

Using Speculation to Meet Evidence:  
Reply to Alba and Messner

Gary Kleck

In 1995, the *Journal of Quantitative Criminology* published a sharply critical analysis of Gary Kleck's book *Point Blank*. Professor Kleck wrote a lengthy response, of which the *Journal of Quantitative Criminology* published only a small part. The following article is Kleck's full reply, which has not appeared in print elsewhere. Professor Kleck revised the article in February 1997, for publication in the *Journal on Firearms and Public Policy*.

## 1. Introduction

For the first time in the history of the *Journal of Quantitative Criminology* (*JQC*), editors John Laub and James Fox decided to publish an article delivering a consistently one-sided attack on a single book (Alba and Messner 1995a). This extraordinary treatment was reserved for my book, *Point Blank* (Kleck 1991; hereafter *PB*). As the man in front of the firing squad remarked, "If it weren't for the honor of the thing, I'd decline." Apparently the fact that 7% of the pages in *PB* (pp. 191-201, 216-22, 394-408, 419-425, for 36 of 512 total pages) were partially based on an analysis reported in *JQC* provided the editorial rationale for this unprecedented decision (telephone conversation with John Laub).

The category into which the Alba and Messner (hereafter A-M) article falls is an odd one, in that it was neither a report of new empirical evidence contradicting my conclusions nor a (very belated) book review. This meant that A-M were neither obliged to affirmatively present new evidence, as would the authors of an empirically based critique, nor to provide a balanced and minimally representative overview of the contents of the book, as would the authors of a book review. Since there are no minimal standards or rules A-M were bound to respect, they were effectively given license to write anything they liked. Their rather indiscriminate attack followed the strategy "throw up enough mud and some of it is bound to stick." (Or perhaps the expectation that readers would assume that "where there's smoke, there must be fire" is the relevant cliché). If enough pages are devoted to the attack, maybe no one will notice how little affirmative support there is for the charges.

The novelty of the article's format liberated A-M from any customary rules about what they were obliged to do, or forbidden from doing. To every criticism, they have a convenient reply: "We were under no obligation to do that (or to refrain from doing that)." If I point out the unbalanced, unremittingly negative, often *ad hominem* character of their article, they can reply "Why should we be even-handed? This is not a book review." Likewise, if I note that their critique relies largely on one-sided speculation unsupported by empirical evidence based on technically better research, they can reply "So what? We did not claim to be writing an empirically based critique." Heads they win, tails I lose.

Before disposing of the individual A-M charges, it is worth taking this opportunity to point out what seems to have escaped the notice of both A-M and other critics of *PB*. If there is bias in *PB*, it is in exactly the opposite direction from that which pro-control critics profess to have identified. The evidence in favor of the pro-control position was consistently so weak that I felt obliged to bend over backwards to give that position every benefit of the doubt. This sort of thing apparently did no good with A-M, who characterize me as "too one-sided" (in an anti-control direction). Their own ideologically crippled vision rendered them incapable of recognizing just how often I held open pro-control possibilities in the face of overwhelmingly anti-control empirical evidence.

Consider just a few of the more noteworthy instances. (1) I repeatedly stressed the possibility that more finely-tuned measures that definitively isolated gun availability among criminals might show a net positive effect on crime rates, despite the absence of any affirmative evidence of such an association, and despite the fact that the best available evidence, using less specific measures, showed no net effect (*PB*, p. 203). (2) I gave comprehensive consideration to even minimally plausible links between gun possession and suicidal behavior, despite an almost complete lack of empirical support for most of them (pp. 239-246). (3) I drew conclusions in favor of the hypothesis that some gun laws reduce violence rates, even though supportive hypothesis tests were no more frequent than could have been produced by chance alone (pp. 398-405; more on this later). (4) And in the concluding chapter, I made a long series of pro-control policy recommendations based on a rather optimistic reading of very thin evidence (pp. 431-444). The only

critic who seems to have noticed these indications of pro-control bias is, not surprisingly, the Research Coordinator of the National Rifle Association (Blackman 1993).

To be charitable, perhaps this is not attributable just to the critics' ideological blind spots. Perhaps A-M, not having reviewed the entire body of empirical evidence themselves, simply do not understand how one-sided, in an anti-control direction, the technically sound evidence on this issue really is. There is such a huge volume of truly dreadful research in this area, that it is easy enough to be honestly misled by the sheer volume of research supposedly supporting pro-control conclusions. Perhaps because of their unwillingness to seriously consider the possibility that the results of competent research really are this one-sided, A-M assume that my reading of the evidence must be attributable to my supposedly one-sided, anti-control biases.

Students of mass media information manipulation techniques will recognize some of the propaganda techniques used by A-M. They began by marginalizing my conclusions, by describing them as "controversial." If this word connoted nothing more than the obvious fact that some gun control supporters, such as Handgun Control member Alba (and presumably Messner, who does not, however, share his gun control views with readers) disagree with my conclusions, there would be reason neither for me to object nor for A-M to bother making the statement. However, use of such a word also serves the illegitimate function of marginalizing my conclusions by insinuating that they are somehow less than respectable, and hinting that they are the views of a deviant minority. For what it's worth, among scholars who specialize in studying guns and violence, my conclusions are squarely in the mainstream. Consistent with my findings, most scholars have found no impact of gun laws on crime rates (*PB*, p. 417), and half of the admittedly not very good previous studies of the subject have found no net impact of gun levels on crime rates (*PB*, pp. 214-5).

Because readers are not likely to divine the actual content of *PB* from A-M's distorted and selective presentation, it is necessary for me to briefly summarize some of my key conclusions, so that readers can anticipate in which direction I would bend interpretation of evidence, were I inclined to do so. Some of my key conclusions are the following. (1) Guns in the hands of noncriminal prospective crime victims *may* deter some criminals from committing some crimes, and actual defensive gun use by crime victims reduces the likelihood of injury or property loss to the victim (*PB*, Ch. 4). (2) Guns in the hands of criminals may facilitate some types of crimes, and increase the lethality of wounds inflicted, but also reduce the likelihood that the criminal will attack and injure his victim (Ch. 5). (3) On the other hand, aggregate measures of gun ownership, which seem on their face to reflect gun availability among criminals, do *not* indicate any net positive effect of gun ownership levels on crime rates, including homicide (Ch. 5). (4) Nevertheless, since there is no net social benefit to criminals having guns, a generously pro-control reading of the evidence implies that policies that are aimed at reducing gun availability among criminals, but that do not reduce gun possession among noncriminals, seem advisable. (5) Most gun laws have no net impact on most violence rates, but some laws apparently do affect some violence rates and might be worth implementing, where they are not already in place. Potentially effective measures would include background checks on prospective gun buyers (like the Brady Act, but extended to cover all gun types and both private and commercial transactions) and well-enforced laws banning unlicensed carrying of guns in public places (Ch. 11).

My policy recommendations could probably be regarded more as banal than controversial, since they amount to supporting the gun control status quo. They correspond closely to the general themes of existing American gun control: do what can be done to keep guns from criminals, but without denying them to noncriminals (Ch. 8). Public opinion polls likewise indicate that solid majorities of Americans support moderate regulatory controls but oppose prohibitionist measures that would deny them access to guns (Ch. 9). What is so remarkable about A-M's remarks is that they appear to believe that my views are off the mainstream. Unless one defines the mainstream as being the opinions of the 1/10th of 1% of the population that belongs, as does Richard Alba, to Handgun Control, Inc. (or to the American professorate), my conclusions could scarcely be more boringly middle-of-the-road.

Given the banal nature of my policy recommendations, and A-M's coyness about their own policy preferences, I can only speculate that what A-M were really perturbed by was that (1) I have not recklessly endorsed prohibitionist controls, as some scholars such as Franklin Zimring and Marvin Wolfgang have publicly done (Newton and Zimring 1969, pp. 143-4; *Time* 7-54-68, p. 6), and (2) I made the unforgivable mistake of pointing out that many Americans effectively use guns for self-protection, and concluded that this fact should be given significant weight in policy deliberations on gun control.

### *An Ounce of Evidence Outweighs a Ton of Speculation*

The A-M exercise does serve one useful purpose: illustrating the illegitimate but altogether too common practice of treating speculation as if it can rebut or outweigh empirical evidence. Speculation has a useful role as a motivator of efforts to gather more and better evidence. Used by itself, however, it lends itself to one-sided

confirmation of the user's own biases. For A-M, no pro-control speculation is too feeble, far-fetched, or unsupported to merit mention, and no pro-control evidence too technically flawed to be cited.

A-M speculated that my models of violence rates are not adequately specified (pp. 395-396), speculated that the models are underidentified (A-M forthrightly tell readers that this is "a definite possibility" (p. 396), speculated that the instruments used to identify the models are inadequate to achieve the rank condition for identification (p. 396), speculated that there may be biases in National Crime Victimization Survey (NCVS) data that distort my findings regarding effects of guns in the hands of aggressors (p. 398), speculated that differences in fatality rates between gun woundings and nongun woundings would not disappear if offender motivation were controlled (p. 401), speculated about the "potentially serious" limitations of surveys indicating large numbers of defensive gun uses (p. 403), speculated that criminals are not deterred from committing crimes by the possibility of confronting an armed victim, but rather are "merely displaced to more vulnerable targets" (p. 404), speculated that availability of guns "can be a key factor underlying the emergence of criminal motivations" (p. 405), and so on, ad nauseam, for 20 evidence-free pages.

All of these speculations were made without a shred of empirical support, often in the face of directly contradictory empirical evidence. Indeed, that is the beauty of this sort of attack: since the critics provide no evidence of their own, there is no way for it to be shown that their evidence is less technically sound than the evidence on which their target's conclusions were based. Thousands of imaginable possibilities *might* be true, but this is not the same as saying they *are* true. Distinguishing the two is what doing research is for.

A-M's speculations were invariably, without a single exception, directed at undercutting evidence with anti-control implications. Had the speculations been a bit less one-sided and less myopically ideology- and policy-driven, they might have helped promote a search for the truth. However, when speculations about flaws in research are invariably made only in a direction which challenges views contrary to the critic's preconceptions, the only possible result of such a fruitless exercise is confirmation of the biases with which the critic began. Until it is linked with research which improves on the targeted work, this sort of unproductive one-sided speculation is better confined to cocktail party chat than to the pages of scholarly journals.

A-M have not, to my knowledge, bestirred themselves to apply such a searching critique and such high technical standards to the voluminous and far less technically sound research that has been used to support pro-control conclusions. For example, while speculating that my violence rate models were underidentified, they did not mention that of 12 pre-1991 studies findings a significant positive association between gun ownership levels and violence rates, 10 did not even make an effort to model a possible two-way relationship, and thus all of these studies relied on models which were indisputably underidentified, assuming there actually is such a reciprocal relationship (PB, pp. 214-215). If there is not, of course, A-M's concerns about identification are simply irrelevant.

Rather than comparatively assessing which of a large number of studies offer the technically strongest evidence on a given point (as I did in *PB*), A-M selectively applied stringent methodological standards only to research yielding findings that cast doubt on pro-control positions. If everyone were to apply this sort of literature review practice, it could lead to only one outcome: ratification of the reviewer's preconceptions and, on a collective level, reinforcement of whatever political positions on a given issue are most popular with scholars in the field.

The rest of my remarks will be devoted to point-by-point refutations of A-M's claims. To help orient readers, I will use the same section headings used by A-M.

## **2. General Levels of Gun Ownership and Rates of Criminal Violence**

A-M questioned my city-level findings that gun prevalence levels show no net impact on rates of criminal violence, based on two alleged problems: (1) supposed inadequacies in measurement of gun availability, and (2) supposed underidentification of the statistical models of violence rates that I employed.

A-M claimed that there was some inconsistency in how I interpreted my aggregate measures of gun prevalence, supposedly waffling between whether they reflect criminal or noncriminal possession of guns (pp. 394-395). This is false. In both *PB* and in the related *JQC* article (Kleck and Patterson 1993), I consistently maintained the exact same, very unambiguous stance: criminal and noncriminal gun ownership can be distinguished, both theoretically and empirically, at the individual level, but currently available data do not permit us to distinguish them at the aggregate level. I further maintained the view that, at the aggregate level, since the two gun prevalence rates seem to be so highly correlated, it may not even matter much whether we make this distinction empirically. Even if one only could separately measure gun prevalence in the general, largely noncriminal, public, it would serve well as a surrogate for relative gun prevalence levels among criminals, and vice versa.

A-M claim that "at various points" in *PB* I implied that there was no strong correspondence between gun possession by law-abiding citizens and that by criminals (p. 394). This is false. A-M do not cite any pages where I

implied any such thing, for the simple reason that there are no such pages. My comments on the strong aggregate-level correspondence between criminal and noncriminal gun prevalence levels could not have been any clearer. For example, I aggregated individual-level survey data from a large number of surveys, for nine regions, and reported in *PB* that “survey-measured ownership among self-reported arrestees and among nonarrestees are highly correlated with each other, indicating that *criminal and noncriminal ownership are highly correlated*” (*PB*, p. 195, emphasis added). Likewise, in the *JQC* article, Britt Patterson and I wrote that “we interpret the gun index as an indirect measure of gun prevalence among criminals,” and then went on to say that “at the city level it is doubtful whether the two can be distinguished, as we suspect they are highly correlated” and that “our indicators probably necessarily serve as indicators of noncriminal gun prevalence, as well as gun prevalence among criminals” (Kleck and Patterson 1993, p. 263). In short, the statements in both sources were internally and mutually consistent in interpreting the macro-level gun prevalence indices as reflecting both criminal gun prevalence and noncriminal gun prevalence, which are probably so highly correlated at aggregate levels that they cannot be distinguished. A-M assert that “Kleck seems initially unsure whether these gun-supply indicators better reflect criminal or non-criminal possession of guns” (p. 395). This is a red herring. It does not matter which of these concepts the indicators “better reflect” if they are so highly correlated that a measure of one could serve as a measure of the other. As noted in both *PB* (p. 195) and the *JQC* (p. 264) article, the aggregate indicators were positively and significantly correlated with survey-based gun ownership levels among both self-reported arrestees and among nonarrestees, indicating that they serve as indicators of both criminal *and* noncriminal ownership.

It was only by wrongly assuming that the indicators must reflect only one or the other that A-M managed to conclude that there are problems here. They claimed that if the indicators “reflect primarily criminal gun possession, then they are inadequate for assessing the causal effect of crime on the level of gun ownership” (p. 395). This is wrong. Contrary to A-M’s simplistic assertion that “this effect presumably involved the purchases of gun for defensive purposes by the general non-criminal population” (p. 395), criminal gun ownership is at least as likely to be responsive to crime rates as noncriminal ownership. This alone would justify the modeling of possible reciprocal causation between gun ownership and violence levels: if there is a two-way relationship to be modeled, it is *especially* likely to exist among criminals.

A-M then asserted (p. 395) that “If ... the linkage between the use of guns in crime and the supply of guns in the general population is so tight that criminal gun use can be used to measure the total gun supply ... , then a central tenet of the case in favor of gun control is established: where guns are widely available, they are more frequently used for criminal purposes.” Depending on how one interprets the meaning of these words, this statement is either a *non sequitur* or is irrelevant to the pro-control case.

It is correct that where noncriminal gun ownership is higher, criminal gun ownership is also higher, and that where criminal gun ownership is higher, the *percent* of crimes which are committed with guns is often higher. If, however, A-M were saying that higher gun prevalence in the general population causes higher total rates of violence (e.g., higher total homicide rates), my research directly tested this claim, and did not support it (*PB*, Ch. 5).

On the other hand, if all A-M meant was that a higher *percentage* of crimes are committed with guns where guns are more widely available, the assertion is sometimes true but is not “a central tenet of the case in favor of gun control.” People killed with knives are no less dead than those killed with guns. Thus using gun control to, for example, reduce the gun homicide rate without reducing the total homicide rate would be worthless.

A-M complained (p. 395) that the specifications of exogenous variables “shift inexplicably” from one model to another, but then in the next sentence admit that “it is appropriate for the predictors ... to differ in each model.” This does not prevent A-M from pointing out, as if it were a flaw, differences in the sets of exogenous variables included in various models, nor did it deter them from labeling appropriate differences across models “troubling inconsistencies.” A-M may well have been troubled, but the explanation of why a given variable (e.g. Southernness) that was strongly related to one violence rate was omitted from other models is hardly mysterious. If such a variable was not significantly related to some other dependent variable, it would be dropped from that variable’s model because there was no empirical justification for retaining it. Instances where insignificant variables were retained in models were due to either strong *a priori* theoretical justification for considering them causally relevant, or to the need to maintain comparable models across total, gun and nongun versions of a given violence rate model.

A-M did their best to make much of a handful of minor differences in specification between the models in *PB*, estimated with LISREL structural equation procedures, and the models in the *JQC* article, estimated with two-stage least-squares. They raised a red herring by asserting that differences in estimation method “should not affect the substantive logic underpinning the models.” Of course it should not. Such differences can, however, alter the estimates of coefficients, and thus the empirical basis for retaining or omitting variables. There would be no point to using a

different estimation procedure if it always yielded the same results as other procedures. Consequently it was perfectly reasonable for LISREL and 2SLS estimates to sometimes differ, and for these differences in estimates to motivate differences in specification.

In any case, we had explicitly informed readers that these specification differences were unlikely to influence estimates of gun control law effects, since the exogenous control variables were so weakly correlated with the gun law variables (Kleck and Patterson, pp. 260-1), and thus A-M knew of this. Despite this knowledge, and without any new information to contradict our reports of low correlations, A-M proceeded as if the specification of control variables *were* a crucial matter.

It is far-fetched to hint, as A-M implicitly do, that criminological theory is adequate to completely specify aggregate crime rate models. Therefore, empirical estimates are obviously crucial to specification decisions. It is worth asking at this point whether A-M have ever applied such bizarre standards to any other research. Are there any extant examples in the criminological literature where it could seriously be argued that crime rates were successfully modeled entirely on the basis of criminological theory, without any reference to empirical estimates?

A-M speculate that some of my models were underidentified (p. 396). Their assertion that I used significance tests rather than theory to “eliminate” (by which I assume they meant “exclude”) exogenous variables from models is either irrelevant or false. It is irrelevant to identification whether *some* of the exclusions were based on statistical tests. Rather, what is crucial is whether *enough* exclusion decisions were based solely on *a priori* theoretical considerations. On the other hand, if A-M meant that *all* of my exclusion decisions were based on statistical tests, the claim is false. There were enough exclusions made solely on *a priori* grounds to achieve identification. Specifically, it was argued, *a priori*, that the subscription rate to gun-related magazines and the hunting license rate could have no direct effect on violence rates, independent of their effects on gun ownership, but that they did have a direct causal effect on gun prevalence.

One can quibble with the assumptions underlying these exclusion restrictions, but there is no doubt that the rank condition for identification was met if those assumptions are accepted. (The motivated reader may apply Maddala’s identification procedures [1992, pp. 363-366] to determine this for themselves.) If A-M care to provide new evidence that interest in recreational applications of guns *does* have a direct causal effect on violence rates, or does *not* have a direct causal effect on gun ownership, I would be happy to hear about it. In the meanwhile, they have no empirical basis for questioning identification, so here again their criticisms amount to unsupported speculation.

A-M tried out some red herrings here as well. They noted that gun magazine subscriptions and hunting license rates are “*more attuned*” to gun ownership in small cities or rural areas than in big cities (p. 396, emphasis added). Undoubtedly they are. This is, however, irrelevant to the identification issue. It is sufficient that these variables exert a direct effect on gun ownership in big cities, whether or not they may have even more effect on gun ownership elsewhere.

A-M report that they found it “hard to imagine that these instruments have the posited casual connection to gun ownership in cities like New York” (p. 396). The fact that hunting is not done *in* large cities does not mean that the residents of such cities do not hunt elsewhere and do not own guns, kept in their big-city homes, for such purposes. Nor does the fact that crime-related motives for gun ownership dominate in cities imply anything about whether recreational motivations for gun ownership do not also operate. The 1987 General Social Survey indicated that, among residents of cities of 100,000 population or larger (i.e. in cities of the size covered in the *PB* analysis), 30% of the gun owners hunt (Davis and Smith 1987). Thus, a very large share of urban gun owners hunt. Since there is no basis whatsoever (other than one-sided speculation) for doubting that these individual-level connections also operate at the city level, the case for believing that higher levels of interest in recreational shooting would motivate higher city levels of gun ownership remains unchallenged.

By the end of their Section 2, A-M had promoted their musings about hypothetically possible problems in my models from the status of idle speculation to that of actual flaws. Indeed, they had even determined, by methods which they did not share with their readers, that my analysis was afflicted by “*severe* model specification” (p. 397, emphasis added). In fact, they failed to show *any* misspecification, never mind “*severe*” misspecification. It would surely be misleading to describe as “*severe*” flaws whose correction would not even alter a single substantive conclusion. A-M provided no evidence that correcting any of the “flaws” that they imagine afflicted my analyses would necessitate any substantive changes in my conclusions.

If all A-M were saying is that specification errors and underidentification are hypothetical possibilities with these models, they need hardly have bothered, since such problems are *always* a possibility with complex structural equation models. On the other hand, if they believed that they had demonstrated actual flaws of this type, they were mistaken.

Here, as they do throughout their article, A-M treated speculation as equivalent, and sometimes even superior to, empirical evidence. How else can one interpret their willingness to dismiss a large body of sophisticated empirical findings on the basis of nothing more than the *possibility* that the findings might have been influenced, to an unknown degree, by flaws whose very existence they could not document?

What was most extraordinary here was that A-M were oblivious to how my findings were most likely to be distorted if my models really were underidentified. I know of no scholars in this field who disagree that violence rates have a positive effect on gun ownership levels. The key unresolved issue was whether there is also a net positive causal effect running in the other direction, from guns to violence. If one failed to properly model this possible two-way relationship (or ignored it altogether), the result would be that one would confuse a supposed positive effect of gun levels on violence rates with the positive effect of violence rates on gun levels. That is, other things being equal, underidentification would tend to *favor* the guns-cause-violence argument. Thus, if A-M's speculations that my models were underidentified are correct, it means I have erroneously given too much support to the argument in which they so fervently believe, that higher gun ownership levels cause higher violence rates.

### 3. Offender Gun Use and the Lethality of Incidents

A-M expend a good deal of space repeating (pp. 398-9) a caveat I had already stated in *PB* (p. 176), without adding anything to what was only a speculation even when I stated it. While the flaw I warned about may exist, there is no evidence that it would be sufficient to completely account for the observed association between offender gun possession and lower injury rates. Whether the flaw actually exists and distorts these findings enough to alter my conclusions remains to be seen.

A-M did, however, create the impression, by exclusion of relevant information, that the basis for my conclusions is far thinner than it really is. Throughout *PB*, I drew conclusions based not only on my own research, but also on the accumulated body of knowledge produced by other scholars. The reader of A-M's article would get no hint of this. For example, I concluded that aggressors armed with guns are less likely to attack and injure their victims than otherwise similar aggressors without guns (*PB*, Ch. 5). A-M discussed this conclusion only in connection with my own analysis of NCVS data (p. 398). This is misleading because my conclusion was based on a long series of prior studies as well as my own data analysis. All of the studies, regardless of data source, analytic methods, crime type studied, or nation where the study was conducted, yielded the same conclusion: aggressors armed with guns are less likely to attack and injure their victims than aggressors not armed with guns (see the nine studies cited on pp. 161-162 of *PB* or a more complete list of 16 such studies in Kleck 1995a, p. 22).

In their Part 3, A-M did identify a single genuine mistake on my part. I misinterpreted the meaning of a linear probability coefficient as indicating a 1% *relative* change in the risk of death associated with the offender's possession of a gun, rather than as an absolute one percentage point change in the risk, thereby understating the actual association. Correcting this misinterpretation strengthens my policy recommendations favoring background checks and other measures intended to disarm criminals, while having no bearing on whether disarming noncriminals would be harmful. I appreciate Alba and Messner pointing this out, and will revise the relevant conclusions in future publications.

After making this accurate point about the guns-death association, A-M immediately jumped to a *non sequitur* conclusion, that "the presence of guns, in short *makes* incidents much more lethal *than they otherwise would be*" (p. 11, emphasis added). Thus, A-M draw an unambiguously causal conclusion from this statistical association. Despite having read a long detailed explanation (*PB*, pp. 163-170) on why this inference is illegitimate, A-M nevertheless drew the *non sequitur* conclusion, without one whit of justification or rebuttal to my arguments and evidence.

Neither A-M's rudimentary computation of the bivariate association between gun use and death rates, nor my somewhat more sophisticated multivariate probit analysis, can tell us whether the presence of guns "makes" fatal outcomes more likely, i.e. contributes causally to victim death, independent of the effects of the greater lethality of the aggressors who choose to use guns rather than other weapons. Even strongly pro-control authors such as Zimring and Cook have conceded that aggressors who use guns are likely to be, on average, more lethal in their intentions than persons who attack others with their fists or a knife (e.g. Zimring 1972, p. 107; Cook 1982, p. 248). Thus, until we can explicitly measure and control for aggressor strength of motivation, we cannot isolate how much of the guns-death association is attributable to the attributes of the weapon itself. It should be stressed that this is not an *ex post facto* argument developed to respond to A-M, but a point that was made in *PB* (p. 166).

A-M simply sidestepped this entire issue by once again resorting to their favorite strategy, the unsupported speculation. They wrote that they doubted that controlling for motivation could make an association so large disappear, because this would fly "in the face of [their] experience with multivariate analysis" (p. 12). While I cannot speak to the representativeness of A-M's experiences, there is no formal reason whatsoever why introduction of a single control

cannot make even very large bivariate associations disappear--it merely requires a control variable sufficiently strongly associated with the original correlates. Neither A-M nor I know how strong the association is between aggressor motivation and either weapon choice or the likelihood of victim death.

Interestingly, *PB* includes at least two empirical examples of large bivariate guns-violence associations that *do* largely or entirely disappear with the introduction of a single control. First, I noted that both homicide rates and gun ownership rates are far higher in the U.S. than in Japan, a two-case “association” that is often cited as evidence that higher gun availability partly causes the higher U.S. homicide rate. However, I demonstrated that this entire “association” not only disappears, but actually reverses itself when one simply “controls” for Japanese descent: Americans of Japanese descent, who live in a nation awash with guns, actually have a lower homicide rate than residents of Japan, who live in a country with virtually no guns (*PB*, p. 189). Even more pertinently, a medical study indicated that almost all of the association between weapon and victim death disappeared once one controls for the body location and type of the wound. Wilson and Sherman (1961) controlled for these factors through the simple device of studying only “penetrating wounds of the abdomen” (p. 643). They found that the mortality rates within this relatively homogenous set of woundings were 16.8% for pistol wounds and an only slightly lower 13.3% for ice pick wounds. Notwithstanding A-M’s vast “experience with multivariate analysis” (none of it, to my knowledge, done on the topic at hand), there is no logical or empirical reason to consider it implausible that the guns-death association could disappear once aggressor motivation was controlled.

A-M concluded Section 3 of their article with an argument which attempted to derive, through a very indirect chain of weakly coupled premises, a conclusion that has already been rejected using far more direct empirical tests. They began by asserting that where general public gun ownership is higher, gun use in crime is higher. If “gun use in crime” merely refers to the *share* of crimes that are committed with guns, this statement is true; if “gun use in crime” refers to the per capita rate of gun crimes, it may or may not be true (evidence is mixed-*PB*, Ch. 5). A-M then appeared to derive from this assertion the claim that where general population gun ownership is high, “guns frequently substitute for other weapons in the commission of crime.” Either A-M meant the second assertion as nothing more than a rephrasing of the first one, or the second statement is a distinct statement with no clear logical link with the first assertion. They then proceeded to the logically unconnected conclusion that “where guns are frequently used in crime, the homicide rate is higher.” Assuming that they intended this as some sort of causal assertion, it is still unclear how this implies that a higher gun supply leads to a higher homicide rate. The per capita rate of gun crime (“where guns are frequently used in crime”) is not itself a measure of the gun supply, and also has a tautological link with the homicide rate, since gun homicides constitute a share of both the gun crime rate and the homicide rate.

To be charitable, perhaps A-M were simply clumsy in their phrasing, and intended the following syllogism:

(1) Higher rates of gun ownership in the general public cause higher rates of gun use by aggressors in crime incidents.

(2) Higher rates of gun use by aggressors in crime incidents cause a higher fatality rate in crime incidents.

(3) Therefore, higher rates of gun ownership in the general public cause higher homicide rates.

As previously noted, we do not in fact know if gun use has a net positive causal effect on the fatality rates of assaults, since we cannot separate weapons effects from the effects of aggressor motivation. Thus, we do not know whether premise (2) is true. Premise (1) is reasonable, and probably true in some circumstances, though we cannot be sure because no one has been able to separately measure criminal and noncriminal gun ownership at the aggregate level. This syllogism, however, would be invalid even if both of its premises were true. The reasoning does not lead to A-M’s conclusion unless one makes an additional unstated premise: that the supposedly fatality-increasing effects of gun use by offenders in crime is the *only* impact that gun availability has on homicide rates. Not only are such effects *not* the only impact of general gun availability on homicide, they are not even necessarily the most important ones. Higher gun availability also implies higher victim defensive use of guns, which reduces the probability of victims being injured, and thereby contributes to reducing the homicide rate (*PB*, Ch. 4).

As to what the net balance of these opposite-sign effects on the homicide rate is, my research directly assessed this. Gun prevalence levels showed no net positive effect on the homicide rate (*PB*, p. 221, Table 5.15, top panel, left column; Kleck and Patterson 1993, p. 267, left column). Indeed, the associations are negative. A-M’s very indirect and incomplete chain of reasoning, built up out of dubious premises, is a poor substitute for a direct empirical test of their conclusion. Given their failure to identify any actual flaws in the analysis behind my direct test, it remains as the best information we have on the issue.

#### 4. Defensive Gun Use and the Deterrent Effects of Gun Ownership

In Section 4, A-M tried to resuscitate the now rapidly fading notion that defensive use of guns is rare. In this

instance, A-M's critique is second-hand, as well as purely speculative. They repeat Philip Cook's highly selective and speculative critique of the surveys that indicated large numbers of Americans use guns defensively (hereafter, the "gun use surveys"). Cook speculated that these surveys were afflicted by "telescoping," an assertion which is very likely true but of little consequence for estimates (Kleck and Gertz 1995). Neither Cook nor A-M provide any evidence or even reasoning why the estimate-elevating effects of telescoping would exceed the estimate-lowering effects of respondent underreporting. Elsewhere (Kleck and Gertz 1995), I have cited actual empirical estimates of the relative sizes of these countervailing errors in victim reporting of crime experiences, and noted that telescoping effects are roughly cancelled out by recall failure.

Further, my own recent (1993) national survey, conducted with Marc Gertz, was designed specifically to estimate the prevalence and incidence of DGU, and has strongly confirmed the *PB* conclusion that such events are very common. Indeed, the findings indicate that DGUs are two to three times *more* common than criminal uses (Kleck and Gertz 1995). A-M also failed to inform readers that *PB* (p. 146) lists *eight* previous surveys that all indicated very large numbers of defensive gun uses, instead only vaguely referring to "surveys" (p. 14). Including post-*PB* studies, there are now a total of at least fourteen surveys indicating huge numbers of defensive uses, all of them yielding estimates at least eight times larger than those derived from the NCVS. One might be tempted, in the light of such near unanimity, to describe the evidence in favor of the claim of large numbers of defensive uses as overwhelming, but A-M resist the temptation.

A-M cite a book review to the effect that "methodological concerns have been raised" about my estimates of civilian justifiable homicides. In fact, the reviewer in question (Sherman 1993) did no more than speculate that maybe national data might indicate something different from what local studies have indicated. He did not demonstrate this to be so, nor did he identify any flaws in my methods, other than to note that I did indeed use studies of local samples, in the absence of any similarly detailed data at the national level. Thus, A-M are so anxious to cite anything that sounds like a rebuttal to my evidence that they resort to citing the personal opinions and speculations of other people, by way of buttressing their own personal opinions and speculations.

Then they cite the conclusions of McDowall and his colleagues (1991) to the effect that their analyses "do not support the idea that publicity about gun ownership measurably deters criminal behavior." In fact, these authors' tests *did*, without exception, support just such a conclusion. As I have explained elsewhere (Kleck 1995b), the *non sequitur* conclusions of McDowall et al. were based on their inappropriate use of low-power significance tests, sometimes in circumstances where it was impossible for even the strongest deterrent effects to pass the tests. Leaving aside the irrelevant significance test results, both the direction and magnitude of associations uniformly supported my hypotheses. Nevertheless, conclusions about deterrent effects will always be less firm than those pertaining to either disruptive effects or the frequency of defensive uses.

A-M misstated my conclusions regarding defensive gun use, claiming that I concluded that, under gun control measures applied to the general public (by which I assume A-M meant prohibitionist controls), "the positive deterrent effect of routine gun ownership would be decreased more than would the crime-causing effect of criminal gun ownership" (p. 403, citing pp. 144-145 of *PB*). I did not draw such a conclusion, either on pp. 144-145 or elsewhere. I remain fairly agnostic about the existence of *deterrent* effects of gun ownership, or any net reduction in crime attributable to criminals refraining from attempting to commit some crimes, due to fear of gun-armed victims.

The evidence is much stronger concerning the *disruptive* effects of actual defensive use of guns by victims once a criminal has made an attempt to commit a crime. The distinction between deterrent and disruptive effects was made very clear on p. 122 of *PB*. A-M could scarcely have misunderstood this distinction since they explain it in their footnote 8. The unwary reader might be misled into thinking that this footnote identifies a conceptual error on my part, since it leaves the impression that I failed to make this distinction in *PB*, an idea that is encouraged by A-M's citation of someone else to make this differentiation. In fact, A-M are simply repeating here a distinction that I had already made in both *PB* (p. 122) as well as in earlier work (Kleck and Bordua 1983; Kleck 1988, p. 9).

The conclusion that I actually drew about the relative balance of benefits and costs was that prohibitionist controls, because they would almost certainly reduce noncriminal gun ownership more than criminal gun ownership (a premise A-M concede--p. 405), would produce a relatively larger reduction in the beneficial effects (of *any* kind--deterrent, disruptive, or otherwise) of defensive gun use by noncriminals than in the harmful effects of criminal uses. This is an important distinction because A-M go on to misconstrue the issue of weighing benefits and costs as if the "deterrent effect" of guns in the hands of victims had to outweigh the "crime-inducing effect of gun in the hands of criminals" (p. 405), when the issue really concerns weighing *any* beneficial effects of victims having guns against the harmful effects of criminals having them.

A-M also misconstrue the issue in assuming that the effects of guns in the hands of victims would have to be

weighed against the effects of guns in the hands of criminals. No such tradeoff is involved if one is considering policies that are aimed at disarming only criminals, since there is then no cost linked to efforts to disarm prospective (noncriminal) victims. It may be diagnostic of A-M's own unstated policy preferences that they automatically assumed that consideration of such a tradeoff was inherent in the gun control debate--the tradeoff is relevant only if one thinks of "gun control" as meaning gun bans.

A-M's confusion on this point continued when they stated that the material in Chapter 4 (concerning defensive gun use) cannot resolve the issue of the "balance sheet" of positive and negative effects of guns and their net contribution to criminal violence. Indeed, the evidence in Chapter 4 obviously could not resolve the matter by itself, since it addresses only one side of the "balance sheet": defensive gun use. Nor did I claim it could. Rather, it is the material in Chapter 5, concerning my city-level analysis, which is pertinent to this question (a point that A-M concede, but marginalize by burying it in a footnote [footnote 9, p. 405]). It is there that I report findings indicating that the net effect of general gun availability on violent crime rates is not significantly different from zero. Given A-M's failure to identify any actual flaws in that analysis, or to establish that correcting any speculated "flaws" would require amending any of my conclusions, I can only conclude that this analysis remains the best available evidence on the question of the net effect of gun prevalence on violent crime rates.

A-M also describe as "popular" certain gun control policies that my analyses indicate would have "harmful consequences." This is misleading for two reasons. First, the popularity of the policies is irrelevant to the issue of whether they would have harmful consequences, so no legitimate purpose could be served by raising the issue of popularity. An illegitimate *ad hominem* purpose, of course, would be to portray me as opposing the "will of the people." Second, the assertion is factually wrong. The policies to which A-M refer are *prohibitionist* controls, i.e. gun bans, and as I noted at length in *PB* (Ch. 9), such controls are *not* popular with the American population (regardless of how popular they may be in the Alba and Messner households). Banning all guns is supported by less than 17% of Americans, while banning just handguns is supported by 36% (*PB*, p. 379). Is it not just a bit misleading to describe as "popular" policies which do not enjoy even simple majority support?

A-M's challenge to my skepticism about prohibitionist controls entailed two points. First, they conceded that I am probably correct that noncriminals would be disarmed at a higher rate than criminals under such laws (p. 405). Second, they disagreed with my evidence-based conclusion (which they inaccurately described as an "assumption") that the "crime-reducing consequences of a lower supply of guns in general will be insufficient to counteract the crime-inducing effect of the shift in gun distribution in favor of aggressors" (p. 405). Their support for their challenge to this conclusion consisted entirely of citing the opinions of unnamed "gun-control advocates." In short, they rebutted evidence with opinion.

The opinion in question is the view that "guns can be a key factor underlying the emergence of criminal motivations in the first place" (p. 405). While it is quite possible that, once motivations to do a crime have emerged, guns can facilitate acting on those motives, the empirical record does not support the view that the availability of guns plays *any* role in the original emergence of criminal motivations, never mind a "key" role. In fact, A-M's claims notwithstanding, even gun control advocates rarely make such an extreme claim.

The closest that previous scholars have come to asserting that guns can actually call forth a "criminal motivation" is the weakly related assertion made by some social psychologists that, among already angered persons, the sight of a gun can "trigger" aggressive behavior. The research literature on this hypothesis was assessed in *PB*, and the review of 21 experimental studies indicated that half of the studies supported the existence of this effect, while the other half did not. It further indicated that the more realistic the studies were, the less likely they were to support the "weapons effect" hypothesis (pp. 158-61, 205-6; see also the review of this literature by Tochand Lizotte [1991], which reached similar conclusions). Thus, as far as we can tell from empirical evidence so far, guns do *not* play any role at all in the genesis of "criminal motivation."

Diagnostic of how they arrived at their conclusions, A-M apparently believed that the opinions of (unnamed) gun control advocates could legitimately be given as much weight as the findings of 21 empirical studies. How else could they reach the conclusion that the evidence in *PB* does not provide a secure empirical foundation for my conclusions? If A-M, and many others like them, are willing to dismiss the results of all research whose conclusions they dislike on the basis of nothing more than one-sided speculation and the opinions of people like themselves, what is the point of even considering evidence? It is just going through the motions, in a way which can only result in confirmation of whatever preconceptions the critics began with.

A-M's most misleading distortion of *PB* appeared on pp. 404-5, where they falsely asserted that my conclusions about the implications of defensive gun use for crime-control policy were based on evidence concerning "deterrent effects," evidence that I myself described as weak. I did indeed note that conclusions about deterrent effects

were weak, and probably always would be, due to the inherent difficulties of evaluating effects which could neither be directly observed nor experimentally assessed. My conclusions concerning the policy implications of defensive gun use, however, did *not* rely to the slightest degree on any conclusions concerning deterrent effects. Rather, they depended on evidence that I considered then, and even more strongly consider now, to be quite sound, i.e. the evidence indicating that defensive gun use is both quite frequent (at least as frequent as criminal uses), and that it is effective in reducing the risks of injury and crime completion once a crime has occurred (i.e. it has “crime disruptive” effects). The possible deterrent effects of gun availability might well turn out to be important in their own right, but my conclusions did not in any way rely on conclusions about their existence or magnitude.

It is hard to believe that A-M honestly misunderstood these points, since they themselves concede that I drew only weak conclusions concerning deterrent effects, they understood the distinction between deterrent and disruptive effects of DGU (see their footnote 10), and since there is not a single passage in *PB* where I state or imply that any of my policy conclusions rely on the existence of deterrent effects (see the concluding chapter of *PB*, where implications for crime-control policy are discussed). A-M’s accusations were thus nothing more than a clumsy attempt to set up a straw man to knock down by fabricating their own strategically distorted version of my arguments.

## 5. Effect of Gun-Control Laws

In their section 5, A-M began by repeating their erroneous claims that they had identified “deficiencies” and “methodological defects” in the models that I used in my city-level analysis of the impact of gun levels on violence rates. They thus treated as real “flaws” that were actually nothing more than unsupported speculations.

They then reviewed the findings of Ch. 10 in *PB* and Kleck and Patterson (1993) concerning the impact of gun control laws on violence rates. They complained that I did not interpret the findings in a sufficiently pro-control light (pp. 406-7). Their erroneous conclusions on this point derived from their mistakes in interpreting how many statistical hypothesis tests I performed. They quoted p. 402 of *PB* to the effect that there were 121 “tests” of the direct effects of gun laws on violence rates. The citation is correct, but A-M’s interpretation is wrong. This count refers to the number of different law-violence rate links (i.e. links between a given gun law and a given violence rate) which were evaluated (“tested”), not the number of statistical hypothesis tests or the number of “chances” gun laws were given to show some impact. Each of the possible law-violence rate links were given multiple (up to three) chances to show an impact. Laws were evaluated in models (1) with the full set of gun law dummies included, (2) with a reduced set of especially strong laws, and (3) with the full set of laws, plus multiplicative interaction terms reflecting the possible interaction of gun law effects with police enforcement effort (*PB* pp. 398-402). In yet another instance of my bending over backwards to give the pro-control position every benefit of the doubt (again, ignored by A-M), I drew an overall positive (pro-control) conclusion if even just one of the tests yielded a supportive result. Consequently, the proportion of the overall evaluations that were positive is necessarily larger than the proportion of the separate hypothesis tests that were positive.

As a result, A-M’s claim that I obtained more pro-control results than could be expected based on chance was based on a misunderstanding of how many chances gun laws were given to show an impact. In *PB* (the numbers are slightly different in the Kleck and Patterson [1993] article), 121 law-violence rate links were assessed, 28 (involving the four laws listed on p. 401) were each given three chances (in the three types of models listed above) to show any impact, and the remaining 93 were given two chances each, for a total of 270 tests of the hypothesis that a particular gun law reduced the rate of a given type of violence. The ten supportive results that I obtained would be 3.7% of 270. Thus, as I indicated in *PB* (p. 402), one could easily get this many supportive results just by chance alone, given the very large number of hypothesis tests.

I nevertheless drew some pro-control conclusions because the findings concerning a few of the law-violence links accorded with prior research (*PB*, pp. 404-405). Thus, I drew pro-control conclusions *despite* the fact that I obtained no more supportive hypothesis test results than one might obtain by analyzing a set of nonsensical randomly generated numbers. To describe this as one-sided is accurate only if what one means is that I was unduly generous to the pro-control side. Somehow, I doubt that A-M intended such a meaning.

A-M also apparently misunderstood the nature of my assessments of the impact of gun laws on gun prevalence levels, claiming that somehow I should have drawn pro-control conclusions because “21 of 102 tests wholly or partly support the hypothesis that such laws do reduce gun prevalence” (p. 407). In this case, the tests of the effects of gun laws on gun prevalence levels in different violence rate models cannot be assessed as if they were multiple independent tests, and the “21 of 102” number is irrelevant. The gun prevalence measures used in different violence rate models (e.g. the homicide rate model, the robbery rate model, and so on) differ only slightly from each other, because some of the components of the gun prevalence factors had to be removed to avoid artifactual associations (*PB*,

p. 195). The gun prevalence measures, however, differ by just one component (out of five indicators used) from one violence rate model to the next. Therefore, the coefficients indicating the impact of gun laws on gun prevalence were estimated multiple times, in multiple violence rate models, but the tests are highly overlapping. Consequently, it is not meaningful to count up their results as if they were independent tests. Instead, they can only be assessed as a group, separately for each law.

For example, waiting period laws showed a significant negative effect on gun prevalence rates in the aggravated assault and burglary rate models, but not in the other five models. The differences are attributable to slight differences in which set of gun indicators were used in the gun prevalence factor in a given model (as was explained in *PB*, pp. 397-8). Evidence of the impact of gun laws on gun prevalence levels therefore can only be meaningfully assessed as a *set* of overlapping results, taking the results one gun law at a time. Thus, for waiting periods, the evidence taken as a whole indicates that this law does not reduce gun prevalence levels, since five of seven overlapping tests indicated no impact. Applying the same procedure to the other 18 gun laws, the same conclusion is reached. *None* of the 19 laws showed a significant negative coefficient in the gun prevalence equation in even a simple majority of the seven violence rate models. Thus, there were really 19 tests of the effects of gun laws on gun prevalence levels, and all 19 supported the same no-effect conclusion. It is far-fetched to describe it as one-sided for me to conclude that existing gun laws do not reduce gun prevalence given that 19 of 19 tests supported this conclusion. It would have been dishonest to describe the evidence as anything but one-sidedly contrary to the hypothesis that gun laws reduce gun prevalence.

Apparently the only course I could have taken that would have satisfied A-M would have been to deceive readers in precisely this way.

A-M criticized me for not discussing in *PB* findings reported in Kleck and Patterson (1993) concerning a “gun law index” (GLI). This was a factor combining all 19 gun laws together, and the GLI findings supposedly show, at least in a few models, that gun laws really do reduce gun prevalence (p. 19). The explanation for this omission is simple. First of all, it was impossible for these results, published only in the 1993 article, to have been discussed in *PB* since they had not even been produced until after I completed *PB* in the Spring of 1990. Second, even had these results been available, I would not have reported them in *PB* since I consider them worthless. Use of the GLI was forced on Britt Patterson and me by the editors of *JQC* and a referee as a condition for their agreeing to publish the article. There is no policy value whatsoever to information on the relationship between violence rates and this heterogeneous mishmash of gun laws. Legislatures pass specific guns laws; they do not increment “gun control severity.” Further, neither Patterson nor I could figure out what exactly this index measures. Factor analysis of the 19 laws yields a single factor only if the analyst artificially constrains it to a one-factor solution. Thus, “gun control” is not a unidimensional concept, but one with multiple dimensions, perhaps of different kinds of gun control with different effects. The GLI may even simply reflect differing kinds of support for “doing something” about crime.

## 6. A Final Note of Skepticism

In A-M’s concluding Section 6, they largely abandoned any effort to mobilize evidence or logic and simply descended to mudslinging, alluding to my alleged “blind spots” and to my supposedly unduly restrictive “paradigm” that prevented me from seeing the alternative interpretations of evidence that A-M could see so clearly (p. 407). The *ad hominem* themes of this section are (1) I am biased, and my research findings and conclusions were somehow distorted by my biases, and (2) I arrived at the conclusions in *Point Blank* only because I was incapable of seeing other interpretations, rather than because they were the interpretations most consistent with the full body of evidence.

A-M provide no direct evidence of my personal biases or political views, inferring them instead from alleged flaws in my research which supposedly somehow illegitimately favor anti-control conclusions. Having failed to identify any flaws in *PB* which might have been a product of my supposedly anti-control biases, this indirect inference of bias also fails. More dishonestly, they withhold from their readers almost all of the only direct information they did have on my political biases. In an “Author’s Voluntary Disclosure Notice,” placed prominently at the very beginning of the book, I reported that I was (and am) a member of, and contributor to, the American Civil Liberties Union, Amnesty International USA, Common Cause, and other organizations widely regarded as liberal (some are even pro-control); that I was a lifelong registered Democrat; and that I have never been a member of, nor had my research funded by, either the National Rifle Association or any other gun owner or anticontrol organization. (A-M mention only that I am not a member of an advocacy group on this issue--p. 408.) In short, every scrap of independent evidence available to A-M on my personal biography argued against their personal bias thesis, unlike the facts pertaining to Richard Alba, who admits to membership in a pro-control organization whose publications on the topic can only be described as blatantly dishonest and crudely propagandistic.

A-M nevertheless concluded that I was biased, solely on the indirect basis of their own one-sided misreading of *PB*. Note the circular nature of A-M's covert reasoning (see also Sherman [1993] for an example of the same tactics): "One cannot accept Kleck's research conclusions because they are distorted by his personal biases against gun control. How do we know Kleck is personally biased against gun control? Because he has drawn anti-control conclusions based on flawed research. Why are his research conclusions flawed? Because he is biased against gun control." It cannot be stressed too strongly that, in the final analysis, A-M's conclusions about supposed flaws in my research rely most heavily on their speculations about my personal biases, since their assessment of my research failed to identify any flaws that could have wrongly buttressed the policy conclusions at which I eventually arrived, and that therefore could have been the product of personal bias. Even if one accepted A-M's evidence-free speculations as fact, nowhere did they provide even a speculative basis for concluding that the alleged flaws were sufficiently consequential that correcting them would overturn any of my policy conclusions.

A-M seemed to be so blinded by their own ideological and cultural biases that they could not even conceive of the possibility that they might simply be wrong on these issues. They did not bring any new evidence to bear on these questions, nor any new theoretical or conceptual insights, and did not identify any actual technical flaws which could overturn any of my policy conclusions. Yet they concluded that it is I who was biased.

A-M speculated that criminals would have less reason to arm themselves if gun ownership were less widespread in the general population (p. 408), an imaginative idea that appears to have originated with Gary Green (1987, p. 71). In a rare use of empirical evidence, A-M cite, as their sole bit of support, the responses of felons to a single question in a 1983 prison survey (Wright and Rossi 1986).

I am happy to see that A-M concede that criminals do think about, and take seriously, the possibility that victims might be armed. As to whether noncriminal gun ownership increases criminal gun carrying, the single survey result that A-M cite does not support their speculation. The issue is not whether concern about victims with guns might be part of the motivation influencing criminals' decisions to carry guns, but whether there are any criminals who carry guns but who would *not* carry them were it not for their concerns about armed victims. Rather than leave this matter, as A-M did, totally at the speculative level, I examined the Wright-Rossi dataset (easily available on a CD-ROM from the National Institute of Justice [NIJ 1994]), to discover how many gun-using felons in this survey cited the armed victim reason for carrying, but no other reasons. There were 377 felons who had reported committing more than one crime with a gun in their lifetimes (a generous definition of "gun criminal"), and who rated victim gun possession as a "somewhat important" or "very important" reason "why a person might decide to carry a gun while doing a crime." Of these 377, only *one* felon gave this as the only reason for gun carrying that he rated as somewhat or very important.

Indeed, gun criminals who considered victim gun possession to be a somewhat or very important reason for carrying also endorsed a median of *seven* other reasons (of 14 possible) that they considered to be somewhat or very important reasons to carry guns. In sum, the notion that there are criminals who would not carry guns were it not for victim gun possession received close to zero support from this survey. The A-M/Green hypothesis did not go "unnoticed" because of "blind spots" in my vision or my supposed "one-sidedness," but rather because it is devoid of empirical support.

A-M's rhetorical excesses reach their crescendo in their last paragraph, where they register their dismay that, because of widespread gun ownership, people will be afraid to "assert their rights ... to honk their horn when their car is cut off" (pp. 408-9). It is tough to satirize arguments this silly. I will just leave it to readers to infer what they may from the fact that the only specific cost of this type that A-M can describe is foregone opportunities for the childish venting of anger.

On a more serious level, A-M's concluding paragraph is foolish because it attacks the messenger for the message. A-M describe an armed America as "the kind of society that Kleck envisions," i.e. as a vision that I have somehow conjured up or a state of affairs I advocate. I have neither advocated bringing about or maintaining such a state of affairs, nor expressed my personal approval of it. Instead, I have simply described America as it currently is, for good or ill. Perhaps A-M wish that I had editorialized against it more. I make no apologies for not having done so.

A-M conclude their article with a final unsupported speculation. They claim that, while selfish gun owners feel better for having guns, everyone else is frightened by "the knowledge that many guns are in its homes, on its streets, and even in its schools" (p. 409). They further assert that, rather than this merely being an empirically unsupported speculation, there is "a great deal of persuasive testimony" supporting the idea.

It is crucial to note that this argument pertains to "guns in ... homes," i.e. gun ownership itself, *not* just violent acts committed with guns. Though the two are obviously conceptually distinct, many people, evidently including A-M, have difficulty avoiding slipping imperceptibly from assertions about gun violence to assertions about guns themselves. Obviously, every rational person is frightened of violent acts committed with (or without) guns. A-M,

however, are making a very different argument that, above and beyond these fears of criminals having and using guns, people are also afraid of gun ownership itself, and thus fear ownership by *non*criminals.

Diagnostic of what A-M consider “persuasive” evidence, their support for this thesis consisted entirely of two newspaper articles. Readers interested in assessing A-M’s reliability are encouraged to read these articles themselves and to search for information pertinent to the effects of gun ownership, as distinct from gun violence, on fear. Neither contains a word on the subject. The first (Ayres 1994) reported on hearings called by congressional gun control advocates, and quoted four teenagers from high crime neighborhoods who expressed their fears of gun *violence*. None said anything about their fears of gun ownership in general or among noncriminals in particular. The second article (Dugger 1994) was equally unsupportive and even less relevant. It profiled New York City teenagers accused of murder, and did not address the issue of fear (of gun ownership *or* gun violence) at all. In short, A-M’s claim to have evidence of “a great deal of persuasive personal testimony” that widespread gun ownership, as distinct from gun violence, is increasing fear is false, based entirely on two newspaper articles that the authors had to have known contained no supportive evidence.

In sum, A-M have provided neither logic nor evidence to support their attack on *PB*. Rather, they have produced a “critique” based on: (1) the repeated use of speculation as the sole basis for rebutting empirical evidence, (2) libelous innuendo (hinting that I have slanted interpretation of the evidence to support my personal prejudices), (3) misleading use of citations to support claims for which they had no evidence (citation of two newspaper articles which contained no support for their claims), (4) deliberate distortions of my positions which enabled the authors to knock down straw man versions of those positions, (5) the use of red herrings which misstated critical issues, (6) careless misreading of material in *PB* that they claimed to understand, (7) rhetorical excesses and propagandist appeals to emotions in an already overheated arena, and (8) a pervasive tendency to try to win by illegitimate means what they could not earn with evidence or logic.

At a political level, of course, none of this will matter. Once published, the A-M article, and others of its ilk, serve their political purposes regardless of how thoroughly their unprincipled and baseless attacks are rebutted. Authors of pro-control propaganda will use the A-M article to write something like the following: “Kleck’s work has been discredited (rebutted/called into question/challenged) (Alba and Messner 1995).” Such authors will not, however, mention that A-M’s attack was thoroughly rebutted.

The rules established by *Journal of Quantitative Criminology* editors John Laub and James Fox for the exchange between A-M and me granted to A-M the opportunity to get in both the first and last word, by writing both a critique and a rejoinder to my reply. Once I had seen the nature of A-M’s attack, I strongly objected to this format, since it meant that A-M would thereby be given license in the second round to simply dish out more of the same, knowing that I would have no further opportunity to rebut any of it. Professor Laub assured me he would not allow A-M to do this (telephone conversation with John Laub). In their rejoinder (Alba and Messner 1995b), this is precisely what A-M did. Their response consisted of a repetition of claims I had already disposed of (e.g., concerning their erroneous counts of hypothesis tests--p. 428), repetition of patently false technical claims (e.g. that negative significance tests provide indications of the magnitude of associations--p. 426), false characterizations of my statements (e.g. distorting my remarks about facilitating effects of guns as if they supported A-M’s claims that gun availability causes “motivations” for aggressive acts to develop (p. 427), and so on.

Their rejoinder was most revealing at its start, when A-M announced that “not every counterargument raised by Kleck is mentioned here; in particular, we refuse to be drawn into a tit-for-tat reply to some of this more contentious charges.” I never heard of a scholar who could resist expressing a decisive reply to an intellectual adversary. Consequently, my personal interpretation of A-M’s remarks is that they are a tacit confession that my counterarguments were correct, and that their criticisms of *Point Blank* were baseless. Until they do decide to honestly engage these issues, and present an intellectually serious rebuttal to my counterarguments, I will stick with this interpretation.

Finally, to put the A-M attack in perspective, it may be worth noting how eccentric their response to *Point Blank* was. More typical of responses were those of the book’s reviewers, who provided the following assessments: Joseph Sheley (*Political Psychology* 17(2):375, 1996)--“clearly the single most authoritative source of information about firearm-related issues.” Robert Cottrell (*Criminal Justice Review*, 1994)--“fresh, methodologically rigorous ... prodigious research.” Lawrence Sherman (*The Criminologist*, 1993, p. 15)--“As a comprehensive reference, there is nothing like it ... essential reading.” Paul Blackman (*The Criminologist*, 1993, p. 16)--“the definitive criminological study of the role of firearms in American life.” Philip J. Cook (*New England Journal of Medicine* 1994, p. 344)--“Comprehensive ... encyclopedic.” H. Laurence Ross (*American Journal of Sociology* 98(3), 1992)--“a necessary acquisition for all social science library collections and for any serious scholar working in the area.” Alan Lizotte

(*Contemporary Sociology*, May, 1993, pp. 339-340)--“easily the most comprehensive work on firearms, violence, and firearms control ever published ... a virtual treasure trove of information ... required reading for those interested in both sides of a serious debate about gun control ... Kleck’s research will change the direction and raise the level of discussion.” Fred Hawley (*Social Forces*, December, 1992, p. 548)--“This ... magnum opus is essential reading and resource material for those interested or engaged in research on issues involving gun-related violence ... Magisterial.” *Choice* (May, 1992, p. 662)--“Comprehensive. Recommended.” Raymond Kessler (*Journal of Criminal Law and Criminology* 82(4), p. 1187, Winter, 1992)--“In recent years there have been a number of books on firearms, violence, and gun control. This book ... is the best so far ... comprehensive and valuable critique and synthesis of the existing literature [and] some of the best ... original empirical research ... This book will be the new starting point for everyone interested in the topic.” Don Kates (*The Public Interest*, 1991, p. 106)--“in any [effort to devise workable gun controls], *Point Blank* will be the primary information source.” Gary Mauser (*Criminal Law Forum*, August 1991, p. 149)--“encyclopedic study ... comprehensive coverage ... an enlightening discussion of an important public policy issue by a scholar who is refreshingly objective.” William Wilbanks (letter to Gary Kleck)--“one of the most important books ever published in criminology.”

My challenge to A-M, and others with similarly one-sided views, is this: Do the research *better*, using technically stronger methods applied to policy-relevant questions, and if you obtain results contradicting my own, I will revise my views. However, using crude univariate methods to assess the efficacy of gun laws (e.g. Loftin et al. 1991), public health analyses of samples of two (!) cases (Sloan et al. 1988), or the use of manifestly inappropriate data sources to assess the frequency of defensive gun use (McDowall and Wiersema 1994) clearly do not constitute doing the research better. And worse still, one-sided, evidence-free speculation promises only to clog up the channels of scholarly communication.

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