

cies in his analyses and standards of evaluation and a willingness to use questionable or misleading statistics to support his position.

First, Kleck routinely dismisses National Crime Victimization Survey (NCVS) results on defensive gun use (DGU), claiming, incorrectly, that they capture less than (a nonrandom) 4% of all such events. Yet he uses the NCVS data concerning self-defense gun use, without caveat, to claim that guns are an effective method of self-defense.

Second, Kleck claims that DGU is far more common than offensive gun use. He obtains this result by inappropriately comparing the large overestimates of self-defense gun use from private surveys² with the estimates of offensive gun use from the NCVS. For his self-defense estimates, Kleck argues that the NCVS misses many crimes, yet when estimating offensive gun use he assumes that the NCVS captures all offensive gun uses (the NCVS misses many offensive gun uses in domestic violence and elsewhere). More methodologically correct would be to compare reports of both types of gun uses from the same survey. When this is done, as Kleck knows, whether the surveys are public (NCVS) or private,³ respondents report far fewer DGUs by them than offensive gun uses against them.

Third, Kleck gives a misleading impression about case-control studies of firearms and suicide. There have been 7 case-control studies in the United States and all 7 found a significant and substantial association between a gun in the home and suicide.⁴ Kleck has written that "One of the least productive lines of inquiry in the gun control debate has been to compare the United States with other nations,"⁵ yet he cites, with no caveat, a study that found no significant ($P > .05$) increase in suicide risk from gun ownership. As it turns out, this was a small study from New Zealand, where extensive background checks for gun ownership are common, gun storage requirements are strict, and there are virtually no handguns. The study had only 20 cases of gun suicide, but even so, in homes with guns, the odds of suicide were 40% greater than in homes without guns ($P < .10$).

Kleck's arguments are often inconsistent and misleading.^{2,6} His recent article is no exception.

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1. Kleck G. What are the risks and benefits of keeping a gun in the home? *JAMA*. 1998;280:473-475.
2. Cook PJ, Ludwig J, Hemenway D. The gun debate's new mythical number: how many defensive uses per year? *J Policy Analysis Manage*. 1997;16:463-469.
3. Hemenway D, Azrael DR. Gun use in the United States: results of a national survey. Report to the National Institute of Justice; Washington, DC; 1997.
4. Miller M, Hemenway D. The relationship between firearms and suicide: a review of the literature. *Aggression Violent Behav*. 1999;4:59-75.
5. Kleck G. *Point Blank*. New York, NY: Aldine de Gruyter; 1991:188.
6. McDowall D, Lizotte AJ, Wiersma B. General deterrence through civilian ownership: an evaluation of the quasi-experimental evidence. *Criminology*. 1991;229:541-559.
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In Reply: I have rebutted Dr Hemenway's claims about DGU elsewhere.¹ It suffices here to note what Hemenway cannot present—technically superior evidence indicating that DGU is as rare as NCVS data indicate it to be. Taking Hemenway's points in order: First, there is no inconsistency in regarding NCVS "estimates" of DGU frequency as inaccurate while using NCVS data to assess the effectiveness of DGU. I take the former position because NCVS-based estimates of DGU frequency have been strongly contradicted by every other source of information,¹⁻³ while I use NCVS data on DGU effectiveness because they are the best available and because there is no evidence indicating that the DGU reports captured by the NCVS are unrepresentative regarding the effectiveness of DGU.

Second, in comparing the number of defensive uses of guns with the number of criminal uses, it is not better to "compare reports of both types of guns uses from the same survey," given that no single survey has provided valid estimates of both parameters. Now that Cook has conceded that NCVS-derived estimates of DGU frequency are too low,³ Hemenway appears to be the last scholar in this field to believe they are accurate. Conversely, no private DGU survey has had the sample size and detailed questioning concerning crime incidents needed to estimate criminal gun uses. Thus, the best course is to do what I have done—use the best available estimates of each parameter, even if derived from separate surveys.

Third, readers may judge for themselves Hemenway's accuracy in describing the New Zealand study as "a small study." It is actually the largest case-control study of guns and suicide ever done, with 499 cases and 1028 controls.^{3(p279-284)}⁴ In similar fashion, Hemenway alludes to "7 case-control studies in the United States," supposedly showing a gun effect without mentioning that 5 of these were merely different analyses of the same sample of 67 or fewer adolescent suicides in Pennsylvania.^{3(p279-284)}

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1. Kleck G, Gertz M. The illegitimacy of one-sided speculation: getting the defensive gun use estimate down. *J Criminal Law Criminology*. 1997;87:1446-1461.
2. Kleck G, Gertz M. Armed resistance to crime: the prevalence and nature of self-defense with a gun. *J Criminal Law Criminology*. 1995;86:150-187.
3. Kleck G. *Targeting Guns: Firearms and Their Control*. New York, NY: Aldine de Gruyter; 1997:149-162.
4. Beautrais AL, Joyce PR, Mulder RT. Access to firearms and the risk of suicide: a case-control study. *Aust N Z J Psychiatry*. 1996;30:741-748.

CORRECTION

Incorrect Affiliation: In the reply letter entitled "Treatment of Attention-Deficit/Hyperactivity Disorder" published in the April 28, 1999, issue of THE JOURNAL (1999; 281:1491), the affiliation for Larry S. Goldman, MD, was incorrect. Dr Goldman's affiliation should have been the University of Chicago, Chicago, Ill.