American Journal of Epidemiology
Vol. 136, No 5

Printed in U.S.A.

LETTERS TO THE EDITOR

RE: "HOMICIDE AND THE PREVALENCE OF HANDGUNS: CANADA AND THE UNITED STATES, 1976 to 1980"

The finding by Centerwall (1) that the prevalence of handguns in the border states and provinces of the United States and Canada is not correlated with the frequency of homicides is upsetting to those who feel that the possession of handguns is an unnecessary evil. However, there is an interpretation of this finding that is consistent with the belief that the private possession of handguns would not increase homicides.

A grossly oversimplified example will serve to illustrate the point. Suppose we have a population composed of a violent 5 percent who commit all homicides and a nonviolent 95 percent who do not commit homicides. Further, suppose that all households in the violent group have handguns, compared with only 5 percent of the nonviolent households (the Canadian prevalence in Centerwall's table 2 (1)). If the proportion of handgun ownership in the population were increased to 26 percent (the US average in table 2), the increase could only occur as the result of increased ownership among the nonviolent group. Increased ownership would not result in increased homicides.

It does not seem unreasonable to believe that in both Canada and the United States handguns are already available to a very large proportion of homicide-prone persons. Consequently, any increase in ownership will occur mostly among persons who are unlikely to commit homicide and will not be associated with a detectable increase in homicides. Conversely, to have a major impact on homicide rates, handguns may have to be removed from the minority of violent, potentially homicidal persons.

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The article by Centerwall in a recent issue of the *Journal* (1) was an interesting ecological study that examined the relation between handgun ownership and mortality from homicide in contiguous US states and Canadian provinces. He found that if major urban centers (e.g., Detroit, Michigan and New York, New York) were excluded, homicide rates varied little between contiguous states and provinces, despite considerably higher handgun ownership in the United States. In essence, this study suggests that in reasonably affluent, socially intact, working- and middle-class areas, high levels of handgun ownership do not appear to increase homicide rates. This, of course, is a useful finding, but this study cannot be considered at all definitive because it totally excludes high-risk populations.

With the exception of the Yukon and Alaska, the states and provinces studied had relatively low homicide rates (1.2–4.9 per 100,000 population) and can be considered low-risk areas. In contrast, Baltimore, Maryland, for example, registered 304 homicides for 1991 in a population of approximately 700,000 for a homicide rate of 43 per 100,000 population. If any handgun/homicide relation does exist, a study in low-risk areas can only be expected to bias the risk estimate toward one. Alternatively, any relation between handguns and homicide may be subject to strong effect modification by other factors.

Indeed, it seems likely that high rates of gun ownership (or gun possession, if illegally obtained) in certain circumstances would be related to homicide rates. These circumstances would include poverty, social disintegration, and drug abuse such as that seen in many major urban centers in the United States. A much more informative study would be to compare high-risk urban areas in the United States and other Western nations. This would be difficult, of course, since the levels of abject poverty and social disintegration seen in US cities are difficult to find in Europe, Canada, or Australia, for example. The development of internationally comparable indices of poverty is also a formidable, although not impossible, challenge. Nonetheless, pockets of urban poverty do exist elsewhere, particularly in some large British cities. Since Britain has the tightest gun control laws in the developed world, it would make an ideal comparison.

I would hypothesize that the relation between handgun ownership/possession and homicide rates is modified by poverty and social disintegration, (e.g., high levels of drug use, single-parent families, low education levels, high unemployment, etc.). That is, the risk of homicide attrib-

utable to handgun ownership/possession increases as poverty and social disintegration increase. This seems to be an eminently testable hypothesis even with ecological approaches. It also suggests that multiple public health interventions should be possible. These would include gun control, antipoverty measures, and the rebuilding of social infrastructures.

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Dr. Brandon Centerwall recently compared the prevalence of handguns (in metropolitan and rural areas combined) and rates of homicide (with metropolitan areas excluded) between Canadian provinces and adjoining US states during 1976–1980 and concluded that "major differences in the prevalence of handguns have not resulted in differing total criminal homicide rates in Canadian provinces and adjoining US states" (1, p.1245). However, he failed to point out that his conclusion was based on the assumption that either the prevalence of handguns in each country was similar between metropolitan and rural areas or the differences in the prevalence of handguns between cities and rural areas in Canada and the United States were of comparable direction and magnitude. Unfortunately, neither assumption is likely to be true. Gun owners residing in rural areas are more likely to have purchased their weapons for recreational reasons than gun owners in urban areas (based on a survey conducted in the United States; data for Canada are not available) (2). A handgun control law, such as the one that was present in Canada during the study period, which has strict restrictions for buying handguns for self-defense but not for recreational purposes (3), probably affects the prevalence of handguns in cities considerably more than in rural areas. Thus, a province-wide prevalence number for handgun ownership may underestimate the true prevalence in rural Canada, the area for which homicide rates were evaluated. Furthermore, while there are no data to compare the prevalence of handgun ownership between US and Canadian cities during the late 1970s, it is likely that the prevalence in US cities was higher: Handgun ownership had increased by more than 62 percent in US cities between 1959 and 1976 (2), while during the same period, the restriction on handgun purchases in Canada would likely have prohibited any corresponding increase in cities in that country. Therefore, the 3- to 10-fold difference in the prevalence of handgun ownership between Canadian provinces and US states may principally reflect the difference between Canadian and US cities rather than between rural areas. In fact, the data in Dr. Centerwall's paper clearly showed that homicide rates in the US cities were much higher than in Canadian cities. However, Dr. Centerwall chose not to compare the cities of the two countries because of potential confounders such as social and economical disparity.

Another concern regarding Dr. Centerwall's finding is its generalizability. The presence of a handgun is only the one of many necessary causes for a gun-related homicide, but not a sufficient cause. The presence of other similar (if not more) lethal and "convenient" weapons (e.g., rifles or shotguns) can easily replace handguns as a necessary cause. It has been reported that in rural areas households with rifles and shotguns were much more prevalent than in cities (2). Therefore, the fact that the prevalence of handgun ownership is not correlated with homicide rates in rural areas does not preclude the possibility of such a correlation in urban areas where another type of firearm may not be as accessible.

In conclusion, while Dr. Centerwall's paper has provided some potentially useful information on the relation between handgun ownership and homicide, additional studies on handgun prevalence in the actual populations for which homicide rates are being compared need to be performed before one can accept his conclusions.

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The *American Journal of Epidemiology* is to be congratulated on the publication of Dr. Brandon Centerwall's fine article (1). Among its many virtues is his familiarity with the vast corpus of criminological literature on firearms and their control. For instance, in his response to Dr. Centerwall (2), Dr. Harold Houser cites a paper

that argued for the value of Canada's restrictive handgun control policy for reducing homicides, but which was based on a short-term comparison of two cities, Vancouver and Seattle (3). The criminological literature reaches opposite conclusions, based on statistical comparison of both *nations* for an entire decade or more (4, 5).

Academic health literature often attributes the much lower violence rates of selected foreign countries to their banning of guns. International criminological research reveals the opposite since these countries' violence rates were as low or lower before their antigun policies were adopted. The purpose of those policies was reduction, not of ordinary violence, but of *political* violence. A further daunting truth is that these policies have had scant success; those countries consistently suffer far more such violence than does the United States. Nor have their antigun laws insulated them against the drastic post-World War II increases in violence which have afflicted the Western world generally. At the same time, nations like Israel and Switzerland also have very low apolitical crime rates, although guns (particularly assault weapons) are even more available than in the United States, and some other countries which ban guns, e.g., Taiwan, South Africa, and the former Soviet Union, suffer apolitical homicide at rates comparable with ours (6–9).

Criminological literature teaches that societal violence rates simply reflect the differences in the numbers of violent people that various societies produce. The differences between societies in availability of firearms have, at most, negligible import since in all societies the numbers of weapons illegally available will always suffice to arm those inclined toward violence.

These lessons are confirmed by the empirical facts refuting two other common misconceptions. First, studies trying to link gun ownership to violence rates find either no relation or a *negative* relation, i.e., that cities and counties with high gun ownership suffer less violence than do demographically comparable areas with lower gun ownership (10–17). Summarizing these and other studies, a recent National Institute of Justice study concludes:

> It is clear that only a very small fraction of privately owned firearms are ever involved in crime or [unlawful] violence, the vast bulk of them being owned and used more or less exclusively for sport and recreational purposes, or for self-protection (19).

Second, murderers are not, as so often portrayed, ordinary citizens who happened to have a gun available in a moment of ungovernable anger against some relative or acquaintance. Criminological studies uniformly reveal murderers (and perpetrators of fatal gun accidents) to be highly aberrant individuals whose spectacular indifference to human life, including their own, is evidenced by life histories of extreme violence and other felonies, substance abuse, and automobile and other dangerous accidents (6, 19–24). Nor is this refuted by the irrelevancy that murderers generally kill relatives or acquaintances. After all, lifelong violent aberrants have relatives and acquaintances, too.

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THE AUTHOR REPLIES

Dr. Comstock (1) hypothesizes that if the violent 5 percent of the population in both Canada and the United States have essentially complete access to handguns, the homicide rates in Canadian provinces and adjoining US states would be the same, despite major differences in the prevalence of handguns among the nonviolent 95 percent of the respective populations. It would follow, as a corollary, that reductions in the prevalence of handguns among the violent 5 percent could result in major decreases in the homicide rate.

If Comstock's model were correct, not only would Canadian provinces and adjoining US states have the same total homicide rate, but the rate at which homicides were committed with handguns would also be the same (since, *ex hypothesi*, the violent 5 percent would have equal access to handguns on both sides of the border). However, as shown in table 5 of my article (2), the rate at which homicides were being committed with handguns was 3–16 times greater in US states than in adjoining Canadian provinces (with the exception of Quebec), even though the total homicide rates were the same (2, table 4). Dr. Comstock's model is reasonable a priori, but the data demonstrate it to be incorrect. Violent Canadians are not drawing upon a pool of handguns in order to achieve homicide rates equivalent to those in adjoining US states. It follows that major differences in the prevalence of handguns, even among the violent 5 percent, have not resulted in differences in the total homicide rate. The violent 5 percent in Canada have efficiently and fully compensated for their relative lack of handguns.

A comparison of Canadian provinces with adjoining US states has failed to find any observable main effect of handgun prevalence upon homicide rates, despite major differences in handgun prevalence (2). Fortunately, to address the concerns of Graham (3) regarding generalizability, the data set includes both high- and low-risk areas (2, table 4). For the years studied, Alaska had one of the highest homicide rates in the United States, and the Yukon had a homicide rate greater than that of any US state. Alaska and the Yukon were characterized by high rates of poverty, endemic alcohol abuse, and as much social disintegration as any investigator would care to see (stemming from the massive destruction of indigenous cultures during the last century). For all that, the Yukon had a homicide rate that was 46 percent greater than that in Alaska (2, table 4), even though only 2 percent of the homicides in the Yukon were committed with handguns, compared with 41 percent of the homicides in Alaska (2). It is concluded that there is no observable interaction between handgun prevalence and social disintegration with respect to homicide rates.

However, what about urban concentrations of population, e.g., Baltimore, Maryland, which has a population of 700,000? Sloan et al. (4) have demonstrated that for carefully matched urban populations—the non-Hispanic whites living in Seattle, Washington (population, 490,000) and Vancouver, British Columbia (population, 420,000)—annual United States and Canadian homicide rates were virtually identical (6.2 and 6.4 homicides per 100,000 white population, respectively). It is concluded that there is no observable interaction between handgun prevalence and urban concentration (up to 500,000 inhabitants) with respect to homicide rates. Indeed, the three US cities in the data set with populations of less than 500,000 —Seattle, Washington, Minneapolis, Minnesota, and Buffalo, New York—had homicide rates comparable with those observed in Canadian cities, whereas the two US cities with populations of greater than 500,000—Detroit, Michigan and New York City—could not be appropriately matched with any Canadian population. In 1980, 88 percent of the US population lived in groupings of 500,000 inhabitants or less and accounted for 69 percent of US homicide deaths (5). This gives a sense of the generalizability of the findings. None of this precludes the possibility of main effects and interaction terms emerging in urban concentrations of greater than 500,000 inhabitants, although to demonstrate their existence

would require, as Graham (3) notes, appropriate control groups.

Contrary to what Graham states, Britain does not have the tightest gun control laws in the developed world. That honor goes to South Africa, where blacks have been prevented from possessing firearms for many years. Indeed, delivery of firearms to blacks in South Africa has been *ipso facto* treason and subject to penalties up to, and including, death (6, 7). As a result, there have been virtually no firearms of any kind whatsoever among South African blacks. As of 1980, 6,500,000 South African blacks were living in urban areas (8), most of them at levels of abject poverty and social disintegration which would be difficult to find anywhere else in the developed world except in US cities. Given the virtually complete absence of firearms among South African blacks, they would make a good comparison group. As of 1983, the annual homicide rate among South African blacks was 39.3 homicide deaths per 100,000 black population (8), compared with a homicide rate among US blacks of 30.3 homicide deaths per 100,000 black population (9). Of course, this compares the entire black population of South Africa with the entire black population of the United States. I will leave to Graham the formidable, although not impossible, challenge of making more detailed comparisons between high-risk urban areas in the US and South Africa—Baltimore, Maryland, and Soweto, for example. Graham's hypothesis is, as he points out, eminently testable.

The criticisms of Li and Weiss (10) are based upon a minor misunderstanding. I did not exclude metropolitan areas from my calculations of homicide rates. As stated in the Materials and Methods section of my article, ". . .homicide rates by state and province are presented both including and excluding metropolitan areas of greater than 1 million population. . ." (2, p. 1246), thereby allowing readers to judge for themselves the relative importance of these metropolitan areas as confounding variables. The rates are in table 4 of my article; Li and Weiss need only compare the homicide rates of provinces and adjoining states—including the metropolitan areas—for their concerns to be addressed. The handgun prevalence data are for the same populations as those for which homicide rates are being calculated. It can be seen from table 4 that for six of the eight metropolitan areas in the data set—Seattle, Washington; Minneapolis, Minnesota; Buffalo, New York; and Vancouver, Toronto, and Montreal, Canada—whether they were included or excluded made little difference. Contrary to the statement by Li and Weiss, only two metropolitan areas—Detroit, Michigan, and New York City—proved to be important confounders.

Li and Weiss (10) also express a concern regarding the generalizability of the findings to urban areas. That issue is addressed in the fourth and fifth paragraphs of this letter.

I thank Kates (11) for his comments. I thank the other writers for the acuity and care shown in their letters. I especially wish to thank the editors. Their willingness to guide this article through to publication on the basis of its scientific merits, despite a personal distaste for the conclusions, represents an adherence to a high ideal of scientific integrity and objectivity —an ideal which should be the ideal of all epidemiologists when they grapple with a politically sensitive issue, be it firearms or fluoridation, abortions or worker safety.

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