeed trying to harm the victim in some way, these ambiguous cases were not likely to be reported in response to NCVS questions. This interpretation was partially incorporated into the published version of the paper, but the authors nevertheless asserted, without any supporting evidence, that gun surveys include in their estimates "defenses against crimes that had not yet occurred."⁵⁹

Early-Intervention Defensive Gun Uses

It would not be legitimate to place early-intervention uses outside the definition of defensive gun use, since it is precisely within this category where

one would find the most successful defensive gun uses. A genuine attempted crime that is disrupted in its early stages is still a crime, and defensive use of a gun in connection with such a crime is therefore properly classified as a DGU. In many crime situations, victims can effectively defend themselves only if they do so before the offender attacks or immobilizes them. That is, they can defend themselves either early or not at all. This is especially true where the victim is physically smaller or weaker than the aggressor, as in violence of men against women.⁶⁰

Respondents will often be less certain in early-intervention cases that a criminal attempt was in fact underway, and some reported "defensive gun uses" may have been responses to a "threat" that was perceived but not real. Uncertainty about possible early intervention defensive gun uses would justify placing such cases in a category separate from less uncertain cases, as Kleck and Gertz did with some possible defensive gun uses, but it would not justify simply treating them as cases known to *not* involve defensive gun use.⁶¹ It is inherent in the nature of defensive gun uses or any other preventive measures that, if they are effective, individual cases of harm averted are necessarily ambiguous.

There might be a temptation among those critical of defensive gun use to adopt a heads-we-win, tails-you-lose strategy whereby one assigns a large share of alleged defensive gun uses to either of two categories: (1) those in which no harm is inflicted on the victim, which are classified as fake "defensive gun uses" in response to "crimes that had not yet occurred," and (2) those in which the victim *is* harmed, which are genuine but obviously ineffective defensive gun uses. If one is unwilling to classify an event as a genuine defensive gun use unless one achieves the certainty of criminal intent on the part of the offender that comes from the victim actually being harmed, then by definition there can be no such thing as a genuine and effective defensive gun use.

Some of the difference in results between the NCVS and other surveys may be due to the failure of the NCVS to capture early-intervention defensive gun uses. The McDowall et al. results suggest that a three-to-one difference between the NCVS and other surveys could be due to the NCVS question content and sequence, though it is unknown how much of this reflects NCVS failure to capture genuine DGU events and how much is due to gun surveys capturing "defensive gun use" cases that were not true defensive gun uses because no actual crime was committed.⁶²

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There is no evidence indicating how large a share of alleged defensive gun use cases involve borderline or ambiguous cases, and thus no sound empirical basis for believing that such cases pose significant problems for defensive gun use estimation. Such cases might be common among all defensive gun uses, but it is unlikely that they would be common among reported defensive gun uses, i.e., those that people are willing to report to interviewers, since respondents are less likely to report ambiguous cases. McDowall and his associates cite a case that seems to fit this scenario, describing a respondent who did not report a crime with a defensive gun use because he "doubted that he could prove he was victimized" (p. 15).

Defensive Gun Use in Crimes with No Harm to the Victim

Another source of the NCVS-gun survey discrepancy is probably more significant. This is the failure of some NCVS respondents to report crimes that they did not regard as sufficiently serious to deserve mention or qualify as relevant to the interviewer's questions. This category of crimes is likely to include both genuinely minor crimes and potentially serious crimes in which the victim nevertheless suffered neither injury nor property loss. Although NCVS interviewers encourage respondents to report less serious crimes (by asking about, and providing examples of, minor crimes) and those in which they did not suffer any harm (asking about threats without injury and attempted thefts), many such events still go unreported in the NCVS.⁶³

From the standpoint of estimating harm from crime, the failure of the NCVS to capture no-harm cases is arguably not that serious a problem. On the other hand, from the standpoint of estimating defensive gun use frequency, it is critical because the vast majority of defensive gun uses involve neither property loss nor injury to the victim, and thus are likely to go unreported in response to NCVS questions for this reason alone.⁶⁴ In many cases it may be defensive gun use itself that caused a crime to be harmless to the victim, and thus to be perceived by the victim as "minor," which in turn ensured that it was not reported in the NCVS.

Supporting this interpretation, when McDowall and his fellow researchers found respondents who reported a defensive gun use in

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response to gun survey questions but had not done so in response to previous NCVS questions, and asked these respondents to account for the conflict, the most common explanation was that the respondent did not think the crime was serious enough to mention when the NCVS victimization questions were asked.⁶⁵ Only when the question specifically referring to defensive gun use was asked did it prompt these respondents to report the crime. Some genuine no-harm defensive gun use crime incidents will not be reported in response to NCVS questions because the respondent will wrongly assume them to be too minor to qualify as relevant to the interviewer's questions, but would be reported in response to a question specifically mentioning defensive gun use because only then is the experience perceived as relevant to the interviewer's questions.

Two kinds of error in answers to defensive gun use questions will distort estimates. A "false positive" occurs when a respondent claims to have had a defensive gun use experience but did not, while a "false negative" occurs when a respondent who had a defensive gun use experience denies it. Respondents who reported a defensive gun use in response to gun survey questions but not to NCVS questions can, for convenience, be called "discrepant respondents." McDowall and his colleagues did not cite any discrepant respondents who admitted that the defensive gun use reported in response to the gun survey questions was false or distorted. Instead, when discrepant respondents were asked to account for their seemingly inconsistent responses, the only specific explanations they offered (excluding "idiosyncratic" responses peculiar to single respondents) were either that they simply misunderstood the questions or that they did not think that the criminal offenses involved were serious enough to justify a positive response (pp. 14-15). As far as one can tell from these researchers' evidence, there were (excluding cases that simply did not fit the defensive gun use definition, such as police or military uses) no false positives in their sample.

Nevertheless, the authors interpreted the discrepant reports as false positives, based on the arbitrary decision to view them as reports of incidents in which the respondent had overreacted to nonexistent threats and "crimes that had not yet happened," rather than as cases of defensive gun use in the early stages of genuine criminal attempts or cases of defensive gun use against less serious crimes. Thus, there was no logical or empirical foundation for their strongly stated conclusion that "many reports to the

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other [gun] surveys were false positives (that is, they were not defenses against criminal acts)" (p. 17). The authors did not list any individual cases of false positives or report any counts of the number of cases, among the discrepant reports, that they had confirmed to be false positives, yet they were somehow able to conclude that there were "many" of them.

The authors inserted this non sequitur conclusion into their paper only after the last draft had been reviewed by the referees. Thus, the main conclusion of the paper was not subject to peer review, although the journal that published the paper describes itself as a "refereed publication."⁶⁶ When I objected to this evasion of peer review to the editor, he refused to respond to my objections.⁶⁷

When the NCVS was revised in the early 1990s, many of the improvements consisted of increasing the number and variety of specific prompts about crime that might trigger respondents' memories, or more effectively convey to them the full set of crime experiences that the surveyors were interested in. As a result, reports of crime did indeed increase.⁶⁸ The lesson for the present debate is that questions specifically referring to defensive gun use apparently serve as better prompts than the NCVS questions for no-harm crimes involving defensive gun use, perhaps because explicit mention of victim gun use more effectively triggers recollection of crimes involving defensive gun use, and more effectively signals to respondents that their crime experience is relevant, based on their gun use, despite the otherwise "minor" character of the no-harm incident.

Telescoping as a Source of Overestimation in the Defensive Gun Use Surveys

"Forward telescoping" occurs when a respondent is asked about the past year but reports incidents that occurred more than a year in the past. Census Bureau research connected with the development of the NCVS, using a one-year recall period, indicated that telescoping could increase reports of crime incidents by no more than a factor of 1.21 (that is, one would get 1.21 times as many reports of crime as actually occurred during the recall period), and thus telescoping in the gun surveys is not likely to have more than a modest effect on defensive gun use estimates.⁶⁹

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Hemenway, however, hinted that telescoping inflated crime estimates by 30 to 40 percent, by misciting research on the NCVS reported by researcher David Cantor.⁷⁰ Cantor had noted that “bounded” interviews in the NCVS yielded 30 to 40 percent more crime reports than “unbounded” interviews. A bounded interview is one preceded by an earlier interview with the same respondent. Since a previous interview can establish what crime incidents occurred in the previous recall period, researchers can eliminate the effects of telescoping by dropping, from estimates based on the later “bounded” interview, any reports of “out-of-recall-period” events already reported in the previous interview.

For example, suppose that a person interviewed in January 2001 was asked about any crime experiences they had in the preceding six months (July to December 2000) and accurately reported that they had been robbed during that period. Suppose the person was then interviewed again six months later and asked about January to June 2001. If the person erroneously reported that the robbery from the earlier six-month period occurred during the January to June 2001 recall period (i.e., the respondent “telescoped” the incident into the later recall period), the researchers would be able to recognize this as the same incident reported in the previous interview. Thus, because the earlier interview had preceded the later one, the researchers could disqualify the telescoped incident and prevent it from inflating their estimates of the frequency of crime. Further, because interviewers can instruct respondents to report only events that occurred “since the last interview,” and the preceding interview provides an easily recalled starting point for the recall period, respondents are less likely to telescope an incident in the first place.

Cantor went to great pains to explain that the 30 to 40 percent difference in crime reports between bounded and unbounded interviews could *not* be viewed as an estimate of the magnitude of telescoping, because the differences in reports of crime were also partly due to respondents failing to report genuine crime experiences in the later interviews, due to “respondent conditioning.” That is, once respondents have reported victimizations in previous interviews, they learn that doing so will result in their having to do more work answering questions concerning details of the incidents and endure a longer interview. This causes some of them to falsely deny, in later “bounded” interviews, having being victimized during the most recent

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recall period. Cantor cautioned that "it is unclear to what degree the drop in reports [of crime] between the first (unbounded) and second (bounded) interviews is due to telescoping or conditioning" (p. 33).

Hemenway, however, cited Cantor's 30 to 40 percent figure without Cantor's caveats, in a sentence immediately following a reference to "a substantial amount of telescoping of criminal victimization" and in the context of a discussion of "false positives" in defensive gun use surveys.⁷¹ Cook and Ludwig also cited the 30 to 40 percent figure in the context of a discussion of telescoping, without mentioning Cantor's caveats.⁷² Cook, Ludwig, and Hemenway aggravated the misrepresentation even further by boosting Cantor's 30 to 40 percent to "over 50 percent."⁷³ Smith even justified his guess of "around 50 percent" telescoping in defensive gun use surveys by citing (in addition to the nonsupportive Cantor report) the degree of telescoping in reports of consumer expenditures on household repairs and of visits to the doctor!⁷⁴ There is in fact no evidence that forward telescoping causes any more than the upper-limit 21 percent increase in DGU reports that was cited in the original Kleck and Gertz article.⁷⁵ In short, telescoping is not likely to cause any significant exaggeration of the frequency of defensive gun use.

Telescoping is not only a fairly minor source of overestimation, but is also counterbalanced by response errors in the opposite direction. Census Bureau evaluations of victim survey methods indicated that for one-year recall periods, the magnitude of overreporting of criminal victimizations due to telescoping is roughly matched by underreporting due to forgetting and other failures to report incidents which actually did occur in the reference period.⁷⁶ Thus, false positives are balanced out by false negatives.

The Scholarly Response to Large Defensive Gun Use Estimates

Faced with a huge body of evidence contradicting the rare-defensive gun use theory, its supporters have had little choice but to simply promote the dubious NCVS "estimate" and ignore or discount everything else. Articles in medical and public health journals are typically the most misleading about the volume of contrary evidence, since they usually do not even mention the exis-

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tence of a large volume of evidence pointing to huge numbers of defensive gun uses. For example, Kellermann and his colleagues mentioned only the NCVS estimate when telling their readers that defensive gun use is rare. While it is possible that these authors did not know of all fifteen of the other surveys that had been conducted by the time their article was written, they certainly should have known of the existence of at least six contradictory surveys, since they were prominently reviewed in a source that they cited.⁷⁷

Further, editors have ensured near-total censorship of contrary information through their publication decisions (see chapter 2). And although these journals commonly provide for letters to the editor, editors of the journals have refused to publish rebuttals or even brief letters challenging the rare-defensive gun use thesis, aside from letters by employees of gun owner organizations, whose views can be easily dismissed by readers as biased. For example, when I wrote a brief letter to the editor to the *American Journal of Public Health* to point out that the journal had published a grossly inaccurate estimate (on the low side) of the frequency of defensive gun uses [McDowall and Wiersema 1994], the editors refused to publish the letter.⁷⁸

Writers publishing in criminological and social science outlets, where editors and referees tend to be more knowledgeable about violence, must be a bit more sophisticated in obscuring the extent of contrary evidence. Some do so by conceding the existence of contrary evidence but being vague about how much of it there is. For example, sociologists Albert Reiss and Jeffrey Roth obscured the extent of the contradictory evidence by vaguely referring to “a number of surveys” that implied larger estimates and then dropping the matter, with no detailed further discussion of any of the other surveys.⁷⁹ Then, later in their essay, they uncritically accepted the dubious NCVS estimate at face value (p. 266), effectively ignoring all the contrary sources. Even at the time they wrote, there were at least eight other surveys yielding defensive gun use estimates, all many times higher than the NCVS estimate. These surveys had been reviewed in sources they cited.⁸⁰ Likewise Cook blandly alluded to “a number of surveys,” without indicating how many there were, and giving detailed attention to only one of them.⁸¹ University of Maryland criminologists David McDowall and Brian Wiersema gave readers the impression that conclusions in an earlier article were based on results of a single survey, simply by not mentioning any of the other five surveys on which the article’s author relied.⁸²

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On those rare occasions when adherents of the rare-defensive gun use thesis address some of the contrary evidence, they counter empirical evidence with a dense web of one-sided speculations rather than offering better empirical information. Cook set the pattern, speculating that early polls yielding higher defensive gun use estimates did so because respondents were telescoping incidents into the recall period.⁸³ As previously noted, telescoping has only mild effects that are cancelled out by the effects of respondents forgetting or intentionally failing to report genuine defensive gun uses. Cook offered no evidence that any gun use survey, or indeed any crime-related surveys, are afflicted by more telescoping than recall failure.

Speculation about the flaws in surveys estimating large numbers of defensive gun uses resemble UFO buffs' beliefs that the federal government captured aliens from other worlds at Roswell, New Mexico, in 1947. The reason most people do not share these beliefs is not that they can be proven false—they cannot, since it is impossible to prove a negative. Rather, most people reject them because there is no credible evidence that they *are* true. It is the same with speculations about massive overreporting of defensive gun uses. Since it is impossible to prove a negative, one cannot prove that frequent misreporting of nonexistent defensive gun use incidents does not occur in the gun surveys. There is, however, no affirmative evidence whatsoever that such misreporting occurs often enough to outweigh misreporting in the opposite direction.

In this context, Cook's earlier assessment of the NCVS's adequacy for estimating gunshot wound incidents is worth closer study.⁸⁴ When faced with a conflict between low NCVS estimates of the number of criminal gunshot woundings, and far higher estimates implied by data from just four other small samples (all but one of them local in scope), Cook quite reasonably concluded that the NCVS radically underestimated gunshot woundings and that the true figure could be three times larger than the NCVS-based estimate. His reasoning was that the four small-scale bodies of data could not all be radically wrong, so it must be the NCVS estimates that were wrong.

In sharp contrast, when faced with precisely the same conflict between low NCVS estimates of defensive gun uses and no less than thirteen other conflicting bodies of data (nearly all national in scope), Cook concluded that all of the other bodies of evidence were radically wrong, that it was the NCVS that was likely to be more correct, and that it was unlikely that the NCVS could be as seriously inaccurate as the conflicting bodies of data indicated.⁸⁵

Fallacious Reductio ad Absurdum Reasoning

Reporting results from the National Survey of the Private Ownership of Firearms (NSPOF), Cook and Ludwig claimed to have established conflicts between their defensive gun use estimates and crime estimates drawn from other sources, concluding that the defensive gun use results must be erroneous because they implied (1) numbers of defensive gun uses that would claim implausibly large fractions of all crimes in a given category, or (2) implausibly large numbers of defensive wounding of criminals.³⁸ Their reasoning was fallacious because it relied on the indisputably false premise that we know the total number of crimes in a given category and the total number of gunshot wounding, whether medically treated or not.

The authors cited data on the number of people treated in emergency rooms for nonfatal gunshot wounds and asserted that their own survey's estimates of criminals wounded during defensive gun uses were implausibly high in comparison. In fact, the two sets of numbers are perfectly consistent. Cook and Ludwig effectively ignored the fact that the wounding figures they cited were only estimates of the frequency of gunshot wounding treated in hospital emergency departments, not the *total* number of gunshot wounding.

It is highly unlikely that most criminals wounded by victims would seek medical treatment, since medical personnel are required by law to report gunshot wounds to the police.³⁹ Because most gunshot wounding are survivable with no more sophisticated medical care than can be administered by the average lay person, most wounded criminals can afford to choose self-treatment over professional treatment at a hospital.⁴⁰ And all have extremely strong legal reasons to do so, since seeking professional medical treatment would often be tantamount to surrendering to police for the crime in which they had been wounded. Cook and Ludwig disputed the claim that most such criminals would not receive emergency room treatment, but not by citing contrary evidence or pointing to some logical flaw in the evidence or arguments supporting this assertion. Instead, they simply issued the rather impetuous pronouncement that “we find that possibility rather unlikely.”³⁹ They offered no further rationale for this remarkable assessment.

Cook and Ludwig also asserted that the estimated numbers of defensive gun uses connected with particular types of crimes were inconsistent with

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the total number of crimes of that type, with or without defensive gun uses.⁹⁰ For example, they claimed to have shown that the estimated number of defensive gun uses linked with rapes actually exceeded the total number of rapes, as estimated by the NCVS.

Alternate sources of information indicate that only a minority of all crime incidents get reported to the NCVS.⁹¹ Therefore, no matter how large the estimated number of defensive gun uses are in the gun surveys, they could still be a plausibly modest share of *all* crime incidents, including both those effectively covered by the NCVS and those not covered. Consequently, comparing defensive gun use estimates with NCVS estimates of crime or estimates of medically treated gunshot woundings can tell us nothing about whether defensive gun use estimates are plausible.

To give Cook and Ludwig's argument its strongest chance to appear credible, consider rape, the only crime category where the estimated number of crimes with a defensive gun use supposedly exceeded the total number of such crimes as estimated by the NCVS. The NSPOF results, according to Cook and Ludwig, implied 322,000 rapes and attempted rapes in which the victim used a gun defensively, while the NCVS for the same year indicated a total of only 316,000 rapes and attempted rapes, with or without defensive gun use. The defensive gun use figure, however, exceeds the crime total only because Cook and Ludwig made an invalid comparison. The NSPOF defensive gun use estimates actually pertained to defensive gun uses linked with "rape, attempted rape, *other sexual assault*" (emphasis added), while the corresponding number from the NCVS used by Cook and Ludwig, 316,000 covered only "Rape/Attempted rape," even though the NCVS also provided an estimate for the separate category of "Sexual Assault."⁹² When the correct figures from properly corresponding categories are used, the NCVS figure is 432,750 rapes, attempted rapes, *and* sexual assaults. The NSPOF estimate of 322,000 defensive gun uses linked with such crimes therefore did not even come close to exceeding the NCVS estimate of the total number of such crimes. Thus, Cook and Ludwig's only instance of a defensive gun use estimate that was supposed to be impossible (as distinct from subjectively "implausible") turns out to be the product of their error.

Nevertheless, even correcting for this mistake, the rape defensive gun use estimate still looks implausibly high at first glance. Cook and Ludwig, however, also reported that their 95 percent confidence interval estimate

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(which reflects the possible effects of random error in selecting survey respondents) of rape defensive gun uses was 12,000 to 632,000.⁹³ Unlike estimates of *total* defensive gun uses derived from the NSPOF (and NSDS), which are based on large samples, estimates of defensive gun uses linked with particular subtypes of crime depend on knowing the percent of defensive gun uses linked with a given type of crime, which is based on the far smaller number of defensive gun use cases and is thus highly unstable. Given the lower limit of this extremely imprecise estimate, the NSPOF estimate of rape-linked defensive gun uses was not even mildly implausible compared with the NCVS estimate of all rapes, since the lower limit estimate of 12,000 rapes/sexual assaults with defensive gun use would be less than 3 percent of total rapes and sexual assaults. Therefore, even the appearance of “implausibility” could be the product of nothing more than random sampling error, which in turn was due to the NSPOF’s small sample size. There is something vaguely dubious about researchers’ arguments being *strengthened* by the inadequacies of their research—the smaller their sample and the more unstable Cook and Ludwig’s estimates were, the better their chances of obtaining “implausible” results that would buttress their theory of massive defensive gun use overestimation.

The critics’ *reductio ad absurdum* logic is equivalent to arguing that Gallup presidential election polls cannot accurately estimate the share of the entire electorate voting for the Democratic candidate (something we know they *can* do, usually to within 2 percentage points) because they routinely yield implausible estimates for small subsets of the electorate, such as elderly Hispanic females.⁹⁴ One undoubtedly could obtain implausible estimates of voter preference for the Democratic candidate, such as 0 percent or 100 percent, based on a very small number of sample cases, for many subsets of the population. This would imply nothing, however, about the ability of the survey to estimate voter preferences in the entire population. Thus, even if estimates of defensive gun uses linked to a given specific crime type were implausible, which they are not, this would imply nothing about whether estimates of the total number of defensive gun uses, based on the full sample, are accurate.

In any case, the Kleck-Gertz NSDS, which had twice the sample size of the Cook-Ludwig NSPOF and thus considerably less unstable defensive gun use estimates, implied only 209,089 defensive gun uses linked with

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rapes or sexual assaults.⁹⁵ This is less than half the NCVS estimate of "total" crimes in this category. In no crime category were NSDS estimates of defensive gun uses even half the number of NCVS-estimated offenses in the corresponding category.

But, again to give Cook and Ludwig's argument every chance of appearing reasonable, ignore their invalid comparison, ignore sampling error due to an inadequate sample size, and consider just their point estimate of 322,000 rape-linked defensive gun uses. The biggest problem still remains: the NCVS estimate of rape frequency is not complete or exhaustive, and the true total number of rapes is almost certainly far higher. Indeed, rape estimates derived from the present NCVS are two and a half times the size of those derived from earlier versions of the same survey.⁹⁶ If the true number of rapes were actually far higher than the NCVS estimate of 316,000, there would be nothing even mildly implausible about there having been 322,000 rapes with DGU.

How much higher could the actual number of rapes be? Reviewing the results of surveys specially designed to study rape, Loftin and MacKenzie (1990) found that the total number of rapes could be as much as thirty-three times as high as NCVS estimates.⁹⁷ Thus, instead of there being only 316,000 total rapes per year, there could actually have been anywhere from 316,000 on up to a possible (albeit unlikely) 10.4 million. In sum, there turns out to be no logical foundation whatsoever for the claim that 322,000 rape-linked defensive gun uses is implausible.

Nevertheless, as an indication of just how far Cook and Ludwig were willing to take this reasoning, consider their assertion that "even if the NCVS-based estimates of criminal victimization rates are off by an order of magnitude, the NSPOF-based estimates for DGU are implausible."⁹⁸ (The phrase "even if" seems to hint that this degree of underestimation is highly unlikely and that the authors were thus making a generous assumption.) They reported that the NCVS estimates of aggravated assault and robbery in 1994 were 2.48 million and 1.30 million, respectively (p. 123), so if these figures were off by an order of magnitude, Cook and Ludwig were asserting that the defensive gun use estimates would be implausible even if there were in fact 24.8 million aggravated assaults and 13.0 million robberies a year. Yet the NSPOF had implied only 462,000 aggravated assaults and 527,000 robberies with defensive gun use a year. Thus, Cook and

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Ludwig were stating that even if defensive gun use data indicated that only 1.9 percent of aggravated assaults or 1.1 percent of robberies involved defensive gun use, they would still regard such defensive gun use estimates as “implausible.” It is useful to keep this passage in mind in interpreting what these authors mean when they use the term “implausible.”

The Venn diagram in figure 6.1 illustrates hypotheses about the relative frequency of different sets of crime events and the degree of their overlap. The area of circle A represents the true volume of crime that involved a victim-offender confrontation (violent crimes, burglary attempts interrupted by victims, purse-snatchings, etc.), i.e., crimes where a defensive gun use was possible. Circle B represents confrontation crimes that would be captured by the NCVS if everyone in the population were interviewed. Its volume is a third that of circle A, symbolizing the assumption that only a third of confrontation crimes are captured by the NCVS. Circle C represents the set of all defensive gun uses that occurred (all, by definition, in connection with confrontation crimes), whether they could be captured by any survey or not. Its volume is 10 percent that of circle A, reflecting the assumption that a modest 10 percent of confrontation crimes involve a genuine defensive gun use. Finally, oval D represents the set of defensive gun use incidents that would be reported in private surveys like the NSDS or NSPOF if everyone in the population were interviewed.

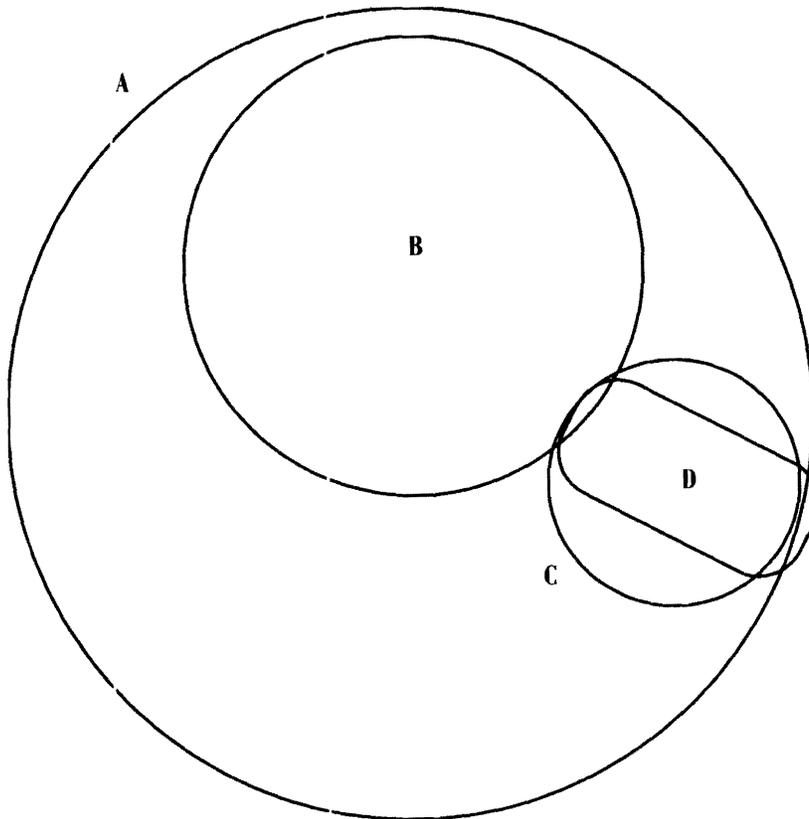
The volume of oval D implies that private surveys capture only about 50 percent of true defensive gun uses, i.e., there are many false negatives (the area within C but outside D) in the private surveys, while the small share of D that lies outside of C or A implies that there would be few false positives. The minimal overlap between B and C denotes that only a tiny share of true defensive gun uses would be captured by the NCVS. Likewise, the minimal overlap between B and D indicates that almost all of the true defensive gun use incidents that can be captured by the private surveys lie outside the set of incidents that victims are willing to report in the NCVS. Thus, defensive gun uses are for the most part not a subset of crimes that can be captured by the NCVS.

The relative volumes and positioning of these shapes are necessarily hypothetical, given the impossibility of estimating unreported crimes and defensive gun uses, but the diagram is fully consistent with what we do know. The diagram does not prove that any given defensive gun use estimate is correct, but it does prove that the Hemenway-Cook-Ludwig mode of

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Figure 6.1 Hypothetical Distribution of Crimes and Defensive Gun Use Incidents



- A = The set of all crimes with a victim-offender confrontation
- B = The set of confrontation crimes that would be captured by the NCVS (assumed to be 33 percent of A)
- C = The set of all confrontation crimes with a true defensive gun use (assumed to be 10 percent of A)
- D = The set of confrontation crimes that would be reported in private surveys as involving defensive gun use (5 percent of A, 50 percent of C)

challenging defensive gun use estimates is invalid, since it demonstrates that the defensive gun use estimates based on the private gun surveys are fully compatible with the numbers of crimes reported in the NCVS.

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Cook and Ludwig's fallacious claims about implausibly high defensive gun use estimates were later accepted by McDowall and his colleagues, who flatly stated that the defensive gun use outcomes of the gun surveys were "contrary to external evidence," citing Cook and Ludwig.⁹⁹ McDowall et al. had apparently read the Kleck and Gertz explanation of why this reasoning was invalid, and neither they nor anyone else has rebutted that explanation.¹⁰⁰

This dispute cannot reasonably be regarded as just an honest difference of opinion. It is an incontrovertible and obvious logical point that it is impossible to know the total number of rapes, aggravated assaults, robberies, residential burglaries, or gunshot woundings. Indeed, none of the advocates of the rare-defensive gun use theory have disputed this. Therefore, it is impossible to know whether a given estimate of the number of defensive gun uses linked with any one of these types of events would characterize an implausibly large fraction of the (unknowable) total number of events.

McDowall and his coauthors also repeated the same mischaracterization of gunshot wounding estimates that Cook and Ludwig had disseminated, claiming that the number of woundings of criminals implied by the defensive gun use surveys "was larger than independent estimates of *all* serious firearm injuries in the nation."¹⁰¹ This was plainly inaccurate. The source on which the assertion was based never even claimed to be estimating all gunshot woundings, but rather only firearm injuries "treated in hospital emergency departments."¹⁰² Further, it had been painstakingly explained in multiple sources, which McDowall et al. cited, that the emergency-room estimates to which they alluded were *not* estimates of total woundings and thus could not place an upper limit on the number of defensive gun use-linked woundings.¹⁰³ To make it clear that their acceptance of the fallacious reasoning was not inadvertent, McDowall et al. repeated it later in the article: "[Hemenway, Cook, and Ludwig] also show that the Kleck and Gertz study suggests armed defense against implausibly large fractions of other violent crimes."¹⁰⁴

The Meaning of Internal Inconsistencies in Defensive Gun Use Reports

Cook and Ludwig reported inconsistencies between responses to different ques-

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tions in seven reported cases of defensive gun use, and concluded that these inconsistencies indicated deliberate falsification of defensive gun use accounts.¹⁰⁵ The main inconsistency, found in all but one of the seven reports, involved respondents reporting a defensive gun use that they asserted (in response to question 72) was linked with a serious violent crime such as a robbery, assault, or rape. But just three questions (and perhaps thirty seconds) later in the interview, these respondents were also recorded as having reported (in response to question 75) that they had not been threatened, attacked or injured.¹⁰⁶

This is indeed an inconsistency, since every violent crime, by definition, involves an attack or threat to the victim. The question is whether the inconsistency indicates that (1) respondents alleging defensive gun uses were inventing fake events or falsifying parts of their accounts, as Cook and Ludwig preferred to believe, or whether (2) the inconsistencies were the result of survey staff error in recording responses or honest mistakes by respondents with a genuine defensive gun use to report.¹⁰⁷

Cook and Ludwig arbitrarily rejected the staff error interpretation, based on the fact that 10 percent of interviews were checked by "senior staff" of the survey organization.¹⁰⁸ This was arbitrary because all seven, and most likely six, of the inconsistent defensive gun use cases could have fallen in to the unchecked 90 percent. Further, if the "senior staff" were as incapable of recognizing inconsistencies that needed to be resolved as interviewers, supervisor checks would be useless.

The assertion that staff error of some sort occurred in these cases is in one sense indisputably true, since the interviewers and staff both obviously failed to correct conspicuous inconsistencies—the interviewers did not probe for a clarification during the interview, and supervisors did not resolve the inconsistencies afterwards. Even if the inconsistencies *had* been due to respondents falsifying their accounts, the evidence still indicates failures on the part of the survey staff to resolve inconsistencies. Further, data cleaning is supposed to catch and resolve these kinds of flaws, but obviously did not. Thus, these cases indicate, at minimum, poor performance by members of the survey staff.

Even apart from staff error, one would expect a few inconsistencies in honest and otherwise accurate accounts of genuine defensive gun uses. Humans are not error-free robots even when they are being completely honest. In the NSPOE, 50 respondents initially reported a defensive gun use

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against a human in the past year, and were asked as many as 27 questions concerning details of the incident, potentially providing as many as 1,350 responses that could be crosschecked against one another.¹⁰⁹ After what one suspects was an exhaustive search for inconsistencies, Cook and Ludwig identified all of eight of them (including one incident with two inconsistencies).¹¹⁰ Some people would be inclined to see this as evidence of remarkably consistent reporting on defensive gun uses, but Cook and Ludwig effectively interpreted anything short of flawless reporting as evidence of falsified defensive gun use accounts.

A less results-oriented approach would be to simply consider which of two alternative interpretations is more plausible: (1) simple staff error and a handful of honest errors by respondents with genuine defensive gun uses to report, or (2) deliberate falsification. To accept the falsification interpretation in this instance requires one to believe that respondents fabricating imaginary events were so inept that they could not keep their stories straight for even thirty seconds at a time, providing blatantly inconsistent responses to questions separated by just two intervening items. Even if respondent falsification were common, falsification this inept seems far-fetched. Further, the methodological experiment of McDowall et al. found no significant evidence of falsification of defensive gun use reports.¹¹¹

A less strained interpretation is that even honest respondents will provide a few inconsistent details about genuine experiences, and that careless or poorly trained interviewers occasionally record responses incorrectly, especially on the more complicated questions. For example, some respondents or interviewers may simply have misinterpreted question 75 as an inquiry about any *additional* threat or injury beyond that already established in question 72.

Cook and Ludwig's carefully phrased conclusion was: "At the very least, these apparent contradictions are *consistent with* the idea that a sizable share of DGU reporters are falsifying part of their accounts."¹¹² Perhaps, but the fact that some of the respondents were wearing red sweaters when they were interviewed would also be "consistent with" this theory, but hardly supportive of it. The inconsistencies are even more "consistent with" simple staff error or the mistakes that would inevitably be made by respondents with genuine defensive gun uses to report.

LABORATORY

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Direct Evidence on the Relative Balance of False Positives and False Negatives

Cook and Ludwig were willing to arbitrarily infer respondent falsification from evidence (the seven inconsistent cases) that was of only the most dubious relevance. In contrast, they did not share with readers other information in their possession that bore far more directly on the issue of deliberate falsification of responses. At the end of every NSPOF interview, interviewers were asked to provide their own assessments as to whether respondents either (1) hesitated or otherwise expressed reluctance to answer the initial question about defensive gun use, indicating to the interviewer that the "respondent seemed to be concealing defensive gun use," or (2) gave the impression that the respondent was making up the incident or trying to mislead the interviewer about his/her role in it.¹¹³

Reanalysis of their dataset provided the results that Cook and Ludwig did not publish.¹¹⁴ The interviewers reported that respondents seemed to be concealing a defensive gun use in sixty cases, while, within the full pool of reported defensive gun uses in the previous five years, interviewers believed that respondents were either inventing a nonexistent event or misstating their role in it in just thirteen cases. Thus, suspected false negatives outnumbered suspected false positives by a factor of 4.6 to one. If one corrected for both suspected false positives and suspected false negatives, instead of only false positives as Cook and Ludwig did, it would increase the defensive gun use estimate, not decrease it.

To be sure, one could speculate that interviewers, despite the advantages of actually speaking with respondents and hearing their voices and speech patterns, nevertheless could not reliably detect false responses, and to therefore dismiss this inconvenient evidence. It would be harder for Cook and Ludwig to make this argument, however, since they trusted interviewers' judgments enough to exclude claimed defensive gun uses that interviewers suspected to be false positives, solely on the basis of the interviewers' judgments.¹¹⁵ One could not, however, reasonably argue that these interviewer assessments are completely irrelevant to the issue of misreporting, or less relevant than the handful of ambiguous inconsistencies given so much weight by Cook and Ludwig. Concerns about interviewers' ability to perceive respondent dishonesty might justify reporting their assessments to readers

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and then attaching caveats to the findings, but it cannot justify withholding from readers the only direct evidence available to Cook and Ludwig on the relative balance of false positives and false negatives.

Circular Reasoning in Numerical Exercises on the Sensitivity of Defensive Gun Use Estimates to False Positives

If only one respondent in a survey had experienced a genuine defensive gun use, then just one more respondent falsely reporting a nonexistent defensive gun use would double the resulting defensive gun use estimate, making such an estimate highly sensitive to the problem of false positives. Note, however, that this sensitivity is a function of the actual defensive gun use frequency. If, in contrast, sixty-six sample respondents had a genuine defensive gun use to report, one or a few false positives would have little effect on estimates, and would have no net effect at all if counterbalanced by false negatives.

Hemenway presented a numerical exercise that, somewhat needlessly, confirmed the self-evident general point that estimates of rare phenomena *could* be highly sensitive to false positives, but also falsely claimed to have demonstrated that in fact the defensive gun use estimates from the NSDS *were* highly sensitive to, and greatly exaggerated by, false positives.¹¹⁶ He started by pointing out something that is necessarily true of estimates of any relatively uncommon phenomenon. Since there are only a few people with actual defensive gun uses to report, there are only a few people who could provide false negative responses (i.e., falsely deny a true defensive gun use experience), while there are many who potentially could provide false positive responses. Part of the problem seemed to be that Hemenway made no distinction between two very different kinds of claims: (1) There is more hypothetical *potential* for false positives than false negatives, and (2) There are *actually* more false positives than false negatives in defensive gun use surveys. The first statement is a trivial statement of arithmetic fact, while the latter is a non sequitur assertion without the slightest empirical support.

It is worth noting that neither Hemenway nor those who uncritically cite his novel ideas (Cook, Ludwig, McDowall et al.) have ever applied this

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reasoning to survey estimates of gun crime frequency.¹¹⁷ Since less than 1 percent of the U.S. population is the victim of a gun crime in any given year, this means that less than 1 percent of the population *could* provide a false negative response, while over 99 percent *could* provide false positives.¹¹⁸ According to Hemenway's "logic" it is a virtual inevitability that, given such a huge potential for false positives, the NCVS will grossly overstate the frequency of gun crime (and thereby grossly overstate the need for stricter gun control). Either the critics are not interested in a balanced application of such methodological "insights," or they do not really believe in this line of reasoning.

But the core fallacy of Hemenway's argument was that he had to assume his conclusions in order to prove them, an obvious instance of circular reasoning. His assumptions/conclusions were that (1) the true defensive gun use rate is far lower than repeated empirical estimates, such as those of the NSDS, indicate, (2) there is a far higher rate of false positives in reporting defensive gun uses than empirical evidence indicates, and (3) contrary to evidence from other crime-related surveys, the false negative rate is low enough that it produces far fewer false negatives than false positives in a defensive gun use survey.

The argument that Hemenway built on these assumptions, endorsed by Cook and Ludwig and McDowell et al., was as follows:

- (1) Despite the unanimous findings to the contrary of nineteen consecutive surveys, defensive gun use is actually extremely rare.
- (2) Because defensive gun use is extremely rare, the survey estimates are extremely sensitive to false positives, and therefore not credible.
- (3) Because all of the survey estimates are not credible, we can dismiss them.
- (4) Since we can dismiss all the survey evidence, no credible evidence remains to indicate that defensive gun use is common, and therefore we can conclude that defensive gun use is extremely rare.¹¹⁹

The fallacious character of Hemenway's argument can be illustrated by comparing the implications of two different sets of hypothetical survey results, based on two different sets of starting assumptions: (1) a set that assumes the conclusions that Hemenway was trying to support, and (2) a set

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that reflects preexisting empirical evidence (correct or not), but does not embody the conclusory assumptions. In table 6.3, panel A repeats one of Hemenway's tables, in which he assumed the aforementioned conclusions.¹²⁰ More specifically, he assumed that (1) defensive gun use is virtually nonexistent—it was assumed that only 0.01 percent of respondents had a true defensive gun use in the past year, (2) there were a huge number of false positives in the hypothetical survey—thirty-three times as many as true positives, and (3) there were no false negatives at all.

Panel B shows an alternative version of the table more consistent with empirical evidence. First, it assumes that the true defensive gun use rate is actually somewhat higher than the best surveys indicate (2.04 percent versus 1.33 percent) due to the underreporting common in surveys of other crime-related phenomena.¹²² Second, it assumes that false negatives outnumber false positives by a factor of 1.6 to 1, as was indicated by interviewer judgments in the NSPOF.

Comparison of the two tables illustrates that under more realistic assumptions (panel B), the defensive gun use estimate is not very sensitive to false positives, and that the modest effects of these errors are more than counterbalanced by the effects of false negatives. The only reason that Hemenway's table (or either of the other two in his article) supported his claims is because he assumed that defensive gun use is extremely rare, the very issue that was in dispute.¹²³

If one does not assume that defensive gun use is virtually nonexistent (panel A), but instead assumes it to be about what the best available surveys indicate (panel B), the number of false positives is not nearly as large relative to true positives. Further, unless one assumes a ratio of false positives over false negatives far higher than is indicated either in defensive gun use surveys or in any other crime-related surveys, the false positives do not cause any net overestimate, never mind the enormous overestimates in which Hemenway believes. One can dispute whether the assumptions underlying panel B are in fact true, but one cannot dispute that Hemenway's conclusions only follow if one accepts them as starting premises of his numerical exercise. His logic is clearly circular and thus fallacious.

Cook and Ludwig later repeated both Hemenway's exercise and his fallacious logic.¹²⁴ Like Hemenway, they arbitrarily assumed their conclusions that defensive gun use is virtually nonexistent (assuming it to be 0.1

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percent, only 1/13th of the empirically based 1.326 percent) and that nearly all of the people who report defensive gun uses are lying or otherwise wrong. Again, this example would not have supported their conclusions unless it had incorporated the very conclusion they were advocating, that defensive gun use is extremely rare.

One-sided Consideration of Errors in Defensive Gun Use Estimation

Speculation unsupported by evidence is a weak enough foundation for assessing evidence. But it is especially misleading to speculate only in one direction, in this case speculating only about flaws suspected to push defensive gun use estimates up. If one is not willing to seriously consider errors in both directions, one is simply engaging in adversary scholarship or "sagecraft," an enterprise aimed not at discovering the truth, but rather at buttressing predetermined positions.¹²⁵ For an overt example of this sort of adversary scholarship, see an article by David Hemenway, along with the response to it by Kleck and Gertz.¹²⁶

The most prevalent, consequential, and crude variety of bias among advocates of the rare-defensive gun use theory is the refusal to consider seriously the possibility of false negatives, and thereby address the relative balance of false positives and false negatives.¹²⁷ Without exception, the advocates simply assume that the former outnumber the latter. For example, McDowall et al. (2000, p. 5) noted that there are only a relatively small number of positive responses in defensive gun use surveys, so that "a few false-positive reports could wildly inflate the results," but did not mention the equally obvious implication that equally infrequent false negative reports could wildly *reduce* estimates.¹²⁸

Cook and Ludwig tried to buttress their theory of massive overestimation of defensive gun uses by citing the false positive rate found in a study of drug use that compared self-reports in interviews with urinalysis results, but did not mention to readers the false negatives reported in that same study.¹²⁹ For every drug evaluated, the study had indicated that false positives were rare and, more important, that false negatives outnumbered false positives, producing a net under-estimate of illicit drug use.¹³⁰

258 **ARMED****Table 6.3 Circular Reasoning and the False Positive Issue—
Hypothetical Distributions of Responses to Survey Questions****Panel A Hemenway's Table 2B¹²¹— Figures Assume the Conclusions**

		Reality		
		DGU	No DGU	Total
Response	Yes	2	64	66
to DGU	No	0	4634	4934
Question	Total	2	4698	5000

Estimated DGU rate: $66/5000=1.33$ percentActual DGU rate: $2/5000=0.04$ percent**Panel B Alternate Figures—Do Not Assume the Conclusions**

		Reality		
		DGU	No DGU	Total
Response	Yes	56	10	66
to DGU	No	46	4888	4934
Question	Total	102	4898	5000

Estimated DGU rate: $66/5000=1.33$ percentActual DGU rate: $102/5000=2.04$ percent

Both kinds of response errors almost certainly occur in all defensive gun use surveys, but no matter how many false positives there were, defensive gun use frequency would still not be even slightly overestimated if the number of false negatives were even larger. No one knows how many of each type of error occurs in defensive gun use surveys, but the most direct evidence bearing on this question was information from the NSPOF on interviewer assessments of respondents, which indicated that suspected false negatives outnumbered suspected false positives by a factor of 4.6 (or 4.6 to 1).

There is also a large technical literature on accuracy of responses in crime-related surveys, which advocates of the rare-defensive gun use thesis have declined to confront. This is perhaps understandable, given that this literature indicates that false positive responses are rare in crime-related surveys, and are greatly outnumbered by false negatives.¹³¹ In sum, while no one knows for sure what the relative balance is in defensive gun use surveys, the only direct evidence on the question indicates that false negatives are far more common than false positives, while the indirectly relevant evidence drawn from surveys of other crime-related experiences indicates the same thing.

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Cook and Ludwig frankly admitted that they focused their methodological assessment of defensive gun use surveys exclusively on false positives. They justified this one-sidedness by citing their fallacious *reductio ad absurdum* reasoning and their misinterpretation of the seven inconsistent defensive gun use reports: "Of course it is possible that there are also one or more false negatives in this survey. We focus on the problem of false positives because of the logic of estimating rare events [a reference to their numerical exercise based on circular reasoning], and because we have been persuaded by the evidence offered earlier that the NSPOF estimates overstate the true incidence by a very wide margin [apparently a reference to their fallacious claims that defensive gun use estimates were implausibly high relative to NCVS crime estimates]."¹³² That is, they justified the one-sided character of their analysis of errors by citing *previous* one-sided analyses of errors.

Consistent with this bias, Cook and Ludwig's recommendations for "improving" defensive gun use estimates focused exclusively on reducing false positives, an agenda that could only result in revising the defensive gun use estimate downwards.¹³³ If, however, surveys underestimate defensive gun use frequency, just as they underestimate all other known crime-related experiences, adopting these one-sided suggestions would be worse than useless. By eliminating or adjusting for false positives, while leaving false negatives untouched, they would make estimates that were already too low even worse.

Cook and Ludwig made similarly one-sided use of interviewer judgments about whether respondents were providing false positive or false negative defensive gun use responses. They considered interviewers' judgments to be reliable enough to exclude from defensive gun use estimates those cases in which the interviewer suspected that the respondent was falsely claiming a defensive gun use but did not adjust estimates in any way for the far more numerous cases in which interviewers believed a respondent had falsely denied a defensive gun use in response to the initial defensive gun use question.¹³⁴ Indeed, they did not even reveal to readers that cases believed by interviewers to be false negatives even existed, never mind that they were 4.6 times more common than cases the interviewers believed to be false positives. In short, they were willing to use interviewers' perceptions about possible errors to adjust defensive gun use estimates downward, but not upward.

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Identifying flaws in research can never be, by itself, a justification for dismissing findings or ignoring them altogether. Rather, the identification of known flaws can only justify reducing, in proportion to the seriousness of the known flaws, the weight given to the evidence in drawing conclusions. Evidence based on research that is, on the whole (rather than based on just one or two arbitrarily selected criteria), more technically sound, should be given relatively more weight than evidence generated by less sound research. Speculation about possible, but unsubstantiated, flaws should not be given any weight at all. And still worse than unsubstantiated speculation about flaws is one-sided speculation, since it is both uninformative and systematically distorts readers' understanding of the issues.

Thus, the most pointless and counterproductive mode of assessing evidence is to go down a list of methodology textbook problems and speculate that each one afflicts whatever research one is ideologically uncomfortable with, while declining to do the same with research generating more congenial findings.¹³⁵

Ideologues favor these critical practices because they are powerful and infinitely flexible—they can be used to dismiss even the largest, most consistent, and technically sound body of evidence, and can be used just as easily to discredit good research as bad. The practices can thereby be used to preserve even the most inaccurate beliefs about the world. No preferred proposition about the nature of world need ever be rejected no matter how false, and any proposition, no matter how true, can be made to appear false.

The Rare-Defensive Gun Use Theory: The Creation Science of Criminology

The reasoning used by advocates of the rare-defensive gun use theory is essentially identical to that used by "creation scientists" in challenging modern evolutionary theory. Just as creation scientists propose nonfalsifiable propositions concerning the origins of matter and energy, rare-defensive gun use advocates assert that defensive gun use is rare, and if survey evidence indicates this proposition is false, it only means that nearly all survey estimates of defensive gun use are fraudulent or distorted. Creation scientists speculate about flaws in radiocarbon techniques for dating rocks and fossils

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to support their view that the earth is too young to have allowed evolution of the species, just as rare-defensive gun use advocates speculate that flaws in survey methods “inevitably” result in overstating the frequency of rare phenomena such as defensive gun uses. Creationists speculate about the meaning of gaps in the fossil record of species, and rare-defensive gun use promoters similarly speculate about why survey Rs might fabricate accounts of nonexistent defensive gun uses, without providing any affirmative evidence of such fabrication actually occurring with any significant frequency.

Creationists bring up evidence that is at least as consistent with evolutionary theory as it is with creation science, such as indications of cataclysmic events like floods, but arbitrarily interpret it as if it were only consistent with their preferred theory. Likewise, when McDowall and his fellow researchers found that questions used in the defensive gun use surveys generate more reports of defensive gun uses than questions used in the NCVS, evidence equally consistent with the view that the NCVS generates many false negatives as with the view that defensive gun use survey questions generate many false positives, the authors arbitrarily interpreted the information as indicating that there were “many false positives” in the defensive gun use surveys. And when Cook and Ludwig found that respondents reporting defensive gun uses occasionally provided a few inconsistent responses about the details of their experiences, instead of interpreting these as the inevitable reflection of imperfect understanding of survey questions and flawed recollections of genuine defensive gun use experiences, they arbitrarily interpreted them as indications of falsified accounts of nonexistent defensive gun uses.

Creationists also knock down false straw-man versions of evolutionary theory that are alleged to be incompatible with evidence of large-scale floods. Similarly, rare-defensive gun use advocates falsely assert that the frequent-defensive gun use proposition is somehow incompatible with NCVS estimates of total crime or estimates of emergency room treatments of gunshot wounds, when in fact there is nothing at all inconsistent about the proposition and the evidence.¹³⁶ Speculating your way around evidence that undercuts your beliefs, rather than developing technically better empirical evidence, is no more likely to improve our understanding of defensive gun use than it has proven in explaining the origin of species.

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Discouraging the Development of Better Defensive Gun Use Estimates

Perhaps the most transparently political of all responses to large defensive gun use estimates was that of Jens Ludwig, who frankly admitted that, in his opinion, "far less attention should be devoted to estimating defensive gun use in the future."¹³⁷ Ludwig argued that further estimates should be regarded as meaningless as long as anyone could speculate about possible methodological flaws in the surveys, or as long as there was any uncertainty about the moral quality of any of the alleged defensive acts. Ludwig, however, had nothing whatever to say about the uncertainty surrounding the nature of incidents defined as "gun crimes" in the NCVS and did not feel compelled to call for a moratorium on future survey estimates of the frequency of gun crimes.

Faced with overwhelming survey support for the idea that defensive gun uses are common, advocates of the rare-defensive gun use theory have belatedly adopted the view that all the survey evidence must be dismissed because surveys simply cannot yield useful information about how often defensive gun uses occur (unless, it would seem, they indicate, as the NCVS supposedly does, that defensive gun uses are rare). Faced with defeat on the field of survey evidence, they belatedly developed a radical skepticism toward all survey estimates. Cook, for example, insisted that "surveys are a decidedly flawed method for learning about the frequency with which innocent victims of crime use a gun to defend themselves."¹³⁸

Cook and Ludwig, however, were not satisfied with dismissing all previous survey evidence. They also moved to forestall policy use of any future survey evidence by asserting that "even if we could develop a reliable estimate of [defensive gun use] frequency, it would only be of marginal relevance to the ongoing debate over" gun control.¹³⁹ Thus, for purposes of public policy, Cook and Ludwig can have no compelling interest in improving defensive gun use estimates, since they would not be willing to give any significant weight to even the most reliable estimates. In light of this attitude, it is hard to understand the motives behind their suggestions for "improving" estimates of DGU prevalence, published just two years later.¹⁴⁰

Philip Cook and David Hemenway both served with me on the advisory board for the NSPOF, advising on the development of its design. After the

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NSPOF results almost exactly confirmed the NSDS results, their response was to assert that surveys inevitably overstate defensive gun use frequency.¹¹¹ This appears to be a belated revelation to Cook and Hemenway. In repeated and prolonged meetings of the advisory committee in 1994, during which the members discussed at length the long series of questions asking about DGUs, neither Cook nor Hemenway even once shared with the board their remarkable theory that all that effort was for naught, and that surveys could not generate even approximately accurate estimates of DGU frequency.

Since surveys are the only feasible way we have of measuring the frequency of defensive gun uses, Cook, Ludwig, and Hemenway have transformed their rare-defensive gun use theory into a nonfalsifiable proposition, placing it safely beyond empirical test. Faced with estimates, which he himself helped develop, that radically contradicted the rare-defensive gun use theory, Cook refused to accept the verdict of the evidence. Instead, he and his colleagues have devoted pages of one-sided speculation to explanations of how suspected flaws in their and other surveys might have led to exaggerated defensive gun use estimates.¹¹²

And, on the off-chance that someone might eventually develop defensive gun use estimates that would be hard to speculate around, Cook, Ludwig, and Hemenway still have a fallback position. They declare that even perfectly accurate defensive gun use estimates would be meaningless and useless, because: (1) we cannot be certain in all cases that reported defensive gun uses were morally correct, (2) it is possible that victims who used guns would have been even better off using other strategies, and (3) it is possible that in some cases there were other means of avoiding trouble altogether, which gun possession and use discourage.¹¹³

These arguments were based on still more speculation, and ignored certain crucial facts. First, although some reported defensive gun uses indeed are morally ambiguous, in the vast majority of reported cases there is little doubt about who is victim and who is aggressor, and thus little doubt about the moral role of the person wielding the gun. Few reported defensive gun uses occur in connection with "mutual combat" fights in which the combatants are both aggressor and victim. Instead, the vast majority are linked with burglaries, robberies, and sexual assaults, crimes where there is rarely any honest confusion about which party is the criminal and which the victim.¹¹⁴ Thus, although it is easy enough to imagine many kinds of



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borderline cases and to cite isolated examples, there is no empirical support for the belief that they are common among the cases reported in defensive gun use surveys. In any case, if one wanted, in a policy analysis, to "count" only the morally clear defensive gun uses as a social benefit, one would be free to do so. Completely ignoring unambiguously legitimate cases merely because of the existence of ambiguous cases is irresponsible.

Second, while it is possible that victims using guns defensively *might* have been better off using other self-protective strategies, empirical evidence indicates that, on average, they are not. Among the victim protective strategies on which we have data, defensive gun use is clearly the most effective one, from the standpoint of minimizing either injury or property loss.¹¹⁵ Leaving aside one-sided speculations about possible deficiencies in the relevant research, we *do* have a fair idea, within the usual limits of multivariate nonexperimental research, what would have happened in defensive gun use cases, considered collectively, had the victims not used guns: a larger share of the victims would have been hurt or lost property. The alternatives that we know about, including nonresistance, are worse than victim use of a gun.

Third, there is no evidence that gun possession or use discourages people from avoiding trouble, or that people treat guns as a substitute for other crime prevention/avoidance strategies, rather than as a supplement. And if "avoiding trouble" includes people restricting their life choices and daily movements so as to make themselves into prisoners in their own homes, one must wonder what cultural values underlie an assumption that avoidance is always the socially preferable alternative. It is hardly self-evident that society is improved by encouraging the law-abiding to abandon valued activities and associations in order to avoid places and times where criminals might be present.

The Relative Frequency of Defensive and Offensive Uses of Guns

McDowall et al. raised the issue of comparing defensive gun use frequency with gun crime frequency, agreeing with Kleck and Gertz that the size of the defensive gun use estimates becomes meaningful only in comparison with

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another standard, such as the number of criminal uses of guns.¹⁴⁶ The authors then noted that the NCVS estimate of defensive gun uses was much smaller than its estimate of gun offenses, but did not regard it as essential to mention that many of the events they described as “gun offenses” did not, as far as we know, involve a gun being used in an attack or threat by the offender. They were “gun offenses” only in the sense that the offender (according to the victim) possessed a gun at the time of the incident—but did not necessarily use it in any sense. NCVS “gun offenses” are not definitionally comparable with defensive gun uses, since the latter events must involve a gun being *used* in a threat or attack to qualify as a defensive gun use, while for 36.6 to 83.4 percent of NCVS “gun offenses” it is unclear whether there was either an attack or a threat with a gun.¹⁴⁷

There is also a problem affecting all surveys that contributes to a net undercount of both defensive and criminal gun uses. It has often been recognized that criminals will be among the persons least likely to be interviewed in general population surveys, because of their low income, high mobility, time spent incarcerated, and reluctance to be interviewed even if successfully contacted.¹⁴⁸ Since it is criminals who are in most frequent contact with other criminals, it is they who are potentially most at risk of victimization. Relative to their share of the population, criminals may claim a disproportionate share of both defensive gun uses and gun crime victimizations. Therefore, victimization and gun use surveys share a sampling bias that contributes to underestimating both criminal and defensive uses of guns.

McDowall and his colleagues argued that estimates of defensive and offensive uses of guns should be derived from the same survey, reasoning that this would make them more comparable.¹⁴⁹ However, even under the best circumstances it is unlikely that any one survey can do a good job of estimating both gun crimes and defensive gun uses. The NCVS has distinct advantages for estimating the frequency of gun crime, including the large samples made possible by a large investment of federal dollars, and the high level of cooperation by prospective respondents made possible by the Census Bureau conducting the interviews. On the other hand, federal sponsorship and conduct of the interviews, in the context of nonanonymous interviews, probably create insurmountable obstacles to producing reasonable estimates of defensive gun use frequency. Consequently, the best currently feasible strategy is to compare the defensive gun use results of surveys

that optimize conditions for accurate reporting of defensive gun use (such as the gun surveys), with gun crime estimates based on the NCVS.

Regardless of what survey strategies are adopted, it may prove impossible to produce perfectly comparable estimates, whether using a single survey or not, since respondents are not likely to be equally willing to report (1) unlawful things done to them with guns by other people (i.e., gun crime victimizations) and (2) unlawful acts that they themselves had committed (e.g., unlawful possession of a gun, which is involved in most reported defensive gun uses). Thus, defensive gun use experiences are likely to be underreported more than gun crime victimizations.¹⁵⁰

Let us nevertheless consider the most recent large-scale national survey that directly asked about both defensive gun use and gun crime victimization. In the 1994 NSPOF, 38 respondents reported a defensive gun use in the past year against a human, not in connection with military, security, or law enforcement work.¹⁵¹ Respondents were also asked: "During the past twelve months—that is, since last November—has anyone robbed, tried to attack, or attacked you?" and those who replied "yes" were then asked: "Did the perpetrator use a gun in any of these incidents?"¹⁵² Note that there were no further follow-up questions to disqualify any of these cases as gun crimes, so the survey will yield a relatively generous estimate of gun crime. Nevertheless, just nine respondents reported gun crime victimizations in the previous year.¹⁵³

Thus, using the same-survey strategy, defensive gun uses would appear to be about four times more common than gun crimes, supporting (perhaps coincidentally) the conclusion that Kleck and Gertz drew using their methods.¹⁵⁴ It is not being argued that this strategy is in fact the optimum way to go about this task. Rather, the data merely serve to show that one could arrive at substantially the same conclusion about relative frequency of offensive and defensive uses even if one adopted the dubious same-survey strategy, as long as the procedure is confined to surveys that directly ask about both gun crime victimizations and defensive gun uses. The NSPOF results for gun crimes are nowhere to be found in reports by Cook and Ludwig, not even in a chapter titled "Gun Uses and Misuses."¹⁵⁵ Perhaps same-survey comparisons are valid only if they indicate that gun crimes are more common than defensive gun uses, as is the case when one uses the NCVS.

A few other surveys, besides the NCVS or NSPOF, also seem to provide

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the data for generating estimates of criminal gun use against the respondents, as well as defensive gun use estimates. Unfortunately, the questions supposedly addressing gun crime victimization actually make no distinction between offensive and defensive uses of guns. Instead, they merely ask ambiguous questions like that posed in a December 1993 Gallup poll: "Not including military combat, has a gun ever been used to threaten you in a robbery, mugging or some other situation?"¹⁵⁶ The obvious problem is that a yes answer would be just as accurate for a criminal who had been on the receiving end of a defensive gun use in a robbery or other crime that they had attempted as it would be for a victim of a gun robbery or other gun crime. Thus, these surveys' supposed estimates of "gun crime" actually reflect an inseparable mixture of defensive gun use and gun crime victimization experiences.

Returning to a sounder basis for drawing conclusions, the best estimate of annual defensive gun use frequency, from the NSDS, is 2.55 million in 1993 (table 6.2), while the best, albeit somewhat ambiguous, estimate of gun crime frequency is that of the NCVS, which indicates that in 1992 about 554,000 crimes of all types occurred in which offenders used guns to threaten or attack victims.¹⁵⁷ Thus, defensive uses are about five times more common than criminal uses.

Conclusions

The effect of all the one-sided assessment of methods, suppression of unsupported evidence, selective reviews of prior research, fallacious logic, and arbitrary interpretation of evidence has been a degradation of scientific standards in the study of guns and violence. Perhaps the most destructive effect of all is the legitimization of one-sided methodological speculation as the equal, and even the superior, of empirical evidence as a basis for drawing conclusions from research. In considering how widespread acceptance of this detrimental practice might be, it should be borne in mind that acceptance, or at least tolerance, of these practices is not confined to a handful of scholars. Every instance of published nonsense requires the collaboration of journal referees who recommend that it be published and of editors who can persuade themselves that the publication of one-sided



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speculation can somehow make a constructive contribution to scholarly debate. And for every irresponsible scholar who puts one-sided speculations down on paper, there no doubt are others who privately indulge in the same vice. Further, the fact that such material is published sends a message to much larger audiences of scholars that these critical practices are acceptable—thereby legitimating debased intellectual goods and depressing the implicit standards of scholarly quality sufficient to gain publication.

The most obviously slanted critique of large defensive gun use estimates published to date was probably that of Hemenway.¹⁵⁸ Perhaps the only thing more shocking than Hemenway's ideological diatribe was the fact that a respectable professional journal, the *Journal of Criminal Law and Criminology*, decided to publish it. Its criminology editor, John Hagan, attributed his decision to publish the paper to the fact that two or three outside reviewers recommended publication. But all that it takes for an editor to get such recommendations is to select reviewers with strong published views consistent with the author's thesis who are willing to overlook its tactics, one-sidedness, speculative character, and lack of supporting evidence.¹⁵⁹

After Kleck and Gertz supplied Hagan with a long series of documented instances of deceptive claims, logical fallacies, red herrings, inaccuracies, and even potentially libellous insinuations in the Hemenway paper, Hagan did not dispute their claims. Instead, he insisted that publishing Hemenway's paper would somehow "contribute to the debate."¹⁶⁰ To suggest that publishing a long series of falsehoods, red herrings, irrelevancies, libelous insinuations, and personal ideology disguised as scholarly criticism somehow "contributes" to the scholarly debate over gun use is nonsensical. Intellectually debased argumentation only muddies the waters and makes the already difficult task of assessing the evidence even harder.

All of the challenges to large defensive gun use estimates have been thoroughly rebutted. From a crudely political standpoint, however, this is inconsequential. The political functions of a piece like Hemenway's were served the instant an editor decided to publish it. Even if a "critique" is utterly devoid of valid content, and each of its points are thoroughly refuted in the pages of the publishing journal, once the piece appears in print in a journal, propagandists can cite the publication as evidence that large defensive gun use estimates have been "discredited."

And indeed, this is precisely how the Hemenway piece has been cited,

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even before it was published. In a letter to the *Journal of the American Medical Association*, three public health gun control advocates stated that "the reasons that this survey [the NSDS] is incapable of yielding an accurate estimate of defensive gun use are described at length in the Hemenway article."¹⁶¹ Apparently a series of unsupported and one-sided speculations was a sound enough basis for these individuals to reject the findings of at least fifteen (at that time) large-scale, professionally conducted surveys.

We can be confident that propagandists will in future cite these one-sided speculations as authoritative proof that large defensive gun use estimates have been "discredited," while procontrol academics who regard themselves as moderates will conclude that while Hemenway and others like him may have overstated a few points, they had nevertheless somehow "cast doubt" on the estimates or "raised serious questions" about them.

The critiques can be cited by gun control advocates, procontrol scholars, and reporters alike in good conscience, as part of a "balanced" presentation of the issue. Hemenway's outrageous and unsupported speculations will be cited in scholarly sources alongside the NSDS estimates, implicitly giving equal weight to careful, empirically based estimates and misleading, evidence-free speculations.¹⁶² The fact that the balance is completely spurious, and that only one side of the debate can present credible supportive empirical evidence, is politically irrelevant. Since it is highly unlikely that either reporters or the rest of the audience for propaganda will bother to read a rebuttal, the complete lack of any intellectual merit to the defensive gun use critiques will not be evident, and thus will not in any way reduce their political utility.

In contrast to advocacy scholarship and political warfare, the effectiveness of science depends on upholding the primacy of empirical evidence over speculation. Scientists are not allowed to ignore or discount evidence merely because they are imaginative enough to conjure up possible flaws in the evidence, for the obvious reason that this can easily be done with even the soundest evidence. Since flawed evidence is the only kind we have, if one rejects flawed evidence, one rejects all evidence. And in the absence of evidence, people tend to fall back on their biases and taken-for-granted preconceptions (euphemistically referred to as "common sense") in drawing conclusions. Once scholars tolerate one-sided speculation, bad ideas are never abandoned because advocates can speculate their way around any evidence.

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The hypothesis that many Americans use guns for self-protection each year has been repeatedly subjected to empirical test, using the only feasible method for doing so, surveys of representative samples of the population. The results of nineteen consecutive surveys unanimously indicate that each year huge numbers of Americans (700,000 or more) use guns for self-protection. Further, the more technically sound the surveys, the higher the defensive gun use estimates are (compare the NSDS and NSPOF with earlier surveys). The entire body of evidence cannot be rejected based on the speculation that all surveys share biases that, on net, cause an overestimation of defensive gun use frequency because, ignoring fallacious reasoning, there is no empirical evidence to support this novel theory. At this point, it is fair to say that no intellectually serious challenge has been mounted to the case for defensive gun use being very frequent.

One of the main reasons that elements of the American intelligentsia are reluctant to accept large estimates of defensive gun use frequency, one suspects, is that to do so would put them in agreement with the NRA, an organization so thoroughly demonized within these quarters as to make agreeing with them intolerable. Intellectuals who would normally be perfectly aware of the illegitimacy of using ad hominem standards for judging the validity of an assertion will nonetheless recoil from accepting any conclusion that would put them in agreement with the "evil empire."

Further, if Don Kates is right (chapter 1), some of the resistance to high defensive gun use resistance may be due to a visceral dislike for forceful self-protection actions of all sorts, grounded in a moral repugnance for self-defense. For some, like David McDowall and Colin Loftin, it is self-evident that "collective security requires the disarming and demobilizing of individual capabilities" for self-defense. Some people regard the acceptance of forceful behavior for defensive purposes as moral compromise and a weakening of opposition to violence. In their view, claims of self-defensive use of force are mere rationalizations of what was actually aggressive behavior. For example, referring to defensive gun use surveys, Reiss and Roth insisted that "some of what respondents designate as their own self-defense would be construed as aggression by others," the "others" apparently including Reiss and Roth.¹⁶³ Under this view, to see any value in defensive gun use would amount to valuing aggression, since very little of the supposedly "defensive" gun use really was defensive.

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Discussion of the merits of DGU estimates in the scholarly community will continue to amount to little more than junk science and advocacy scholarship until a consensus is reached among researchers that speculation, while useful in generating testable hypotheses, should carry no weight at all in assessing evidence and arriving at conclusions. Instead, conclusions, however tentative, should be based solely on the best available empirical evidence. The best survey on defensive gun use frequency indicates 2.55 million defensive gun uses a year in the United States (table 6.2), and this estimate has been repeatedly confirmed by all surveys of comparable technical merit. Until a methodologically better national survey yields a substantially different estimate, honest scholars will have to continue to accept, however tentatively, that millions of Americans use guns for self-protection each year.

Advocates of the rare-defensive gun use theory, however, have shown no interest in developing methodologically better surveys on defensive gun use. Quite the contrary, as soon as it became evident to them that good-quality surveys would continue to yield high defensive gun use estimates, they invented the theory that all surveys are inherently biased to overstate defensive gun use frequency, thus making it impossible, in their minds, to produce valid defensive gun use estimates using surveys.¹⁶⁴ This nihilistic position effectively renders their rare-DGU theory evidence-proof. The only kind of further research on the topic that these critics have called for is transparently one-sided research designed to detect only false positives, and thus to justify revising defensive gun use estimates downward.¹⁶⁵

Policy Implications of Large Defensive Gun Use Estimates

It is ironic, in light of all the impassioned scholarly dispute, that large defensive gun use estimates pose no threat whatsoever to the moderate gun controls, such as background checks of prospective gun buyers, that most Americans support. These measures would not deny guns to any significant number of noncriminals, and thus would not prevent defensive gun use among the law-abiding. People who sincerely support only moderate controls, but oppose gun prohibition, should have no political concerns about large defensive gun use estimates.

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Such estimates do, on the other hand, constitute a very serious obstacle to promoting gun prohibition, which would deny guns to criminals and non-criminals alike, and thus would reduce whatever benefits defensive gun use may yield. Therefore, in light of the absence of any intellectually serious basis for discounting large defensive gun use estimates, one plausible explanation of why some scholars cling to the rare-defensive gun use theory in the face of overwhelming contrary evidence is that they favor a disarmed populace and accurately perceive high defensive gun use estimates as a significant political obstacle to achieving national gun prohibition.¹⁶⁶

Appendix—Adjusting Earlier Estimates of Defensive Gun Use

The results of earlier gun use surveys were adjusted to make them more comparable with one another and with the National Self-Defense Survey. The basic idea was to estimate the annual, national estimate of defensive gun uses (DGUs) which each survey would have yielded if it had resembled the NSDS, i.e., if it was a national survey of noninstitutionalized adults (age 18+) that covered uses involving any gun type, excluded uses against animals or uses connected with military, police, or security guard work, had a one-year recall period, and asked the DGU question of all respondents (Rs), not just gun or handgun owners. There was no attempt to adjust for differences in crime or gun ownership levels in different years or locales. For all estimates it was conservatively assumed that there was only one DGU per DGU-involved person or household.

Adjustments

A. Adjustment **A** was applied to surveys inquiring only about uses of handguns, in order to produce an estimate pertaining to all gun types. The NSDS indicated that 79.7 percent of DGUs involved handguns, so the adjustment consists of multiplying a handgun-only estimate by 1.2547 (1/0.797).¹⁶⁷

B. Adjustment **B** was applied to surveys inquiring about an indefinite period of time (“have you ever . . .”), in order to produce an estimate pertaining to a one-year recall period. The Field poll indicated that the same

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survey yielded a 1.4 percent prevalence figure for a one-year recall period and an 8.6 percent figure for the unlimited period (see table 6.1, note a), so the adjustment consists of multiplying an "ever used" estimate by 0.16279 (1.4 percent/8.6 percent).

C. Adjustment C was applied to surveys inquiring about a five-year recall period, in order to produce an estimate pertaining to a one-year recall period. The NSDS yielded the following ratios of one-year prevalence over five-year prevalence:¹⁶³

C1. All guns, person-based: $1.326/3.315=0.40000$

C2. All guns, household-based: $1.587/3.898=0.40713$

C3. Handguns, household-based: $1.105/2.974=0.37155$

D. Adjustment D was applied to surveys that failed to exclude uses of guns against animals, in order to produce an estimate pertaining only to uses against humans. The NSDS indicated that of 244 Rs initially reporting DGUs, 22 had used guns only against animals, so the adjustment consists of multiplying a humans-plus-animals estimate by 0.90984 (222/244).

E. Adjustment E was applied to surveys that asked the DGU question only of Rs who reported personally owning a gun or handgun, in order to produce an estimate pertaining to the entire population and thus reflecting uses among those who do not report current ownership of a gun. The NSDS indicated that only 59.5 percent of Rs reporting DGUs reported current personal ownership of a gun (p. 187), so the adjustment consists of multiplying a gun-owners-only estimate by 1.68067 (1/0.595).

F. Adjustment F was applied to surveys that asked the DGU question only of Rs who lived in households reporting a gun, in order to produce an estimate pertaining the entire population and thus reflecting uses among those who do not live in a household reporting current ownership of a gun. The NSDS indicated that only 79.0 percent of Rs reporting DGUs reported current household ownership of a gun (p. 187), so the adjustment consists of multiplying a gun-owners-only estimate by 1.26582 (1/0.790).

G. Adjustment G was applied to surveys that inquired only about DGUs that occurred outside the R's home, in order to produce an estimate pertaining to all DGUs, regardless of location. The NSDS indicated that

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62.7 percent of DGUs occurred outside the R's home (p. 185), so the adjustment consists of multiplying an outside-the-home-only estimate by 1.59490 (1/0.627).

Population Bases Used

Estimated resident population, age 18 or older, as of March, 1993: 190,673,523. Estimated U.S. households as of March, 1993: 96,391,000.¹⁶⁹

Adjustments Applied

Survey	Population Based Used	Adjustments Applied	Other Information Used
Field	Persons	A,D	Used the 1.4% past-year figure
Bordua	Persons	B,D	
DMIa	Households	B,D	
DMIb	Households	B	
Hart	Households	A,C3	
Ohio	Persons	A,B,D	26.4% of U.S. households had handguns in 1993 (average of 5 national surveys)
Mauser	Households	C2	46% of households reported a gun; 47% of these reported a handgun
Gallup 91	Persons	A,B,D	31% of Rs personally owned gun
Gallup 93	Persons	B,D,E	
L.A. Times	Persons	B,D,G	Used "1% of Rs" figure
Tarrance	Persons	C1	

Computational Procedures

Typically, the prevalence figure reported for the survey (e.g., the proportion of adults who had used a gun for protection) was multiplied times the appropriate population base (e.g., number of U.S. resident adults) and then times

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each of the necessary adjustment factors. In cases where the prevalence figure applied only to gun owners (or some subset thereof) the number of gun owners had to be computed first, using the gun owner prevalence figures generated in the same survey, or, in one case, figures generated in other national surveys conducted in the same year.

Illustrative Example

The 1993 Gallup poll estimate required considerable adjusting, so it serves as a useful example. In that survey, among persons reporting that they personally owned a gun (who constituted 31 percent of the entire sample), 11 percent reported that they had ever used a gun for protection. The survey did not exclude uses against animals. The estimate was therefore computed as follows:

$$190,677,523 \times 0.31 \times 0.11 \times 0.16273 \times 0.90984 \times 1.68067 = 1,618,428$$

	B	D	E
Population	% of pop.	% of	1 year
(# Adults)	that owns	owners	vs. "ever"
	guns	w. DGU	
			Humans
			only
			Entire
			pop. vs.
			only gun
			owners
			Est.
			DGUs

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