

# Research Agenda on Guns, Violence, and Gun Control

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*In the last two decades, social scientists began serious, extensive study of firearms issues. This research attention marks a significant change from the academy's lack of interest in earlier decades regarding serious research about guns and gun control. Gary Kleck, Professor of Criminology at Florida State University, has done more than any other scholar to improve and advance firearms-related quantitative social science research. In this essay, Kleck outlines suggested research directions for further quantitative scholarship.*

The intention of this paper is to identify topics related to the links among guns, violence and gun control that have not yet been adequately studied, and to very briefly outline the kinds of research that might add useful knowledge about these topics. In each section a topic is listed, followed by one or more possible research projects. There is no intention to be exhaustive in coverage. Instead, only the more important issues that have not already been adequately addressed in past research are listed. The research projects are limited (with one exception) to those that are reasonably feasible, and are only broadly outlined, rather than described with the kind of detail that would be provided in a research proposal. Supporting citations have been kept to a minimum to save space; interested readers may find relevant information in the pertinent chapters of my book, *Targeting Guns* (Aldine de Gruyter, 1997).

## I. GUNS IN THE HANDS OF CRIMINALS

### A. Distinguishing the Effects of Criminal Gun Ownership Rates from the Effects of Noncriminal Gun Ownership Rates

The best available macro-level evidence indicates that levels of gun ownership in the general population (i.e. criminals and noncriminals combined) have no net effect on rates of crime or violence, including homicide rates (Kleck and Patterson 1993; Kleck 1997, pp. 24a-256), even though evidence on individual crime incidents indicates that when criminals use guns in attacks, the victim is more likely to die (Kleck and McElrath 1991; Kleck 1997, pp. 242-243). One way to tie these and other findings together is to hypothesize that gun ownership among criminals does, on net, have violence-increasing effects, but that these effects are balanced out by violence-reducing effects of gun ownership and defensive use by noncriminals. Yet no research has been done that separately measures criminal gun ownership levels and noncriminal gun ownership levels.

*Research Project:* This research has not already been done largely because there are no indicators currently available that are known to credibly distinguish between criminal and noncriminal gun levels. Thus, persuasive research on this topic may have to await the development of the necessary indicators. Therefore law enforcement agencies should be encouraged to develop indicators of criminal gun possession/carrying levels, which could serve not only scholarly research purposes but also as valuable indicators of whether law enforcement efforts are succeeding in reducing criminal gun possession.

The measure that best combines utility and feasibility is the percent of arrestees found in possession of a gun at the time of arrest. This does not require police to acquire any new information, but rather merely to record on arrest reports something they already know. Thus it is feasible and cheap, as well as useful. When and if many law enforcement agencies begin to routinely record this information it will be possible to separately assess the impact of criminal gun possession on violence rates, and directly estimate the impact of gun laws and enforcement policies on criminal gun possession.

### B. The Impact of Gun Theft on Crime Rates

The primary route by which guns get from noncriminals to criminals is via theft, mostly residential burglary (Wright and Rossi 1986). Thus, it is reasonable to hypothesize that higher gun theft rates would

increase gun possession among criminals, and thereby increase crime rates. No research has ever been done on this, even though the FBI gathers data on the dollar value of guns reported stolen to law enforcement agencies.

*Research Project:* Because the FBI data on stolen guns pertain to individual cities and towns, a city-level analysis of crimes could be done. This would be a cross-sectional design because there is little data available on likely confounding variables at the city level for multiple time periods between censuses. The data would pertain to a census year such as 1990 or 2000 because many crime-relevant variables are measured for cities at the census years and thus could be statistically controlled to isolate the impact of stolen gun rates. The key independent variable in the statistical analysis would be the value of guns reported stolen (perhaps adjusted for the local cost of living) divided by city population, and the dependent variables would be the rates of Uniform Crime Reports (UCR) Index crimes.

### C. Weapon/Ammunition Lethality

The most persuasive rationale for gun control is that guns are more lethal than any other weapons likely to be substituted if guns were denied to those who would use the guns to attack others (or commit suicide). Yet, there is virtually no reliable information on the relative lethality of either guns vs. nongun weapons, or of different types of guns or ammunition, as distinct from the differing “lethality” of those who use the weapons. Since people with more deadly intentions are likely to use weapons they believe are more deadly, it is difficult to separate weapon lethality from user lethality. Much is claimed, but little is firmly known, about the relative lethality of guns vs. long-bladed knives, “assault weapons” vs. other guns, handguns compared to rifles and shotguns, or “cop killer” bullets vs. other bullets.

Some of the most scientifically sound information comes from laboratory ballistics tests (unknown to most violence researchers) in which projectiles are fired into blocks of standardized ballistic gelatin designed to simulate human tissue. These tests in effect hold constant the “lethality” of the shooter, but cannot compare guns with other weapons or, even regarding guns, simulate the full set of real-world circumstances that affect the impact of guns and ammunition on the human body. In any case, it would be helpful if the laboratory data could be supplemented by detailed information on real-world shootings.

*Research Project:* In one or more large cities, information would be gathered on both fatal and nonfatal assaultive injuries, involving both guns and other weapons that might be substituted for guns if they were not available. The ideal project would combine (1) the sort of detailed medical information on the wounds suffered that is available only from medical sources such as emergency room personnel and medical examiners with (2) detailed information on the circumstances of the shooting that is normally available only from police sources. Thus, the cooperation of at least one large city hospital in a high-crime area and the local police department would be required. One would like to know the impact of different kinds of weapons and ammunition on the seriousness of the injury produced, separate from the effects of other determinants of injury seriousness. One would also like to obtain information on how many separate wounds were inflicted, the location of the wounds, any vital organs that were struck, the range at which the attack was launched, the angle the projectile entered the body, whether the projectile struck some object before hitting the victim, struck bone in the victim’s body, fragmented, or exited the victim’s body, and whether the victim was heavily muscled or was wearing thick clothing.

Regarding the weapon, if it was a knife, one would like to know its length; if a blunt instrument, its weight. If it was a gun, one like to know type (revolver, semi-auto pistol, rifle, shotgun), caliber or gauge, barrel length, magazine capacity, number of shots fired, mass and shape of the bullets or shot fired, and the muzzle velocity of the ammunition. Each attribute of the weapon(s) and injuries inflicted in an attack would be separately coded for a statistical analysis in which the dependent variables would be measures of injury seriousness (lethal/nonlethal; number of wounds; seriousness of nonlethal wounds), and the key independent variables would be attributes of the weapons used in the attack, while other determinants of injury seriousness would be statistically controlled.

#### D. How Guns are Used in Crime

We know little about crucial tactical details of gun use in crimes, such as how the gun got into the crime situation and to whom the gun belonged. These issues are important to assessing the degree to which gun crime can be reduced by restricting the carrying of guns in public places or keeping guns locked up.

*Research Project:* A multi-state survey of imprisoned felons should address the question of how the gun was introduced into the crime situation, among felons who have committed violent crimes with guns. Did the offender carry the gun to the scene intending to use it in the crime, or for some other purpose such as self-protection, or make use of a gun that was already at the scene? To whom did the crime gun belong—the offender, the victim, someone else in the victim's household, or some third party? Was the gun loaded or locked at the time the offender decided to use it to commit a crime?

To some degree, some of this same kind of information could also be obtained by close examination of police offense reports and interviews with victims or investigating officers. A prison survey would, however, also have additional uses mentioned elsewhere in this agenda.

### II. GUNS IN THE HANDS OF CRIME VICTIMS AND PROSPECTIVE VICTIMS

#### A. The Accuracy of Estimates of Defensive Gun Use Frequency

The best available survey evidence indicates that the most common way in which guns are used in connection with crimes is not offensive use by the perpetrators but rather defensive use by the victims (Kleck and Gertz 1995). Strongly pro-control scholars have speculated, without empirical support, that surveys grossly overestimate defensive gun use (DGU) frequency because false positive reports (claiming a DGU that did not occur) enormously outnumber false negative reports (denying a DGU that did occur).

*Research Projects:* This topic is of enough interest that an extensive separate agenda addressing the possible sources of error in survey estimates of DGU frequency has been produced, listing 18 different projects that could be done (see Part IX of this article).

In addition, new surveys on DGUs could improve on previous ones by using larger samples and by asking about details of DGUs concerning where the guns were located prior to the DGU and how they were stored. That is, was the gun loaded and/or unlocked? About how long did it take to make the gun ready to use, after the person had decided to retrieve it? Comparing time estimates between those whose guns were locked in various ways (and/or unloaded) and those whose guns were unlocked would shed light on how often and to what degree laws requiring guns to be kept unloaded would delay defensive gun use.

#### B. Deterrent Effects of Gun Ownership among Prospective Victims

Are criminals ever deterred from attempting crimes by the prospect of running into an armed victim? If so, do they simply substitute some other victim, without any net reduction in crime? How do criminals react to the belief that gun carrying by prospective victims has increased? Do they refrain from some crimes, or do they arm themselves more heavily to deal with armed victims?

*Research Project:* The prison survey mentioned in section I.D. could be used to more thoroughly investigate topics only briefly touched on by Wright and Rossi (1986) in their 1982 survey of prison inmates in 10 states. Specifically, felons could be questioned about whether they had ever refrained from committing crimes because they knew or suspected that a prospective victim possessed a gun, the basis for their belief that the victim had a gun, what type of crime was involved, whether the gun was carried on the person, in a vehicle, or in the home of the potential victim, and what the offender would have done as a substitute for victimizing the armed victim.

### III. EFFECTS OF GUNS ON SUICIDE

#### A. Fatality Rates of Suicide Methods

The most common argument that gun availability affects suicide is that attempts by shooting are more likely to be fatal than attempts using other methods. Yet it has been almost 30 years since any serious attempt was made to compare fatality rates among suicide methods. This dated information indicated that the fatality

rate was 80% for attempts by hanging, 77% for attempts by carbon monoxide poisoning, and 75% for drowning attempts, while data from the 1990s indicate the rate for guns to be an essentially identical 78%. More recent data covering all major methods is needed

*Research Project:* In a large city researchers would conduct a systematic search of the records of hospitals, walk-in medical centers, private physicians' offices, and any other place where nonfatal injuries from suicide attempts were likely to be treated. By combining this information with data on suicide deaths by method from the county medical examiner, fatality rates by method could be computed.

#### B. The Impulsiveness of Suicide Attempts

Probably the second most common argument that gun availability affects suicide is that guns provide a quick method of suicide for people who were seriously intent on committing suicide for only a short period of time. Attempts by adolescents might be especially impulsive. Since attempts using alternative methods such as hanging require somewhat more time to carry out, some impulsive deaths might be prevented if a gun were not available. The key piece of missing information is how much time elapses between the moment that a person resolves to make a serious suicide attempt and begins to act on this decision, and the time they actually carry out the attempt.

*Research Project:* Normally this information could not be recovered after the fact because it is known only to the attempter. However, persons who attempt suicide but do not die might be able to provide estimates. Since at least 20% of attempters using even the most lethal methods survive, there are ample numbers of survivors for all methods if a sufficiently large population were studied, such as that of a large city or urban county. Lists of suicide survivors could be compiled from the same medical sources as were listed for the previous project, and as many as possible would be interviewed, perhaps by suicide prevention counselors.

#### C. Does Gun Ownership Affect Whether People Commit Suicide?

A few case-control studies comparing persons who committed suicide with either suicide attempters or community controls have been done, but are unsatisfactory because so few confounding factors were controlled. There is substantial potential for improvement, especially with respect to measuring personality or cultural traits that could affect both gun ownership and suicidal behavior. For example, gun owners often characterize themselves as self-reliant, and it is possible that self-reliance could, among depressed people, become self-blame. Those who see themselves as masters of their own fate may tend to blame themselves for their troubles, which could encourage suicide.

*Research Project:* A serious case-control study of guns and suicide is long overdue. A serious study would entail a systematic search through the prior literature for known or suspected suicide risk factors that may also be associated with gun ownership—an effort that clearly was not made with prior case-control studies. Persons who committed suicide, those who attempted it but survived, and nonsuicidal community controls could then be assessed for as many of these possible confounding risk factors as possible. (Psychologists have applied personality assessment inventories even to deceased persons, by questioning close relatives and intimates about the deceased.) By controlling for a respectable number of genuine confounders, more credible conclusions could be drawn about the impact of gun possession on the attempting and committing of suicide.

### IV. GUN ACCIDENTS

Very little is known about the shooters in gun accidents, excluding those where the injury is self-inflicted, because most information on participants pertains to victims. A study that is now a third of a century old was the last one to shed much light on the attributes of accidental shooters, showing that they were general risk-takers who often had been involved in assaults, motor vehicle accidents, and alcohol-related offenses, and had often been given traffic tickets for speeding and reckless driving. More information of this sort, of more recent vintage would be valuable.

Likewise it would be useful to know more details about the circumstances in which gun accidents occur. For example, were the guns locked and/or loaded prior to the incident? Was the shooter the authorized owner of the gun, or an unauthorized user? If many shooters in accidents are not authorized users, this

increases the accident-reduction potential of locking guns, since authorized users would normally possess the key or combination to disengage a lock. Likewise, how many accidents occurred after an unauthorized user defeated a locking device of some kind? This information would bear on the effectiveness of various devices in blocking unauthorized gun use, if the locks are conscientiously used.

*Research Project:* Because gun accidents, especially fatal ones, are rare (only about 900 in 1998 in the entire U.S.), either a very large city would have to be studied over many years, or multiple fairly large cities or counties would need to be studied for a sufficient number of cases to be available. Police offense reports and (for fatal cases) medical examiner reports would provide some information on shooters and circumstances of shootings, while interviews with shooters, victims of nonfatal accidents, and witnesses could provide more information, on both fatal and nonfatal shootings. Finally, searches of criminal history files (including traffic offenses) could shed more light on prior risk-taking behavior by shooters.

A closely related research project, which could be combined with the descriptive study, would be a case-control study comparing accidental shooters with a representative sample of gun owners in the general population. This could shed light on factors that affect accident involvement, the impact on accidents of gun safety training, and the effectiveness of various gun storage practices and different locking devices.

## V. CRIMINAL ACQUISITION OF GUNS

How and from where/whom do criminals get guns? It has been 18 years since Wright and Rossi (1986) interviewed imprisoned felons to address these questions, making their information dated. Further, even in 1982, their questioning of felons left many questions unanswered. They asked only about the origins of the felon's most recently acquired gun, rather than guns connected with particular kinds of crimes. This made it impossible to determine, e.g., how murder guns, or robbery guns, were acquired.

*Research Project:* The multi-state survey of imprisoned felons discussed previously could question felons about guns used in the most recent gun crime they committed. The focus of questions would be on the combination of:

- (1) types of sources of guns (licensed dealers in stores, relatives, crime victims, etc.) and
- (2) modes of acquisition (purchase, theft, trade, gift, inheritance, borrowed).

Careful distinctions, not made in the Wright-Rossi study, would be made between licensed and other sellers, licensed dealers operating in stores vs. licensed dealers operating out of their homes, licensed dealers operating at gun shows vs. nonlicensees selling guns at gun shows, illicit dealers who make a living from significant numbers of illicit sales vs. thieves and drug dealers who occasionally sell a few guns, and intrastate illicit dealers vs. interstate illicit dealers.

For juveniles, important distinctions could also be made between buying a gun themselves directly from a licensed dealer vs. making such a purchase using a "straw purchaser," i.e. a legally eligible older person who bought the gun on their behalf. Another important distinction relevant to youth would be between guns given to them by a parent and guns the youth stole or "borrowed" from his parents without their knowledge.

## VI. GUN CONTROL OPINION AND SOURCES OF SUPPORT FOR GUN CONTROL

Why do people support gun control? The obvious answer to this question is that supporters want to reduce violence and crime, and believe that gun control laws will help accomplish this. This answer, however, is at best only a very partial explanation in light of the fact that many, possibly most, supporters believe gun laws will have little or no impact on violence.

Conversely, it is not obvious why people oppose gun laws. Gun ownership is not a sufficient reason for opposition, given that many gun owners in fact support a wide variety of moderate restrictions on guns, and nearly all support at least a few kinds of controls. Virtually all survey research on these questions has been confined to analysis of responses to questions about gun purchase permits. As a result, little is known about determinants of positions on other gun measures or whether patterns of support significantly differ by type of control measured considered. Also, survey work has not convincingly established the strength of commitment to gun control positions, as opposed to merely establishing whether the person favors or opposes a measure.

*Research Project:* A national survey could explore the determinants of support for, or opposition to, gun control. The survey should measure opinions on a wide variety of gun control measures, both those proposed and those already law, ranging from popular but weak measures like waiting period provisions to less popular but strict controls like bans on the private possession of handguns or all guns.

Strength of commitment could be measured by self-reported past actions taken on behalf of a position rather than mere verbal claims about the strength of one's views. Examples would include writing a letter to the editor, attending a meeting concerning the topic, voting for a political candidate largely on the basis on his or her gun control positions, joining an organization devoted to the issue, or contacting an elected representative to express one's views on the issue.

Some of the possible determinants of gun control positions that have previously received little or no attention are: anti-government attitudes (suspicion of the intentions of government officials; skepticism about the efficacy of governmental actions in reducing social problems); attitudes towards the use of force for self-protection; self-reliance; number of friends or relatives who own guns; and the value placed on guns as sources of recreation or aesthetic objects.

## VII. ENFORCEMENT OF GUN LAWS

Many people believe that unenforced laws are unlikely to reduce crime. Gun control opponents commonly ask "Why pass more gun laws if we are unable or unwilling to enforce the ones we already have?" This has recently become one of the chief rhetorical tactics of the National Rifle Association (NRA) in countering demands for more gun control laws. Yet, we know almost nothing about how much existing gun laws are enforced, in the sense of police making arrests for violations, which laws are enforced more than others, or why some gun laws are not enforced while others are. For example, how often do police make arrests for violations of registration or gun owner licensing laws, where these kinds of laws exist at the state level?

The FBI's Uniform Crime Reports data on arrests addresses only "weapons violations," lumping gun violations of all types together and not even distinguishing between violations involving guns and those involving other weapons. Very limited information currently suggests that enforcement is largely confined to arrests for unlawful carrying of concealed weapons in public places.

*Research Project:* There may already be a great deal of information on enforcement of gun laws in law enforcement agencies' files, in computerized datasheets, and possibly even in departmental reports and internal memoranda receiving little or no circulation outside the agency. One simple project therefore would begin by contacting the thousands of law enforcement agencies in the nation and asking what information they already have on hand on arrests for gun violations, beyond what they report to the FBI.

If such material is not available from a wide variety of agencies, more detailed information would have to be compiled by researchers themselves, by going through police arrest reports to identify exactly what kind of legal violation and what kind of weapon was involved. The project should cover a variety of jurisdictions in multiple states, so as to generate data on the full array of gun laws.

## VIII. EFFECTS OF GUN CONTROL MEASURES

### A. Macro-level Assessment of the Impact of New Laws

Interrupted time series studies of changes in crime before and after a new law are worthless for judging the impact of the gun law. They cannot separate effects of the laws from effects of any other confounding changes occurring at the same time, and they can be (and often are) selectively applied to one or a few unrepresentative areas that artificially favor the analyst's biases about the effectiveness of the laws. The method should not be used any further, as it is prone to abuse and as likely to generate misinformation as useful evidence (Britt et al. 1996).

Thorough cross-sectional comparisons of jurisdictions with different laws have already been carried out, utilizing multivariate controls for many other confounding factors (Kleck and Patterson 1993), but this approach cannot detect short-term effects of new laws.

Another useful approach is pooled cross-sections time-series studies, otherwise known as multiple time series (MTS) designs, applied to entire sets of legal jurisdictions. This approach could be used with monthly

crime data, but these data are available only for unrepresentative subsets of states and are of dubious reliability (many agencies submit only annual reports or very irregularly submit reports covering shorter periods). This methodology can nevertheless detect reasonably short-term effects even when applied to annual data. Further, it avoids the problem of selectively studying just a few unrepresentative jurisdictions, and provides respectable sample sizes (roughly equal to the number of areas studied, times the number of time periods). Also, the inclusion of dummy variables for individual states and years reduces the problem of confounding variables somewhat, and at least a few possible confounding variables can be directly measured for states on an annual basis and thus directly controlled in a statistical analysis. Marvell and Moody (1995) provide a reasonable general model of how the MTS design can be used to assess gun law impact.

*Research Project:* The MTS design should be applied to state-level annual data covering the 50 states and D.C. for the full period for which requisite UCR data and gun law information are available—probably at least 1968-98. The core of the project would be detailed coding (by law students?) of whether each one of an exhaustive list of types of gun laws were in effect in a given state in a given year. The baseline set of laws in effect at the beginning of the study period would be established using statute books pertaining to that time point, then Session Laws (available in any good law library) would be used to identify every significant change in gun law passed by the state legislature during a given session, along with its effective date. Thus, every year in every state would be coded 1 or 0, denoting whether a given type of gun restriction was in effect. The standard fixed effects models would be estimated, with the dependent variables being the UCR Index crime rates, the suicide rate and the fatal gun accident rate. Where data permitted, gun and nongun varieties of these rates would be separately analyzed for homicide, suicide, robbery, and aggravated assault (e.g. gun homicide and nongun homicide rates). Interest would focus on whether gun law dummy variables had significant negative coefficients, indicating that crime or violence rates were lower in places and times where a given legal restriction on guns was in effect.

Modified versions of this design might also be applied to counties and large cities, to incorporate local gun ordinances. The gun law information for cities might even be supplemented by Lexis-Nexis searches for news stories describing changes in local gun policies that did not involve statutory change, such as gun “buy-back”/amnesty programs and police crackdowns on unlicensed carrying of guns.

#### B. Use of Gun Registration Records.

The principle goal of the Brady Campaign (BC) is to get the Brady II bill passed, which mandates federal licensing of gun owners and registration of guns. BC argues that registration will help police solve gun crimes, yet offers no evidence that police have actually solved significant numbers of crimes with registration records that otherwise would have gone unsolved, despite the fact that at least eight states already register handguns (California, Hawaii, Maryland, Massachusetts, New Jersey, New York, Pennsylvania) and thus provide ample opportunity to determine the degree to which police in fact solve crimes and obtain convictions through the use of registration records (U.S Bureau of Justice Statistics 1996).

*Research Project:* Examine the records of cleared violent crimes involving guns, drawn from the files of police departments in large cities located in the registration states—as many departments as will cooperate with the research. Based on a careful reading of offense and arrest records, and interviews with investigating offices, coders would assign each solved gun crime to one of the following categories:

- (1) there was no evidence that registration records were used in any way,
- (2) registration records were somehow used, but were incidental or unimportant in identifying or convicting a perpetrator,
- (3) the use of the records was important but not necessarily essential to the identification or conviction of a perpetrator, or
- (4) use of the records was essential to identifying/convicting a criminal.

### C. Criminal Responses to New Gun Controls

How do criminals anticipate they would respond if new gun controls were implemented? If laws were effective in denying guns to some criminals, how would they adapt to the change in circumstances?

*Research Project:* The prison survey could be used to inquire about this topic. For example, it would be useful to know how criminals anticipate they would respond if they were unable to acquire a handgun. Would they (a) do without a weapon of any kind, (b) substitute less lethal weapons such as knives or blunt instruments, or (c) substitute a more lethal weapon such as a sawed-off shotgun or rifle? Wright and Rossi (1986) addressed this topic briefly in 1982; a new survey could not only update their information, but could also make careful distinctions not made in the earlier survey, between weapon substitution in (a) the weapons kept for self-protection in the criminal's home, (b) weapons carried in public places for self-protection, and (c) weapons carried specifically to commit crimes.

## IX. ASSESSING THE VALIDITY OF SURVEY ESTIMATES OF DEFENSIVE GUN USE: A RESEARCH AGENDA

A number of steps might be taken to explore some of the sources of error in surveys estimating defensive gun use (DGU) frequency. Some sources are probably unmeasurable, precluding any definitive statements about the net effect of all of the sources of error. Nevertheless, the following proposals address the more measurable sources of error, and present some general strategies for estimating or exploring them. The general design for assessing many of these sources of error would be that of the survey experiment, in which respondents (Rs) are randomly assigned to various survey conditions. Many of the methodological conditions could be randomly varied together, using factorial survey methods.

### A. Survey Organization Effects

To what degree are Rs influenced by the fact that they are interviewed by employees of the Census Bureau, working on the National Crime Victimization Survey (NCVS), when they are questioned about self-protective actions? This project would require the cooperation of the U.S. Census Bureau, and thus might be difficult to implement. A sample could be generated by conventional random digit dialing (RDD) procedures, except that two survey organizations would be simultaneously doing so: the Census Bureau, and some good-quality private survey firm. Both would use the same interview schedules, except that at the start of the interview, the Census interviewers would (accurately) tell Rs that they were from the Census Bureau, as in the NCVS, while the private firm interviewers would mention their firm's name.

### B. Purpose-of-Interview Effects

Within each survey organization sample, Rs would be randomly assigned as to whether they would be told, at the start of the interview, that the survey was designed to gather information for the U.S. Justice Department (as is done in the NCVS).

### C. Anonymity Effects

Within each survey organization sample, Rs would be randomly assigned as to whether they would be asked for their names and addresses (as is done in the NCVS) at the beginning of the interview, using the same questions to do so as in the NCVS. These Rs would only be assured that their responses would be held in confidence. The other Rs would be guaranteed complete anonymity, and explicitly assured that they were randomly selected and that the interviewers did not know their names.

### D. Question Sequence Effects

Within each survey organization sample, Rs would be randomly assigned as to whether they would be (1) first asked questions about whether they had been a victim of crime in the past 12 months, and then asked (if they reported a victimization) whether they had used a gun for self-protection, or would (2) first be asked whether they had used a gun for self-protection, followed by questions getting at details of the alleged DGU, including the type of crime the R thought was involved.



#### E. Question Wording Effect: General vs. Specific Prompts.

Within each survey organization sample, Rs would be randomly assigned as to whether they would be asked a general question about whether they used any self-protection in connection with a reported incident, vs. a more focused question specifically asking about use of a *gun* for self-protection.

#### F. Question Wording Effect: Stressing the Reporting of Minor Events/Incidents with Favorable Outcomes

Within each survey organization sample, Rs would be randomly assigned as to whether they would be asked a DGU question that stressed that even minor crime events, and those where the victim was not injured and did not lose property, are relevant, vs. a question that did not stress this. This procedure could provide information on the effects of Rs failing to report minor or successful DGU incidents.

#### G. Question Wording Effect: Stressing the Reporting of Incidents Involving Intimates

Within each survey organization sample, Rs would be randomly assigned as to whether they would be asked a DGU question that stressed that incidents where guns were used to defend against a family member or someone close to the R would be relevant.

#### H. Direct Interviews of All Household Members Age 12 and Over

Random subsamples of households contacted via RDD would be exposed to the Census NCVS procedure of interviewing every member of the household age 12 and over regarding personal (as opposed to household) victimizations. In other households, a single adult informant would be interviewed, providing information on behalf of all household members as to whether any of them had experienced a DGU, as has been done in many prior DGU surveys.

#### I. Covering the Adolescent Population

The foregoing procedures would allow coverage of persons age 12 to 17 (as is done in the NCVS), in addition to those age 18 and over, to determine how much larger aggregate DGU estimates would be if those involving adolescents were included.

#### J. Covering Households without Telephones

Local studies could be conducted in which computerized utility and voter registration records were compared with telephone directories to identify persons apparently lacking telephone service. Using the names and addresses in these lists, face-to-face interviews could be conducted in which the same basic interview schedule was used with both persons without telephones and persons with telephones. Lack of telephone service could be confirmed early in the interviews with those on the phoneless list, then the usual DGU questions would be asked. DGU frequency would then be compared between households with and without telephones.

#### K. Sample Size and Repeat Users

If the sample size could be greatly increased, e.g. to 10,000 cases, it would allow more stable estimates of repeat DGU experiences. In past surveys there were no more than a few dozen repeat users, yet these few could have large effects on incidence estimates because the overall sample sizes were small enough that a few Rs reporting unusually large numbers of DGU experiences would greatly increase the total number of reported incidents. Under these conditions it would be worthwhile applying the same thorough questioning to every claimed incident that in past surveys was applied only to a single (typically the most recent) incident, to uncover false positives.

#### L. Recall Period Effects

Rs would be randomly assigned to be asked about their experiences in the past 6 months (the NCVS recall period), 12 months, or five years (the Kleck-Gertz recall periods). This could provide information on the effects of forgetting incidents, especially minor one.

#### M. Telescoping Effects

A two-wave panel design could be used, with one survey using a one-year recall period being followed by another survey of the same Rs (recontacted by calling the same telephone numbers and asking for anyone in the household who had previously been interviewed on this topic) a year later. The second wave of interviews would be “bounded”—interviewers would be able to tell if an incident 12-24 months old was being telescoped into the past-12-months recall period, since such an incident would have been reported in the first wave of interviews. This would permit a fairly direct estimate of the inflation of DGU estimates due to telescoping.

#### N. Follow-up Interviewing to Detect and/or Resolve Inconsistencies

For those Rs reporting a DGU, follow-up interviews could be conducted, perhaps 30 days later, to see if Rs gave consistent accounts of the events, and also to try to resolve any inconsistencies that were apparent in the initial interviews.

#### O. In-Depth Interviewing of Rs Reporting a DGU

All Rs reporting a DGU could be asked if they would be willing to discuss their experiences at greater length. Those agreeing could be given longer interviews, possibly tape-recorded when allowed by Rs. These interviews would focus on determining (1) the exact nature of the threat or crime that the R thought was being committed against them, (2) what precisely the R did with their gun, and (3) whether an objective observer might have thought the R was the aggressor rather than the victim. The in-depth interviews might also utilize the full NCVS incident report question sequence, so as to nail down whether a crime by NCVS standards was being committed against the R when the alleged DGU occurred, as well as to obtain further information on details of the events.

These interviews could also include a question concerning whether the R thought that anything they did, including possession of the gun at that time and place, was unlawful. This would provide some baseline information on how often DGU-involved Rs thought they had reason to conceal the event from police or other strangers, such as survey interviewers, among Rs willing to report DGUs. This would probably be a minimum estimate because such concerns would almost certainly be greater among those were not willing to report a DGU at all.

#### P. Cross-checking with Police Reports

Rs reporting a DGU would be asked if they reported the event to police; if they did, they would be asked what law enforcement agency they reported the event to, and the approximate date. The agencies involved would be asked for records of the events, and these records would be compared with the Rs' accounts. This would provide information only on a nonrepresentative subset of alleged DGUs, presumably the more legitimate ones, but still could provide some useful information about those kinds of incidents. Police reports, for example, might be more likely to reveal provocative behavior by the “victim” that would suggest that their actions were not purely defensive.

#### Q. Reverse Record Checks

The reverse of the foregoing would be to identify recent (past 6 or 12 months) crime incidents in police files in several large cities in which victims reported using guns for self-protection. Using the names, addresses, and/or telephone numbers of these victims in the police reports, these persons could be contacted and (without the interviewers saying how they got the R's name) given the same interview as was applied to members of the general population, to see if these “known cases” would report a DGU. This could not get at

DGUs that victims were not willing to report to anyone, police or survey, interviewers, but it could provide a minimal baseline estimate of underreporting of DGUs to survey interviewers.

#### R. Questions to Explore Sources of Reluctance to Report DGUs

This procedure would be intended to investigate possible reasons why Rs with a genuine DGU experience might fail to report them. All Rs in gun-owning households would be asked whether they would be likely to report a hypothetical DGU to an interviewer in a survey. They would be presented with DGU scenarios in which certain elements were randomly varied, including whether or not:

- (1) the R was in a public place, unlawfully carrying a firearm,
- (2) R was in their home, in possession of an unregistered gun in a place where registration was required,
- (3) the DGU was against a person with a close relationship to the R,
- (4) the R was certain their gun use was genuinely defensive and they were in the right, but the police might nevertheless view the R as the aggressor.

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