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ELUSIVE FACTS ABOUT GUN VIOLENCE: WHERE GOOD SURVEYS GO BAD

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Abstract:

The evidence base for the study of guns and violence begins with data on such fundamental issues as the number and distribution of guns, the number of people shot each year in criminal assaults, and the frequency of gun use in self-defense. It seems that these simple descriptive statistics should be readily available, and in fact the rhetoric of the debate over gun control in the United States routinely includes reference to 300 million guns, or 100,000 people who are shot each year, or 2.5 million defensive gun uses. But it turns out that such statistics should be viewed with considerable skepticism. Developing reliable estimates of basic facts in this arena is surprisingly difficult, even with the best of intentions. Even surveys that meet the highest standards of current practice may produce heavily biased estimates. The results discussed here should encourage skepticism and engender what might be called "plausibility tests" – common-sense comparisons of the resulting estimates with other sources of information. Too often the review of scientific contributions is like appellate review of a criminal conviction – the court focuses on just the process rather than the outcome. For policy-relevant work it is important to test the conclusions against what else we know about the reality of the situation.

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ELUSIVE FACTS ABOUT GUNS VIOLENCE: WHERE GOOD SURVEYS GO BAD

Introduction

The evidence base for the study of guns and violence begins with data on such fundamental issues as the number and distribution of guns, the number of people shot each year in criminal assaults, and the frequency of gun use in self-defense. It seems that these simple descriptive statistics should be readily available, and in fact the rhetoric of the debate over gun control in the United States routinely includes reference to 300 million guns, or 100,000 people who are shot each year, or 2.5 million defensive gun uses. But it turns out that such statistics should be viewed with considerable skepticism. Developing reliable estimates of basic facts in this arena is surprisingly difficult, even with the best of intentions.

There exist administrative data compiled by government agencies on each of these topics, but those data are incomplete, difficult to access, error-prone, or all of these. As a result, analysts have made extensive use of population surveys, which in principle can overcome the limitations of administrative data. For example, if you want to find out how many guns are in private hands, why not ask a representative sample of U.S. households whether there is a gun on the premises, and if so, how many? But it turns out even state-of-the-art survey methods can generate heavily biased estimates. The existence and nature of these biases has been a matter of heated debate in one of these areas, defensive gun uses, because of its direct relevance to advocates' claims concerning the value of widespread gun ownership. But the word "bias" in this context does not refer to political bias, but rather a predictable error characteristic of a particular estimation method. The surprise is that the survey methods used to generate such error-prone estimates are not obviously deficient, but rather widely accepted in social science. Hence there are methodological lessons that go well beyond the arena of gun violence. Here we recount three examples in some detail. The point is not that surveys are useless, but rather that their accuracy should not be taken by faith.

Accepted practice for scholars who are assessing the accuracy of a survey-based estimate of some population statistic is to review the methods used to generate the data. Indications that the survey-based statistics may be off the mark include a sample frame that omits some of the population (e.g., those who lack a land-line phone), a low response rate, poorly written questions or a response mode (e.g., telephone, on line, in person) that is not reliable given the subject matter. If given the survey method it is reasonable to consider the respondents as representative of the relevant population (possibly after population weights are applied, and subject to normal sampling error), and their responses to the survey items are in some sense credible, then the resulting estimate is to be taken seriously. (Of course it helps if the agency that conducted the survey has a good reputation.) Less common is to include in the assessment a comparison with other estimates of the same population statistic, or of related statistics. An assessment based solely on the survey procedures is akin to the approach of the appellate courts in reviewing a criminal case – the guilty finding is tested not by looking at new evidence in the case, but rather by assessing the process used to arrive at that verdict.

The problem is this: Sometimes a survey is well designed, but the resulting estimates are demonstrably wrong, and by a wide margin. For that reason, we believe that if getting a reasonably accurate estimate is important (and if it is not, why bother?), then the analyst should ask and attempt to answer the following prosaic question: "Given everything we know, both from the survey in question and other sources, is this particular estimate in the right ballpark?" We might call this a "plausibility test." It may seem like common sense, but a quick scan of reports of survey results will demonstrate that a discussion of procedure is far more common than a discussion of plausibility.

As an example, consider a survey-based estimate of the amount that Americans drink. High-quality surveys of the United States population that ask respondents whether they drink and if so, how much, can be used to generate estimates of the national average per capita consumption of alcohol. In that case there is an obvious check on the accuracy of the result – tax-paid sales of alcoholic beverages. (In recent year those sales have amounted to about 2.3 gallons of ethanol per individual age 15 and over.) As it turns out, survey-based estimates are typically 40-60% of tax-paid sales, suggesting either that a great deal of beer and whiskey is poured down the sink every year, or that respondents under-report their true consumption. (Another possibility is that the heaviest drinkers tend to be non-respondents or left out of the sample frame entirely.) In this case the availability of reasonably accurate administrative data (tax-paid sales) provides a valuable external standard by which to assess and correct survey-based estimates. That situation is more the exception than the rule, but in other cases there may be other approaches to "plausibility testing."

In what follows we consider three examples from the study of gun ownership and use (or misuse). The first example is gun ownership – the household prevalence of guns, and the number of guns in private hands. The second is the number of individuals who are shot and wounded in assault circumstances. And the third is the number of instances in which a private individual uses a gun to defend against crime. In each case the apparent bias in estimates based on population surveys is remarkably large.

How many guns in private hands?

Key statistics in the debate over the feasibility of gun control are the household prevalence of private gun ownership, and, closely related, the number of guns in private hands. Administrative data on the prevalence of household gun ownership is almost entirely lacking. Data of that sort could in principle be generated through licensing or gun registration. But at the federal level registration is only required for owners of machine guns, hand grenades, and sawed-off shotguns. A few states require licensing or registration, but compliance with those requirements is likely to be far less than 100 percent.

Sample surveys appear to offer a good alternative to administrative data. For example, the General Social Survey (GSS), conducted by the National Opinion Research Center, has long included questions on gun ownership. In 1999 it estimated that just 36 percent of American households owned at least one firearm, down from nearly 50 percent in 1980 (Smith 2000, p. 55). The most recent estimate (February 2014) from the reputable Pew Research Center survey finds that 37% of households had a least one firearm.

As it turns out, however, there appears to be systematic response error (bias) in "household prevalence" estimates of this sort. That bias was discovered by comparing the responses of husbands and wives to the same question (Cook and Ludwig 1996). In the GSS sampling procedure, whether the husband or wife is selected as the respondent for a household that is headed by a married couple is determined randomly, so the same percentage should report a gun in the household. In fact husbands are consistently more likely than wives to report a gun, with the difference as high as 10 percentage points in some years (Ludwig, Cook, and Smith 1998). And GSS is not the only survey with this problem. Using the National Survey of Private Ownership of Firearms (NSPOF) data, we found that if husbands' answers were to be believed, the estimated national stock of handguns would be twice as high as if we believed the wives' answers (Cook and Ludwig 1996).

It is tempting to believe that the husbands are more accurate, since they are likely to be the primary owners and users of any guns, and may be better informed and less reluctant to admit to owning a gun in a survey. But it is not necessarily true that the gap is due to wives' underreporting – the husbands may be over reporting – some respondents may want to overstate their gun collection to impress the interviewer. Still, at this point it is reasonable to suppose that responses to the question of individual gun ownership are more accurate than responses to a question about household gun ownership. In the Pew survey cited above, 24% of adults indicated that they personally owned a gun, with men more likely to report ownership than women by a factor of 3 to 1.

Analysis of a two-generation survey in California found that the "household gun reporting gap" appeared when teenagers were asked about guns in the home – the boys were much more likely to say yes than the girls (Cook and Sorenson 2006). In this survey there was enough information to determine that the difference in response was accounted for by the difference in participation in gun sports, suggesting that the response is influenced by whether the respondent has first-hand knowledge of the existence of a household gun collection.

To determine the number of guns in private hands (as opposed to the prevalence of gun ownership) requires that a survey ask how many guns are in the household, and that question has been quite rare. We used the 1994 NSPOF to generate detailed estimates: we found that 25% of adults (most of them men) owned at least one gun, that the average gun-owning adult owned 4.4; multiplying up, we estimated the total number of guns in private hands as 192 million (one-third of which were handguns) (Cook and Ludwig 1996). Does that estimate meet the "plausibility" test? Perhaps. The federal government has kept track of the number of civilian guns manufactured, imported, and exported since 1899. By 1993 (when the NSPOF was fielded) the cumulative total of guns introduced into the US civilian market (manufactures plus imports minus exports) was 223 million. It makes sense that the cumulative total somewhat exceeds the stock in that year (223 vs. 192 million), since guns are durable but not immortal – over the decades, millions have been broken and discarded, or destroyed following confiscation by the police. So the survey-based estimate is not in obvious contradiction to the administrative record.

But before concluding that that estimate of 192 million guns was in the right ballpark for 1993, we should consider the quality of the administrative records used as a benchmark. They necessarily omit counts of off-the-books imports and exports. Most prominent in recent years

has been the illegal (and unrecorded) exports to Mexican drug gangs, but there is nothing new about such exports -- the United States has long been a source of guns smuggled to Canada, the Caribbean, and Central America. It is also true that there are unrecorded imports, such as military weapons brought back from war by American troops (Kleck 1991, App. 1). If the illicit exports exceed the imports, then the result would be that the administrative data exaggerate the net flow of guns into the civilian market.

Thus we have no "bulletproof" way to estimate the number of guns in private hands. The best that can be hoped for is a ballpark estimate. If we accept the Pew estimate that one quarter of adults own at least one gun, that amounts to 60 million individual gun owners age 18 and over. Some adolescents own guns as well, so perhaps the true number is 65 million. Estimating the number of guns per gun owner requires a replication of NSPOF- type questions, which has not been done. If the average number per gun owner is 4, then the total is 260 million; if 5, then we are up to 325 million (which is about equal to the cumulative total between 1899 and 2011). In any event, it should be understood that there are a wide range of plausible estimates.

How many gun injuries from assault?

The number of gunshot victims in assault cases includes those who die and are hence counted in the Vital Statistics program as gun homicides. While not perfect, the Vital Statistics counts are generally presumed to be quite accurate. The challenge, then, is to estimate the number of assault cases where the victim is shot but does not die, since there is no national system for counting such cases.

In principle, survey data of a nationally representative sample of households could provide a comprehensive estimate of nonfatal injuries. The National Crime Victimization Survey (conducted by the Census Bureau on behalf of the US Department of Justice) has asked the relevant questions of a nationally representative sample since 1973, and released annual estimates of the number of gunshot victims in assaults. These estimates turn out to be gross underestimates of the truth, despite the fact that the NCVS is an exceptionally well-crafted survey. One of us (Cook) first became aware that there might be a problem after comparing the estimated nonfatal injury rate with the known rate of fatal gunshot wounds in assault (homicides). The ratio of nonfatal (from NCVS) to fatal was 2:1, implying that fully one in three gunshot victims die. By considering a variety of sources of information on the case-fatality rate in assaults where the victim was shot, a consistent finding emerged –rather than a 1 in 3 death rate among victims of criminal shootings, the actual fatality rate is typically about 1 in 7 (Cook 1985).

Since that demonstration a new source of data has become available and helped confirm the magnitude of the true case-fatality rate. This source taps into the administrative data of a sample of emergency departments called NEISS, which provides (among other things) an estimate of the number of gunshot victims treated in emergency departments and the circumstances in which the injury occurred (assault, suicide, accident). If we divide the number of gun homicides by the total of gun homicides and nonfatal gun assaults treated in the emergency department (as estimated from NEISS data), the case fatality rate in 2011 is 1 in 6 (11,101 homicides and 55,544

nonfatal injuries). That is in line with the 1-in 7 estimate, given the fact that some gunshot cases do not show up at an emergency room.

The bias in NCVS estimates has if anything become more pronounced in recent years. Here is a recent report from the Bureau of Justice Statistics: "In the 5-year aggregate period from 2007-11, a total of 46,000 nonfatal firearm victims were wounded with a firearm and another 58,483 were victims of a firearm homicide (Planty and Truman 2013, p. 11)." The "46,000" is estimated from the NCVS, and implies that over half of all gunshot victims die. The report goes on to note that the estimate of nonfatal gunshot cases from the NEISS is far higher, over 250,000 per year during this period, and then observes "The differences noted between the NCVS and NEISS-AIP firearm injury estimates are due in part to a variety of technical issues….. Therefore, NCVS may miss injuries that involve persons who are homeless, victims who require lengthy stays in a hospital, and offenders who are incarcerated or placed in other institutional settings after the incident." While one might hope for a stronger statement – namely, something to the effect that "The NCVS estimate understates the true number by a factor of 5 or more" -- the careful reader will still get the idea.

The likely reasons for the gross underestimate of nonfatal gunshot victims in NCVS is that they are underrepresented in the sample, and not just for the reasons noted in the quote above. A large percentage of assault victims are drawn from the ranks of youthful men who are difficult to contact because they have no regular address, and in any event may be reluctant to talk to an interviewer. (In these respects there is a good deal of overlap between the shooters and the victims.)

When it comes to estimating the number of nonfatal gunshot assaults, best practice may be to ignore the national survey estimates, and generate estimates instead by using an evidence-based multiplier of the official count of gun homicides. That work-around appears to provide fairly accurate estimates.

Note that the large bias in the NCVS estimates is with respect to a narrow category of victimization (shot during a criminal assault) that is concentrated among a group that may in practice be underrepresented in the survey sample. That would not be the case for estimating victimization rates for such crimes as burglary or auto theft.

How many Defensive Gun Uses?

While guns do enormous damage in crime they also provide some crime victims with the means of escaping serious injury or property loss. The NCVS, despite its limitations (see above), is generally considered the most reliable source of information on predatory crime, since it has been in the field since 1973 and incorporates the best thinking of survey methodologists. From this source it would appear that use of guns in self-defense against criminal predation occurs on the order of 100,000 times per year (Cook, Ludwig, and Hemenway 1997). Of particular interest is the likelihood that a gun will be used in self-defense against a residential intruder. Based on the NCVS data for the mid 1980s, only 3 percent of victims were able to deploy a gun against someone who broke in (or attempted to do so) while they were at home (Cook 1991). Since about 45 percent of all households possessed a gun during that period, it appears that it was

relatively unusual for victims to be able to deploy a gun against intruders even when they have one nearby.

In contrast are the results of several smaller one-time telephone surveys, which provide a basis for asserting that there are millions of defensive gun uses per year (Kleck and Gertz 1995; Cook and Ludwig 1996). The best known estimate in this literature is 2.5 million (Kleck and Gertz 1995). Why do these one-time surveys produce estimates that exceed the NCVS estimate by a factor of 25 or more? One explanation is that the NCVS only asks questions about defensive actions to those who report a victimization attempt, while the phone surveys ask such questions of every respondent. While as a logical matter it seems like it would make little difference, it is quite possible that some NCVS respondents fail to report a defensive gun use (DGU) because they did not think to report to the interviewer the criminal threat that initiated it. In that case the NCVS will include false negatives in its estimate of DGUs. On the other hand, survey questionnaires that ask an open-ended question about self-defense uses greatly expand the scope for false positives (Cook, Ludwig, and Hemenway 1997; Hemenway 1997a,b). Moreover, as the National Research Council's Committee to Improve Research Information and Data on Firearms notes, "fundamental problems in defining what is meant by defensive gun use may be a primary impediment to accurate measurement" (Wellford, Pepper, and Petrie 2005, p. 103; see also McDowall, Loftin, and Presser 2000). When respondents who report a defensive gun use are asked to describe the sequence of events, many of the cases turn out to have involved something other than an immediate threat, and in fact a majority of such self-reported cases were thought to be illegal by a panel of judges (Hemenway, Miller and Azrael 2000).

Perhaps the most compelling challenge to the survey-based claim that there are millions of DGUs per year derives from a comparison with what we know about crime rates. The 2.5 million DGU estimate is well over twice the total number of robberies and assaults committed with a gun, as estimated at that time in the NCVS, which in turn is far more than the number of gun crimes known to the police. Likewise, the number of shootings reported by those who claimed to be defending themselves vastly exceeds any plausible estimate of the total number of gunshot cases in the United States. The Kleck-Gertz survey suggests that the number of DGU respondents who reported shooting their assailant was over 200,000, over twice the number of those killed or treated in emergency departments (Hemenway 2004, p. 67).

This last point engendered a small skirmish in the gun debate that dovetails nicely with our call for plausibility test. Gary Kleck (1997) responded to his critics by asserting that the true number of gunshot cases greatly exceeds the number treated in emergency departments. He claims that many gunshot wounds are not life threatening, and professional treatment would be optional. He speculates that a high percentage of victims are implicated in criminal activity, and that they would hence want to avoid notice of the authorities by going to the hospital for treatment. His argument, then, is that the estimated number of self-defense cases in which the assailant is shot could plausibly exceed the number of all gunshot cases (assault, suicide, accident, self-defense) that resulted in death or treatment in an emergency department. This far-fetched claim was tested directly in a series of jail surveys by John May and David Hemenway (May et al., 2000; May Hemenway and Hall, 2002). These surveys found that a remarkably high percentage of them had been shot at some point in their lives and had the scars to prove it – but more than 90 percent of those who had been shot reported that they had indeed been treated in a hospital.

Thus they would have been included in official statistics on the number of gunshot wounds, and Kleck's assertions are shown to be far off base. The estimate of 2.5 million DGUs, and its subsidiary estimates, do not pass the plausibility test.

The fact is that the estimated number of DGUs from surveys is highly sensitive to the sequence of questions, and in particular whether respondents are only asked about self-defense if they first report a victimization. It also matters that the respondent is given some help in placing events in time (so that when asked about the previous 12 months they do not bring in events that happened before that period). The latter problem, known as "telescoping," is also important in estimating victimization rates. One of the great strengths of the NCVS, as opposed to these one-time surveys, is that its sample retains a household for seven interviews, one every six months. The previous interview is used as a way of providing the respondent with a bracket in placing events in time in answering the question of whether he or she had been victimized in the previous 6 months. Without any framing to prevent telescoping, the number of gun victimizations balloons up. In fact, when the same respondents in the same sort of one-time survey are asked about both DGUs and about victimization by guns, they report many more victimizations than DGUs (Hemenway, Miller, and Azrael 2000).

There are lessons here for survey methodology and for gun policy. The methodological lesson is that survey-based estimates of what appears to be a well-defined construct (use of a gun in self-defense during the last year or last five years) is hyper-sensitive to survey design, so much that estimates may differ by a factor of 25 or more. Another lesson for gun policy is that what some individuals consider to be a legitimate use of a gun in self-defense may be highly problematic in practice.

Unlike in the two previous examples, we do not have a good answer to the question of how many DGUs occur in a given year. Ultimately the question is not well defined– given the way these estimates are used in the gun debate, it appears that the goal is to ascertain the number of legitimate uses of guns, which is to say both legal and in some sense justified by the objective circumstances. The survey respondents' memory and interpretation of what transpired are likely to be a shaky basis (at best) for deciding whether a survey-reported case should "count." That is all the more so given the prevalence of mental illness, substance abuse, dementia – and pranksters -- in any representative sample of the US population.

One might ask at this point whether any survey-based estimate of the incidence of a rare event is likely to be accurate, given the possibility that even a small percentage of false positives can swamp the true events. The answer depends in part on the nature of the event (whether it is likely to engender false reports), and in part on survey design. The NCVS estimate of DGUs greatly reduces the scope for false positives by screening on whether the respondent reports being a victim of crime that included a personal confrontation (as well as bracketing the time period). Additionally, it may be useful to ask respondents who do report a DGU to give the details, which can then be assessed. This approach is analogous to medical screening for rare conditions, where if the initial test is positive then more expensive tests are administered that have high specificity to the condition in question.

The bottom line is that the results from one-time open-ended surveys asking about DGUs tend to produce estimates that entirely implausible. As noted above, scholars have documented this fact and provided some explanation for where the surveys go wrong. Nonetheless, the notion that there are millions of virtuous defensive uses of guns each year continues to be asserted by advocates for deregulating guns, including the National Rifle Association. To borrow from Al Gore, it is a convenient "truth." The marketplace of ideas (and facts) in such a contentious area has the effect of elevating research results that suit predetermined purposes, rather than results based on the best science (Cook 2013).

Thoughts on methodology

Even surveys that meet the highest standards of current practice may produce heavily biased estimates. The results discussed here should encourage skepticism and engender what might be called "plausibility tests" – common-sense comparisons of the resulting estimates with other sources of information. Too often the review of scientific contributions is like appellate review of a criminal conviction – the court focuses on just the process rather than the outcome. For policy-relevant work it is important to test the conclusions against what else we know about the reality of the situation.

The problem is by no means limited to surveys used by criminologists. In his recent paper on reporting the uncertainty in economic statistics, Charles Manski notes that statistical agencies should (but do not in practice) report both sampling error and non-sampling error. In comparison with reporting sampling errors, "It is more challenging for agencies to report nonsampling errors for official statistics. There are many sources of such errors and there has been no consensus about how to measure them. Yet these facts do not justify ignoring nonsampling error. Having agency analytical staffs make good-faith efforts to measure nonsampling error would be more informative than having agencies report official statistics as if they are truths (p. 2)." The examples reported here may provide some inspiration about just how to assess nonsampling error.

The overarching theme highlighted by our examples is the value for criminologists to take the magnitude of social science estimates seriously. Taking them seriously means understanding the reliability of sources. As a leading pediatric cardiologist told Jerome Groopman for his book How Doctors Think, "you not only need to know what people know, but how they know it" (Groopman, 2007, p. 135). If what a criminologist "knows" about a particular population parameter is based on a survey, then it pays to check, not only about the intrinsic quality of the survey, but about the plausibility of the resulting estimate.

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