National Case-Control Study of Homicide Offending and Gun Ownership*

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Does gun ownership increase the likelihood that a person will commit a homicide? Findings from a recent case-control study (Kellermann et al. 1993) were interpreted as indicating that persons who lived in households with guns were 2.7 times as likely to become homicide victims as persons in households without guns. Problems with that study are identified, and a different approach is described. Survey data on a nationally representative sample of persons in prison for criminal homicide were compared with data on a nationally representative sample of the general population, in the first national case-control study of homicide. A logistic regression analysis was performed on the data, with the dependent variable measuring whether the subject was a killer, and the key independent variable being whether the person owned a gun. Control variables included age, sex, race, Hispanic ethnicity, income, education, marital status, region, veteran status, and whether the subject had children. Results indicated that gun ownership had a weak (odds ratio = 1.36) and unstable relationship with homicidal behavior, which was at least partly spurious. The promise and pitfalls of case-control research are discussed.

The impact of gun availability and use on violence is the subject of intense debate among scholars, policymakers, and the general public. In 1993, about 39,600 people died of gunshot wounds, 19,000 of them in homicides, and offenders armed with guns committed about one million violent crimes. There are over 230 million guns in private hands, about half of U.S. households have at least one gun, and perhaps 2.5 million times a year a crime victim uses a gun for self-protection (Kleck 1997a; Kleck and Gertz 1995). Although both offenders and victims use guns in a large number of violent crimes, it is in dispute just how, or whether, guns influence the frequency and outcomes of violent crimes.

One of the most highly publicized studies of the guns-violence link in recent years was a case-control study of homicide victimization by Arthur Kellermann and his colleagues (1993) published in the prestigious *New England Journal of Medicine.* This frequently cited study concluded that gun possession increased the risk of homicide victimization by a factor of 2.8. This paper is designed to assess the credibility of this conclusion and to offer an improved estimate of the impact of gun ownership on homicidal violence.

Theory and Weapon Effects on Violence

Mainstream social science and criminological theory aims at explaining only certain aspects of crime-related phenomena, focusing heavily on why some individuals are more likely to engage in criminal, delinquent, or deviant behavior than others (e.g., see reviews in Akers 1994; Vold, Bernard and Snipes 1998). It has traditionally had little to say about what

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might affect the outcomes of individual criminal incidents, and thus why some encounters between criminals and their victims have very serious consequences for victims while other encounters do not.

For example, it would seem to be an important scientific goal, as well as an issue relevant to public policy, to discover why some victims of violence die, while others suffer nonfatal injuries, others are attacked but not injured, and still others are threatened but not actually attacked. But because mainstream theories of criminal behavior and deviance rarely address themselves to this sort of incident-oriented question, the task of trying to answer such questions has mostly fallen to empirical researchers who develop new theory on a fairly ad hoc basis to explain and organize their empirical findings.

More specifically relevant to the present paper, one would like theory to address the general question: "Why and how would gun ownership affect the likelihood of a person taking the life of another human being?" Part of the problem of answering this question has been that some people think the answer is already self-evident: guns are more lethal than other weapons that could be substituted for them. The following discussion (adapted from Kleck 1997a) is intended to provide a somewhat broader consideration of the potential effects of weaponry on interpersonal violence.

Guns and Power

The power that weaponry confers has been conventionally treated as exclusively violence-enhancing—it is commonly assumed that the use and possession of weapons serve only to increase the likelihood of the victim's injury and death (e.g., Zimring 1968, 1972). This is an unduly restrictive view of weaponry's significance. A broader perspective starts by recognizing weapons as sources of power, used instrumentally to achieve goals by inducing compliance with the user's demands. The ultimate goal behind an act of violence is not necessarily the victim's death or injury, but rather may be the acquisition of money, sexual gratification, respect, attention, or the humiliation and domination of the victim. Power can be, and usually is, wielded so as to obtain these things without inflicting injury. Threats, implied or overt, usually suffice and are often preferred to physical attack. Inflicting injury may even be an indication that the preferred mode of exercising power failed.

Weapon Effects on the Likelihood of Attacking an Adversary

It has long been argued that firearms give some people the courage to attempt aggressive acts that they would otherwise be afraid to attempt. In particular, a weapon may be especially important in facilitating attacks by weaker aggressors against stronger victims. Further, unlike other common personal weapons, guns permit effective attack from a great distance, although few assaults occur at ranges longer than the length of the average living room (Kellermann et al. 1996:1441).

Nevertheless, for some attackers, maintaining a distance of just a few feet or even inches from their victim may be essential to carrying out an attack. It has been hypothesized that guns may facilitate attack by persons too timid or squeamish to come into physical contact with their victims or to use messier methods to injure them (Wolfgang 1958:79). Some prospective attackers may be psychologically incapable of doing something as distasteful and ugly as plunging a knife into another person's chest cavity or bashing a victim's skull with a blunt instrument, yet capable of shooting their victims. Guns provide a more impersonal, emotionally remote, and even antiseptic way of attacking others, and could allow some attackers to bypass their inhibitions against close contact with their victims.

Possession of guns may also create a "triggering" effect on the likelihood that the weapon possessor attacks an adversary. Experimental psychologists Berkowitz and LePage (1967) pro-

posed the "weapons effect" hypothesis, which stated that the sight of a weapon could trigger aggression from angered persons, due to the learned association between weapons and aggressive behavior.

On the other hand, weapon possession may also have aggression-inhibiting effects. In an early study of victim survey data from eight cities, Hindelang (1976:263) concluded that "when a gun is involved in a victimization, both the victim and the offender appear to be more restrained and interested in avoiding an attack with the weapon." At least twenty studies have consistently confirmed that criminal aggressors armed with guns are *less* likely to attack and injure (rather than merely threaten) their victims than aggressors without guns (reviewed in Kleck 1997a:225–226).

Thus, not only does the victim's defensive use of a gun inhibit aggressor attacks (Kleck 1997a:171–174), but even the aggressor's possession of a gun may inhibit their own aggression, as well as that of the victim. In many assaults, the aggressor not only lacks an intent to kill, but specifically wants to avoid killing the victim. Instead, they may want only to frighten or to hurt without killing. Possession of a lethal weapon gives such an assaulter more killing power than she/he needs or wants, and to attack would risk inflicting more harm than the assaulter wanted. The possession of deadly weapons raises the stakes into what may seem to be an all-or-nothing situation—kill or do not attack at all. Given that the intentions of assaulters, as a group, cluster predominantly at the less deadly end of the continuum, one common effect of aggressor possession of guns and other deadly weapons could therefore be inhibition of attack behavior.

There is another possible explanation for the lower injury rates in incidents where offenders are armed with guns. A deadly weapon empowers its possessor to terrify, coerce compliance with demands, deter another's aggression, nonfatally injure, or kill, and power increases the likelihood its user will get what he/she wants, whatever that may be. If most assaulters do not want to kill, then a lethal weapon enables its user to achieve the other goals. In robberies, the offender's use of a gun ensures compliance with the demands for money and deters the victim from resisting by convincing the victim that the robber has the capacity to inflict death or serious injury (Luckenbill 1982). Without a gun, it would often be impossible for the robber to achieve this without actually attacking the victim. Threat with a gun can thereby serve as a substitute for actual attack, rather than its vehicle, and possession of a gun can make a physical attack unnecessary.

This pattern need not be limited to acquisitive crime such as robbery. Aggressors in ordinary anger-instigated assaults have their own peculiar goals whose attainment can, if they have a weapon, be achieved without attacking. Those who want to frighten, humiliate, or dominate their victims can do so by merely pointing a gun, without firing it. On the other hand, without a gun, nothing short of attack may suffice. The same qualities of weapons that make them dangerous if used to attack can preclude the need to actually do so.

A combatant may also regain a favorable situational identity by using a weapon to control others and compel their unwilling obedience. They can demonstrate to their victim, to themselves, and to any bystanders that they cannot be pushed around, and that they must be granted respect, or at least fear. The weapon can place its possessor into a superordinate position in situations in which this might otherwise be impossible to achieve without an actual attack.

One combatant's use of a lethal weapon may also give the opponent a socially acceptable excuse for not retaliating for an insult or other challenge to her/his self-image: "only a fool attacks someone with a gun." The failure to retaliate, which might otherwise be regarded by witnesses as evidence of cowardice, is instead viewed as mere prudence in the face of greatly unequal power. The extreme imbalance of power can thus prevent an escalation to physical violence by exacting from the weaker opponent some gesture of deference or an exit from the scene.

Weapon Effects on the Likelihood of Injury

If an attack does occur, it may or may not result in injury. The attributes of weapons that can facilitate attack may also reduce the attack completion rate by encouraging attacks at a longer range, against more formidable opponents or under more difficult conditions. It is possible to shoot a victim from a great distance, but the rate at which this is achieved is likely to be far lower than the rate at which thrown punches land. Concerning the more common close-range gun attacks, those unfamiliar with firearms marksmanship might assume that shooters are virtually certain to hit their target. This assumption is not born out by the real-life experiences of persons shooting under conditions of emotional stress.

Weapon Effects on the Likelihood of the Victim's Death

Probably less than 15 percent of gunshot woundings known to police result in death (Cook 1985). Assuming that only half of nonfatal woundings (but all fatal woundings) are reported or known to police, the true fatality rate would be somewhere under 8 percent (based on a doubling of nonfatal woundings). Nevertheless, gunshot wounds are more likely to result in death than those inflicted by a knife, the weapon generally assumed to be the next most lethal, among those that could be used in the same circumstances as guns.

Only some of the difference in death rates of attacks with different weapons is attributable to the technical properties of the weapons themselves. Part of the difference may be due to the greater "lethality" of the users of the more deadly weapons. When a gun is used in an attack, it is almost always the result of a choice, however hastily made, among weapon alternatives. It is a rare gun homicide that occurs where a knife or blunt instrument is not also available, and all gun killers also have hands and feet with which they could have attacked the victim.

Those with more lethal intentions, a greater willingness to hurt others, or a stronger instigation to aggress will tend to choose more serious weaponry, regardless of how vague their intentions are, or how impulsively, quickly, and even unconsciously these might be arrived at. Thus, weapon lethality and attacker lethality should be closely associated, and their effects can easily be confused with one another (Cook 1982:247–248). If the wounding fatality rate of guns is four times higher than that of knives when the attackers are not matched regarding their lethality, then this ratio would necessarily be less than 4–1, though probably higher than 1–1, if one could control for the greater lethality of attackers choosing guns.

Finally, it is possible that possession and defensive use of guns by prospective *victims* influence the likelihood of an attack occurring and thus the likelihood that fatal or nonfatal injury could occur. Some prospective attackers could be deterred from attempting an attack by the specific knowledge that a prospective target is armed or by the knowledge that many victims in general are armed or could be armed. Further, a victim's actual use of a gun could deter an attack or cause the aggressor to cut off the attack before inflicting serious or fatal injury (Kleck 1988; 1997a:171–174).

Previous Research

The effort to assess the effects of guns on violence has involved an extremely diverse array of methodological strategies that can be broadly divided into macro-level and micro-level categories (see Cook 1991; Kleck 1995; 1997a for reviews).

Macro-level Research

Some scholars have used macro-level approaches, examining gun ownership levels and violence rates as they covary across areas or over time (summarized in Table 1). Most of this

Table 1 • *Macro-Level Studies of the Impact of Gun Levels on Violent Crime Rates*[a]

Study	*Sample*	*Modeled 2-way Relat.?*	*Measure of Gun Level*[b]	*Crime Rates*[c]	*Results*[d]
Brearley (1932)	42 states	No	PGH	THR	Yes
Krug (1968)	50 states	No	HLR	ICR	No
Newton & Zimring (1969)	4 years, Detroit	No	NPP	THR,TRR, AAR,GHR	Yes
Seitz (1972)	50 states	No	GHR,FGA,AAR	THR	Yes
Murray (1975)	50 states	No	SGR,SHR	GHR,AAR,TRR	No
Fisher (1976)	9 years, Detroit	No	NPP,GRR,PGH	THR	Yes
Phillips et al. (1976)	18 years, U.S.	No	PROD	THR	Yes
Brill (1977)	11 cities	No	PGC	ICR	No
				THR	Yes
				TRR	No
Kleck (1979)	27 years, U.S.	Yes	PROD	THR	Yes
Cook (1979)	50 cities	No	PGH,PGS	TRR	No
				RMR	Yes
Kleck (1984)	32 years, U.S.	Yes	PROD	THR	No
		No		TRR	Yes
Magaddino & Medoff (1984)	31 years, U.S.	Yes[e]	PROD	THR	No
Lester (1985)	37 cities	No	PCS	VCR	No
Bordua (1986)	102 counties, 9 regions	No[f]	GLR,SIR	HAR,THR, GHR	No
McDowall (1986)	48 cities, 2 years[g]	Yes	PGH,PGS	TRR	No
Lester (1988)	9 regions	No	SGR	THR	Yes
McDowall (1991)	36 years, Detroit	Yes	PGS,PGR	THR	Yes
Killias (1993)	16 nations	No	SGR	THR,GHR	Yes
Kleck & Patterson (1993)	170 cities	Yes	[h]	THR,GHR, TRR,GHR, AAR,GAR	No

Notes:

[a] Table covers only studies and findings where the dependent variable was a crime rate, as opposed to the fraction of crimes committed with guns.

[b] Measures of Gun Level: FGA = Fatal gun accident rate; GLR = Gun owners license rate; GMR = Gun magazine subscription rates; GRR = Gun registrations rate; HLR = Hunting license rate; NPP = Number of handgun purchase permits; PGA = % aggravated assaults committed with guns; PGC = % homicides, aggravated assaults and robberies (combined together) committed with guns; PCS = same as PGC, but with suicides lumped in as well; PGH = % homicides committed with guns; PGR = % robberies committed with guns; PGS = % suicides committed with guns; PROD = Guns produced minus exports plus imports, U.S.; SGR = Survey measure, % households with gun(s); SHR = Survey measure, % households with handgun(s); SIR = Survey measure, % individuals with gun(s).

[c] Crime Rates: AAR = Aggravated assault rate; GAR = Gun aggravated assault rate; GHR = Gun homicide rate; HAR = Homicide, assault and robbery index factor score; ICR = Index crime rate; RMR = Robbery murder rate; THR = Total homicide rate; TRR = Total robbery rate; VCR = Violent crime rate.

[d] Yes = Study found significant positive association between gun levels and violence; No = Study did not find such a link.

[e] Authors modeled two-way relationship, finding no effect of guns (see column 2 of their Table 9-5), and also reported an effect of guns in a less appropriate model where this was not done (see Column 1 of their Table 9-5).

[f] A few gun-violence associations were positive and significant, but almost all involved female gun ownership or male long gun ownership. Bordua interpreted the pattern to indicate the effect of violence on gun ownership.

[g] Panel design, two waves.

[h] 5-item factor composed of PGS, PGH, PGR, PGA, and the percent of dollar value of stolen property due to stolen guns.

research is technically weak, using (1) small samples, with correspondingly unstable findings, (2) unvalidated and often invalid measures of gun availability, and (3) few or no controls for other violence-related factors whose effects could be confounded with those of gun availability.

More important still, most of these studies do not distinguish (1) the positive effects of violence rates on gun acquisition for defensive reasons from (2) the positive effects of gun availability on violence rates. A number of individual-level studies indicate that acquisition of handguns and other weapons for protection is directly or indirectly increased by residence in a high crime area (Kleck 1997b; Lizotte, Bordua and White 1981; Smith and Uchida 1988), and macro-level studies support the hypothesis that higher violence rates increase gun levels (Kleck 1979, 1984; Magaddino and Medoff 1984; McDowall 1986; Kleck and Patterson 1993). Almost all of the studies that did take account of this possible two-way relationship and that used validated measures of gun availability have found that while violence rates affect gun levels, levels of gun or handgun ownership have no net impact on violence rates, including homicide rates (Table 1; Kleck 1997a).

Individual-Level Research

At the individual level of analysis, psychologists have conducted experimental studies of the effects of weapons on artificial forms of "aggression" in the form of mild electric shocks and other noxious stimuli. The relevance of these studies to serious real-world violence is questionable, and the results are evenly divided between those finding aggression-instigating effects and those not. Most of the more realistic studies, however, fail to find any aggression-increasing effects (Kleck 1991:158–161, 205–206; Toch and Lizotte 1991).

Other scholars have studied individual incidents of violence, such as robbery or assault incidents, comparing those committed by gun-armed offenders with those committed by offenders without guns (e.g., Kleck and DeLone 1993; Kleck and McElrath 1991; Kleck and Sayles 1990; Zimring 1968, 1972). Over twenty such studies have consistently found that possession of guns by aggressors reduces the likelihood they will attack and injure their victims (instead using guns only to threaten), but increases the likelihood that any injury inflicted will be fatal (Kleck 1997a).

Other research on the effects of guns in the hands of victims and prospective victims consistently indicates that victims who use guns for self-protection are less likely to be injured or to lose their property than otherwise similar victims who either do not resist at all or resist without a gun (e.g., Cook 1991; Kleck 1988; Kleck and DeLone 1993; see others reviewed in Kleck 1997a:225–226). There is scholarly debate about how often guns are used for protective purposes; while some scholars stress the low estimates implied by the National Crime Victimization Surveys (NCVS [e.g., McDowall 1995]), at least fifteen surveys indicate far larger numbers of uses, ranging from 700,000 up to 3.5 million or more. As yet, no other survey has even approximately confirmed the low NCVS-derived estimates (Kleck 1997a:149–159, 187–189; Kleck and Gertz 1995).

Case-Control Research on Homicidal Behavior

Recently an old methodology has been newly applied to the issue of how gun possession might influence the incidence of homicide. Kellermann and his colleagues (1993) applied case-control methods comparing homicide victims with matched control subjects, to see whether gun ownership was more common in the victims' households than in those of the matched controls.

A case-control study is a retrospective comparison of (1) individuals possessing a given trait (the "cases"), often a relatively rare one (e.g., delinquency, violent behavior, or lung cancer) with (2) individuals lacking the trait (the "controls"). The purpose is to explore possible causes of the trait by comparing persons possessing the trait with those lacking the trait. The

case-control design also commonly involves the rare trait being oversampled through the use of archival records or lists of known cases, which helps insure that the investigator has enough rare cases to compare with the more numerous persons lacking the trait (Goodman et al. 1988; Schlesselman 1982).

"Case-control study" is a fairly recent term for a research design that has been around for decades. By the 1920's, sociologists studying the causes of delinquent behavior were comparing caught delinquents, typically the inmates of juvenile institutions, with samples of the general adolescent population, typically students. These sociologists sometimes matched cases and controls with respect to possibly confounding factors (Lilienfeld and Lilienfeld 1979:10). In 1926, epidemiologists, possibly influenced by sociologists, also began to match cases and controls on confounding variables in studying diseases, a development that Schlesselman (1982:25) identified as the start of modern case-control research, though matching is not a defining element of the case-control design.

Arthur Kellermann and his colleagues (1993) applied this design to homicide victimization, looking for a link with household gun ownership. They obtained lists of persons killed in or near their homes in three urban counties and then located persons of the same sex, race, and approximate age living in the same neighborhood. After interviewing survivors of the homicide victims and the matched controls (or their proxies), they found that gun ownership was 2.7 times more common in the homicide victims' households, controlling for five other risk factors (1089). They concluded that guns kept in the home "pose a substantial threat to members of the household" and that therefore "people should be strongly discouraged from keeping guns in their homes" (1090). The conclusions were phrased in unambiguously causal terms and were not in any way qualified regarding subsets of the population to which they might apply.

This was an oddly indirect approach to the guns-homicide link since it has usually been assumed that if such a link existed it would be due to a given person's risk of homicide being raised by guns belonging to other people, mostly outside the person's household (only 7.2% of homicide victims in the U.S. in the years 1976–1992 were killed with guns by a family member, a roommate, or lover—analysis of Supplementary Homicide Reports—see Fox 1994). The narrow focus on gun ownership in the prospective victim's household made it impossible to detect the far more prevalent risks from sources outside the person's household, including risks from guns.

The association found by Kellermann et al. (odds ratio = 2.7) was repeatedly described by the authors as a strong one, but they did not cite any criterion justifying this assessment. As a rough rule of thumb, epidemiologists using case-control methods to study cancer give little weight to a risk factor discovered in a single study unless it carries a risk ratio of at least three (Taubes 1995:165; see also Lilienfeld and Stolley 1994). The Kellermann association did not even meet this minimal standard for being taken seriously, never mind any standard defining a strong association.

The association was in fact so weak that merely correcting for the modest amount of measurement error that Kellermann's own research had documented could be enough to eliminate the association altogether, if control subjects denied gun ownership at a higher rate than case subjects. Kellermann carried out a small-scale local check on the validity of responses to survey questions on gun ownership using lists of registered gun owners, a group who, by definition, had already shown themselves willing to let strangers (the legal authorities) know that they owned guns. Results indicated that even among this presumably candid group of gun owners, 11.4 percent of the known owners denied having a gun in the household, some claiming that they used to own guns but no longer did so, even though all of the sample members had registered handguns just 30–90 days earlier, while one even denied ever owning a gun (Kellermann et al. 1990). In a similar study, Rafferty and her colleagues (1995) found that 10.3 percent of hunting license holders and 12.7 percent of handgun registrants denied household gun ownership in interviews. Thus, 11 percent would seem to be a conser-

vative estimate of the level of false denial of gun ownership to be found among gun owners. Among the residents of the high-crime areas from which the case-control samples were drawn, the denial level would almost certainly be higher. (For additional evidence of underreporting of gun ownership, see Kleck 1991:455–460.)

Kleck (1997a:245, 260) demonstrated that underreporting by as little as 7.7 percent overall could render the Kellermann association nonsignificant, if there were more underreporting among controls than among cases. This would be an example of "recall bias resulting from differential recollection of past events for cases and controls," which one group of epidemiologists has identified as one of the three biggest threats to validity in case-control studies (Austin et al. 1994:75).

There was also a problem of generalizability. Few would dispute that there are some high-risk subsets of the population where gun possession might raise the risks of homicide. Kellermann et al. (1993), however, stated their conclusions without qualifying them with respect to subsets of the population to which they might apply. However, their sample (both cases and controls), was almost entirely urban, 63 percent male, 62 percent black, probably largely poor, and (given the typical geographical concentration of homicide locations) drawn almost exclusively from high crime areas of the three urban counties studied (1087). Instead of comparing homicide cases with a set of controls representative of the entire nonvictim populations of the three counties, the authors chose to obtain a set of controls matched to the cases by area of residence, sex, race, and approximate age. This eliminated any formal basis for generalizing the results to any larger population. Thus, the authors could not even generalize their claims about the risk-elevating effects of household gun ownership to the three counties from which their cases were drawn, never mind the entire U.S. population.

Another case-control study following along the same lines has been conducted (Cummings et al. 1997), but with even fewer controls for likely confounders. The most interesting finding of this study was that while homicide victimization in general was positively associated with gun ownership, the association was just as high for nongun homicide as for gun homicide. Since gun ownership should affect homicide risks, if at all, by elevating the risk of *gun* homicide (Kellermann et al. 1993), this combination of results supports the view that the homicide-guns association was spurious, due to uncontrolled confounding factors that elevate the risk of homicide victimization in general.

In sum, researchers have approached the gun-violence linkage in various ways, all of them subject to limitations. Case-control research offers another useful strategy for gaining some insight, as long as its limitations are recognized. This paper addresses its promise and its pitfalls.

A National Case-Control Study of Homicide Offending

Rather than studying homicide victimization, as Kellermann et al. (1993) did, we directly studied homicide *offending*, contrasting killers and nonkillers. The present study also improves on the Kellermann et al. study by using large (unweighted n = 13,168, versus n = 420 matched pairs) and nationally representative samples of the incarcerated homicide offender population and the general adult (nonkiller) population, allowing generalizations to legitimately be made to the U.S. adult population, and providing more stable estimates and greater statistical power to discover a guns-homicide association, as well as less vulnerability to a small number of measurement errors. Further, it encompasses all kinds of intentional criminal homicides, not just those committed in or near the victim's home, allowing us to assess the impact of gun ownership on homicidal behavior regardless of where killings might occur or where a homicide-linked gun is normally kept.

Sample

Our sample links two originally separate samples: (1) inmates in state prisons interviewed in the U.S. Census Bureau's Survey of State Prison Inmates (SSPI) in the Summer of 1991 (U.S. Department of Justice 1993a; 1993b) who had committed a homicide as an adult between 1980 and 1991 (the "cases"), and (2) a general sample of U.S. adults (age 18 or older) interviewed in the General Social Surveys (GSS) in the 1982, 1984, 1985, and 1988–1991 surveys (the "controls" [Davis and Smith 1994]). The response rate in the SSPI was 93.7 percent (U.S. Department of Justice 1993b:29) and in the GSS for the years used in this study it was 77.2 percent (Davis and Smith 1994:793–794). In the inmate sample, only those who had committed an intentional criminal homicide when age 18 or older were included, to match the age range covered by the General Social Surveys. Thus, the SSPI sample is a nationally representative sample of persons serving sentences in state prisons in 1991 for committing intentional criminal homicides while adults between 1980 and 1991, and the GSS sample is a nationally representative sample of the adult household population of the continental U.S., of whom we can be confident that over 99 percent had *not* committed homicides. By combining the two samples, we have a nationally representative sample of U.S. adults, of whom a disproportionately large share (8%) are killers—1,095 killers and 12,074 nonkiller members of the general adult population (unweighted frequencies).

Are imprisoned killers representative of all killers? In 1992, an estimated 23,760 murders and nonnegligent manslaughters were committed in the U.S., resulting in about 12,548 felony convictions, of which about 93 percent, or 11,785, resulted in a prison sentence (U.S. Bureau of Justice Statistics 1996:324, 497, 499). Thus, assuming only one person in each homicide incident actually inflicted a fatal injury, about 50 percent (11,785 of 23,760) of killers are sent to prison. We do not know how representative imprisoned killers are of all killers, mainly because we know little about the killers responsible for the one-third of homicides that are not cleared by an arrest.

The two datasets were combined by identifying all variables possibly related to homicide or gun ownership that appeared in both datasets, creating names and category coding schemes for these variables that were identical across datasets, and then merging the two sets of cases.[1]

The central research question we sought to address was: Does gun ownership increase the likelihood that a person will commit a criminal homicide? The primary dependent variable was a dichotomous one, whether the person had committed an intentional criminal homicide, and the independent variable of central interest was whether the subject personally owned a gun. For GSS respondents (Rs), the gun ownership question referred to the time the person was interviewed, while for SSPI Rs, it referred to the month before they were arrested for the killing that resulted in imprisonment.

Personal gun ownership does not completely encompass all access to guns, since some who do not personally own a gun, nevertheless, have access to guns belonging to others, either in their own household or elsewhere. The SSPI did not have a measure of household gun ownership. The logic of this research, however, depends only on the reasonable assumption that those who personally own a gun are more likely to have access to a gun than those

1. The prisoner cases were weighted by the SSPI "Final Weight" that weights them up to the entire U.S. population of inmates of state prisons in the summer of 1991, while the GSS cases were weighted by the OVERSAMP variable, which adjusts for oversampling of blacks in the 1982 GSS, and ADULTS, which adjusts for the lower probabilities of selection for individual adults living in households with more adults (Davis and Smith 1994:788–791). For all SSPI cases, OVERSAMP and ADULTS were set to one, while for all GSS cases, Final Weight was set to one. The weight for each case in the combined sample was the product of all three weights, divided by the combined weight's mean value in the combined sample. The last step insured that the weighted sample size was the same as the unweighted size, so significance tests would not be distorted.

who do not personally own a gun. In short, this gun ownership measure is a valid though imperfect indicator of a given individual's access to a gun.

Multivariate models of homicide behavior were estimated using logistic regression, the method recommended for use in analyzing case-control data (Loftin and McDowall 1988). The estimates were computed using SPSS for Windows Version 6.13 software (Norusis/SPSS 1994).

It was not possible to separately analyze risks of committing gun homicides and nongun homicides, due to inconsistencies in prisoner responses to questions in the SSPI concerning details about the killings that got them sent to prison. Apparently, inmates misunderstood some questions, or interviewers may have failed to follow skip patterns correctly. Because matching would preclude using samples representative of larger populations, it was not desirable to match cases and controls. Instead of matching on sex, race, and approximate age as Kellermann et al. (1993) did, we statistically controlled for sex, race, and exact age, along with Hispanic ethnicity, personal income, marital status, education, whether the subject resided in the South, had any children under 18, or was a military veteran.

Results

Table 2 shows the variables used in this analysis and also provides a rough picture of the bivariate associations between killing and the independent variables. Killers are slightly more likely to own guns than other adults. They are also more likely to be male, black, or Hispanic, more likely to live in the South, and more likely to be a military veteran. Killers are also younger than the rest of U.S. adults, less educated, and less likely to be married or have children. Surprisingly, killers average only slightly less income than other U.S. adults.

Table 2 • ***Variables Used in the Analysis***[a]

		Killers		*Nonkillers*	
Variable	*Description*	*Mean*	*S.D.*	*Mean*	*S.D.*
KILLER	Subject is incarcerated for intentional criminal homicide	1.00	0.00	0.00	0.00
GUNOWNER	Subject personally owns a gun	1.41	0.33	1.28	0.45
MALE	Subject is male	1.93	0.25	1.45	0.50
BLACK	Subject is African American	1.44	0.50	1.11	0.31
HISPANIC	Subject is Hispanic	1.14	0.34	1.05	0.21
AGE	Exact age of subject in years	29.36	10.24	43.91	17.44
INCOME	Midpoint of personal income category, $1,000s	16.45	16.77	16.64	17.07
MARRIED	Subject is married	1.17	0.38	1.63	0.48
EDUCATION	Highest grade completed	10.29	2.67	12.37	3.01
SOUTH	Subject resides in South	1.43	0.49	1.34	0.47
KIDS	Subject has children <18 years	1.62	0.49	1.72	0.45
VETERAN	Subject is veteran of military	1.24	0.43	1.17	0.38

Notes:

[a] For imprisoned killers, the variables describe the person at the time of their arrest on the homicide charge. For general population survey respondents, the variables describe the person at the time of interview. Means and standard deviations (S.D.) pertain to the set of cases used to estimate most of the models, i.e. those with nonmissing values on all of the listed variables. KILLER was coded 0/1 and all other binary variables were coded 1/2.

Multivariate Results—Main Effects

Table 3 shows the estimated logistic regression coefficients, and their antilogs, for multivariate models of homicide behavior. The antilogs can be interpreted as odds ratios, showing the change in the odds of a person committing a homicide associated with a one-unit increase in the associated independent variable. For example, the odds ratio for MALE indicates that the odds of a male killing are 8.54 times higher than the odds for a female, controlling for the other variables in the equation, while the MARRIED odds ratio indicates that the odds of a married person killing are only 15 percent as high as the odds of an unmarried person doing so. The unweighted sample sizes are shown, indicating that sample sizes were large enough

Table 3 • ***Homicide Model Estimates***[a]

Panel A • Main Effect of Personal Gun Ownership on Killing

Independent Variable	*b (se)*	*Antilog of b*
GUNOWNER	0.305 (.083)	1.36[b]
MALE	2.145 (.094)	8.54
BLACK	1.646 (.096)	5.19
HISPANIC	0.786 (.139)	2.19
AGE	−0.078 (.003)	0.93
INCOME	0.026 (.002)	1.03
MARRIED	−1.922 (.083)	0.15
EDUCATION	−0.329 (.015)	0.72
SOUTH	0.317 (.080)	1.37
KIDS	0.361 (.084)	1.43
VETERAN	1.003 (.100)	2.73
Constant	0.667	

Unweighted n = 7372 Model χ^2 = 5333.955 −2 log likelihood = 4881.575

Panel B • Interactions: Gun Effects within Subsamples[c]

Subsample	*n*	*Antilog of GUNOWNER Coefficient*
Full sample	7372	1.36
Males	3427	1.21 (p = .03)
Females	3945	2.54
Blacks	1378	2.02
Nonblacks	5994	1.22 (p = .03)
Age 18–30	2140	1.37
Age >30	5232	1.37
Income <$10k	4090	1.38 (p = .02)
Income >$10k	3282	1.46
Southern	2660	1.51
NonSouthern	4712	1.25 (p = .04)

Notes:

[a] All coefficients were significant at the .01 level except where indicated. *b* = logistic regression coefficient, (*se*) = standard error of coefficient.

[b] Because VET was missing for 40% of the full sample, the model was reestimated with it omitted. The GUNOWNER odds ratio became 1.41, based on 12,393 unweighted cases with valid data.

[c] For the Panel B estimates, the full model shown in Panel A was estimated, minus whatever variable was used to subdivide the sample. For simplicity's sake, however, only the antilogs of the coefficients for GUNOWNER are shown.

that even weak associations were likely to be statistically significant. Various measures of model goodness of fit are also shown (Aldrich and Nelson 1984:57).

The estimates in Panel A of Table 3 indicate that the odds of a person with a gun killing are about 1.36 times as high as the odds among persons without a gun, controlling for the other ten variables included in the model. This is only one-fifth as large an association as Kellermann et al. (1993) found with respect to homicide victimization (recalling that 1 represents no association, (2.7–1.0)/(1.36–1.00) = 4.7). Odds ratios smaller than 1.5 are regarded as "weak" in epidemiological case-control studies (Austin et al. 1994:66).

The association estimates are also sensitive to model specification. The antilog of the gunowner coefficient declines to 1.15 (not significant at the .05 level) when age is omitted, while increasing to 1.41 when veteran status is omitted. The point is not that the model is superior when either of these variables is omitted; rather, this variation merely illustrates the degree to which estimates of the guns-homicide association can be sensitive to the omission of even a single relevant control variable, something that could also be true with respect to variables we have not measured.

Multivariate Results—Interactions

Panel B of Table 3 shows the estimated gun effects within subsets of the sample, illustrating how the apparent effects vary across different subpopulations. Consistent with theory on the facilitating or "equalizer" effects of weaponry on violence (Kleck 1991:156–158; Kleck and McElrath 1991), gun ownership appears to have substantially more impact on homicidal behavior among women than men. Gun possession gives a smaller person of less physical strength the ability to inflict lethal violence on others, even larger, stronger victims, while a weapon is more likely to be redundant among male aggressors.

Guns also appear to contribute more to homicide among blacks than among whites, even controlling for likely correlates of race such as income, education, and residence in the South. Given these interaction results, it is possible that the Kellermann results did reflect a real gun effect, but one that was peculiar to the setting of their research and the composition of their sample, which was 62 percent black. On the other hand, there is virtually no difference in apparent effects for lower versus higher income persons, or younger versus older persons, and only slightly more apparent effect among Southerners than non-Southerners.

Caveats and Discussion

Uncontrolled Confounding Factors

The association between gun ownership and homicide victimization that Kellermann and his colleagues (1993) discovered, and the much weaker association between gun ownership and homicide offending found here, are at least partially spurious, attributable to uncontrolled antecedent confounding factors. Many factors known to increase the risk of homicide victimization should also increase the likelihood that persons exposed to those factors would acquire a gun for self-protection. In general, associating with dangerous persons or engaging in dangerous activities obviously raise the risks one will become a victim of violence, but these dangers are also likely to encourage some people to adapt to them by acquiring a gun for self-defense. Among the more important likely confounding factors that neither we nor Kellermann et al. controlled for are membership in a street gang and drug dealing. Callahan and Rivara (1992:3041) found that among Seattle high school students, the odds of handgun ownership were 12 times higher among youth who sold illicit drugs, and 26 times higher among gang members, than among youth without these traits. Sheley (1994:373) studied inmates in six juvenile institutions and found the odds of gun ownership to be five times higher among

those who sold illicit drugs than among those who did not. Likewise, Sheley and Wright's (1995:85) surveys of high school students in five big cities found the odds of gun ownership to be seven times higher among those who sold drugs, and three times higher among gang members, than among juveniles without those attributes (these figures are all crude odds ratios). (See also Bjerregaard and Lizotte 1995; Decker and Pennell 1995; Fagan 1990; Lizotte et al. 1994; and Lizotte et al. 1997 for similar findings). The omission of even one confounding factor with effects as large as these would be sufficient to completely account for odds ratios of 1.36 or even 2.7 (Schlesselman 1982:56).

Differential Measurement Error in Case Control Studies

Estimates of the impact of gun ownership can also be distorted by differential misreporting of ownership ("exposure" to the risk factor) across the killer and nonkiller groups (the "cases" and the "controls"). There is almost certainly underreporting of gun ownership in all surveys (Kellermann et al. 1990; Kleck 1991:455–460; Rafferty et al. 1995), including the SSPI and GSS. If the degree of underreporting were the same among imprisoned killers and free adults, this would have no impact on the gun effect estimates. If underreporting was higher among free adults, i.e., among nonkillers, this would result in an overestimate of gun effects, while greater underreporting among killers would result in an underestimate of gun effects.

Since most of the imprisoned killers committed their offenses with guns, presumably using their own guns, denying gun ownership (at the time of their arrest) in prison interviews would generally be futile. In contrast, members of the free adult population have both reasons to conceal gun ownership and a sound basis for thinking they could do so undetected.

On the other hand, although some evidence indicates that prisoners provide generally valid answers in surveys (Marquis 1981; Wright, Rossi and Daly 1983:32–38) and that general population adults frequently conceal gun ownership (Kellermann et al. 1990; Kleck 1991:455–460; Kleck 1997a:64–68, 100; Rafferty et al. 1995), one might nevertheless speculate that prisoners are simply generally more dishonest in their responses than members of the general public, and that this applies specifically to their reporting of gun ownership.

It should be stressed, however, that this is all nothing more than speculation and cannot be legitimately used to discount the empirical findings. Underreporting has, so far, only been documented in the nonincarcerated population. For example, Kleck (1997a:66–67) reported that reporting of household gun ownership in numerous national surveys has been consistently lower among married women respondents than among married men, even though household ownership levels should have been essentially identical in the two groups. At this point there is no empirical foundation for believing that response errors on the gun ownership questions have contributed to a net bias in estimates of the gun effect.

Possible Sample Bias Effects

There is, however, empirical evidence that sampling biases in the prisoner subsample related to gun ownership bias estimates of the gun effect. The SSPI sample includes only those killers who were convicted and sentenced to prison, thereby excluding those never arrested, and those arrested but not sentenced to prison. If the kinds of killers who are arrested and sentenced to prison have different rates of gun ownership from all killers, it could bias results. In separate analyses, we examined the possibility that killers who used guns were less likely to be arrested, perhaps because gun homicides are more likely to be premeditated or to involve victims who are strangers to the killer. Looking at FBI Supplementary Homicide Reports (SHR) data covering U.S. homicides from 1976 to 1992 (Fox 1994), we assumed that for any homicide where the offender's sex was unknown, the crime had not resulted in the killer's arrest, but that all other killings were cleared by an arrest. We found that an offender was

apparently arrested in 72.8 percent of murders and nonnegligent manslaughters with a gun, and in a virtually identical 72.9 percent of those without a gun. In another large body of data, concerning a more local sample, but with an explicit measure of clearance by arrest, we looked at Chicago homicides committed between 1965 and 1990 (Block, Block and Illinois Criminal Justice Information Authority 1994). These data indicated that 74 percent of gun homicides were cleared by arrest, compared to 78 percent of nongun homicides. Thus, there is little difference in gun use between solved and unsolved killings, and no reason to expect arrest patterns to bias a prisoner sample with respect to gun ownership. On the other hand, Cook and Nagin (1979:48–52) found that among those arrested, murderers who used a gun or other weapon are more likely to be convicted. And among criminals who are convicted, use of a gun in a crime is also associated with more severe sentences (Cook and Nagin 1979; Loftin, Heumann and McDowall 1983; Wright, Rossi and Daly 1983:300–307). Indeed, 49 of the 50 states have sentence enhancement laws specifically providing for more severe punishment of felonies committed with a gun (Marvell and Moody 1995:259), most of them mandating a longer prison term. Thus, Cook and Nagin (1979:51) found that among murderers who were convicted, (1) 87 percent of those who used a gun were given an incarceration sentence, compared to 83 percent of those who used other weapons and 65 percent of those who were unarmed, and (2) the average minimum sentence was 83 months for gun killers, but only 34 months for those who used other weapons and 56 months for those who were unarmed. Loftin, Heumann, and McDowall (1983) found no impact of murderer gun use on conviction or imposition of incarceration, but did find that it increased sentence length. Similarly, Wright and his colleagues (1983:304) found that while gun use only mildly increased the likelihood of conviction, gun offenders were 74 percent more likely to receive a prison sentence than those who did not use weapons. On the other hand, Lizotte and Zatz (1986) found an effect of gun use on prison sentence length only for defendants with the most serious prior records.

Since gun use increases the probability of conviction or an incarceration sentence, and may increase the length of sentence, the likelihood that gun killers would be included in a prison sample is also increased. To the extent that those who kill with a gun are also more likely to own a gun, this sample bias will contribute to an overstatement of the gun ownership rate among killers, and thus an overstatement of the estimated gun effect on killing. In sum, sample biases contribute to an overstatement of the gun effect, while there is no empirical foundation for any judgment about different rates of errors on the gun ownership question.

Taking account of the sample bias, and the omission from the model of factors with empirically established positive effects on both gun ownership and homicidal behavior, it is likely that the estimated effect of gun ownership on killing is overstated in this study. The only way it is likely to be understated is if imprisoned killers underreport their preincarceration gun ownership to so much greater an extent than the free population that the effects of both bias in the prisoner sample and the failure to control confounding factors are reversed.

Conclusions

Taken at face value, the present results indicate that gun ownership may have a weak effect on homicidal behavior in the population in general, though the effects may be stronger among women and blacks. The failure to control confounding factors that are known to positively affect both violent behavior and gun acquisition, however, is probably at least partly responsible for the positive guns-homicide association. Therefore, an association this weak could be entirely spurious. Future case-control research needs to measure and control for more potentially confounding variables, for example, gang membership and involvement in drug dealing, and to explicitly test for the possibility of differing levels of underreporting of gun ownership among cases and controls.

The present results directly address the effect of gun ownership among potential aggressors on whether those gun owners will kill. Given the strong evidence, reviewed earlier, that defensive gun use by crime victims is both common and effective in preventing injury, gun ownership by prospective homicide *victims* should be even less likely to exert a strong positive effect on homicide victimization than gun ownership by prospective offenders. In this light, the positive association between household gun ownership and homicide victimization obtained in the Kellermann (1993) study—weak yet five times as large as the association obtained herein—is most likely to be largely or entirely spurious, reflecting the common effects of risk factors such as drug dealing and gang membership on homicide victimization and on the acquisition of guns for self-protection.

Leaving the results related to gun effects, this research could serve as a model for analyzing other offenses besides homicides, allowing systematic individual-level comparisons of offenders and nonoffenders involved in other serious crimes such as rape, robbery, burglary, and drug dealing, using samples of adult offenders and nonoffenders with more claim to national representativeness than any other samples known to us. Individual-level multivariate comparisons of known offenders with nonoffenders could serve as a useful supplement to self-report surveys, with their attendant validity and sampling problems, in testing hypotheses concerning the etiology of criminal behavior.[2]

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2. Some ancillary findings are also worth noting. Students of the Southern subculture of violence thesis have commonly studied either (1) violence rates of macro-level units, such as states, across regions, while calling for more research at the individual level of analysis, or (2) survey data on individuals that measured cultural traits, but that did not allow a link to be tested between those traits and serious violent behavior (Ellison 1991). Our results supplement these approaches by linking individuals' region to homicidal behavior. They indicate that Southerners are more likely to kill, controlling for age, sex, race, income, and many other correlates of violent behavior. This is, to our knowledge, the first research to establish that multivariate association using individual-level data and a nationally representative sample.

Likewise, those interested in the "violent veteran" thesis (Archer and Gartner 1976) might be interested to note that our findings indicate that the odds of a military veteran committing a criminal homicide appear to be about 2.7 times as high as the odds for nonveterans. The reason for these associations deserve further exploration.

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