

Analysis of the Johns Hopkins University Study Entitled ‘Effects of Maryland’s Law Banning “Saturday Night Special” Handguns on Homicides

Philip F. Lee, PhD
3/10/02 (revised 4/24/06)*

Those who have knowledge, don't predict. Those who predict, don't have knowledge.

--Lao Tzu, 6th Century BC Chinese Poet

An unsophisticated forecaster uses statistics as a drunken man uses lamp-posts -- for support rather than for illumination.

--After Andrew Lang

Summary

The Johns Hopkins University (JHU) study by Daniel W. Webster, Jon S. Vernick, and Lisa M. Hepburn (*Am J Epidemiol*, Vol. 155, No. 5, 2002) used the wrong data for its analysis and missed a large rise in murders with handguns in the wake of the Saturday Night Special (SNS) ban. The study neglected to account for national crime policy effects in their analysis. The study made a conceptual error by arguing “causality from correlations.” The study authors have placed several “editorial opinion” statements in their paper, which are not supported by data or analysis.

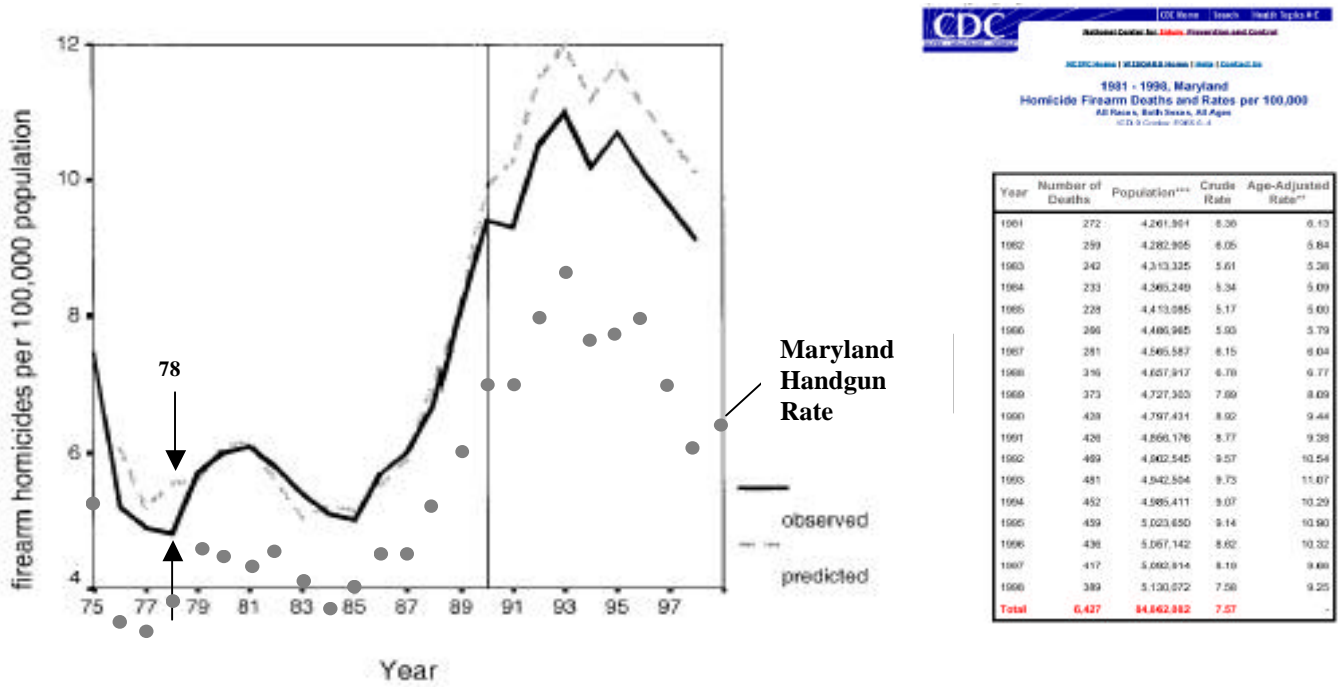


FIGURE 1. Observed age-adjusted firearm homicide rates in Maryland during 1975–1998 versus rates predicted by an autoregressive integrated moving average regression model that assumes a gradual effect of the state’s 1990 ban of “Saturday night special” handguns. During 1990–1998 the predicted series represents the rates that would have been expected without the Saturday night special ban.

Introduction

In passing the law to ban Saturday night special (SNS) handguns in 1988, Maryland politicians expected a decline in violence. What they got was an increase of 68% in handgun murders (65% in rates). Daniel W. Webster, Jon S. Vernick, and Lisa M. Hepburn, referred herein as WVH claim the SNS ban actually lowered violence relative to what it would have been. Their estimates, based on statistical modeling and predictions, are mistaken and they have made several fundamental mistakes in methodology. To explain these mistakes, we reproduce their Figure 1 (with their caption) and add handgun murder rates plotted as dots and an insert identifying the source for *their* data from the Center for Disease Control (CDC). WVH have used age adjusted CDC data for *all firearm homicides*, not just handgun homicides – see <http://webapp.cdc.gov/sasweb/ncipc/mortrate9.html>. Changes in long gun (rifle or shotgun) homicides over the study period contribute an effect on WVH’s prediction as large as the one they claim from the SNS ban. But the SNS ban did not restrict sales of long guns. We will continue to refer to this figure and our additions in explaining WVH’s errors.

* Edited and revised from testimony to the 2002 session of the Maryland legislature

Moral Concerns

Before addressing the technical aspects of the paper and its errors, we note an editorial statement made by WVH creating a moral issue with their study. On page 411 WVH say:

Opponents of the law predicted that it would leave low-income citizens more vulnerable to crime by increasing the price of handguns and would increase the lethality of shootings, because criminals would acquire higher caliber substitutes for Saturday night specials. We could not examine these possible intermediate effects with the data available. Absent such data, the net effects of the law suggest that any negative consequences for homicide rates were apparently outweighed by the law's benefits.

The WVH data contain homicides from police and self-defense killings of criminals in addition to killings by criminals. By including justifiable killings WVH have equivalence all bodies produced by firearm homicides no matter how the killings occurred. That is, WVH would count the deaths of a Sgt. Prothero or Cpt. Toatley (to name two decent Maryland citizens murdered recently) with the same weight as if these two police officers had killed their assailants instead. They dismiss the moral issue of decent people losing their lives from lack of means for self-defense by claiming the data does not allow a better accounting. That claim simply begs the moral question that these researchers have ignored.

It would be fair to raise this moral issue in connection with any paper of this kind, but these authors have made it an imperative to do so because they demonstrate moral blindness in dismissing the issue¹.

Both the motivation for and the conclusions of WVH about the SNS ban and handgun homicides should be questioned because they make no effort to distinguish lawful use of firearms from lawless uses.

Technical Flaws in Methodology

1. **The WVH study uses CDC firearm homicide rates for Maryland in Figure 1 rather than handgun murder data from the Maryland State Police (MSP).** This choice is strange given the subject of their study is the effects of a handgun ban. Their data includes long gun and justified homicides (e.g., self-defense and legal intervention by police). Using the CDC data WVH assert the SNS ban produced fewer deaths than would have happened without the ban. **Actually, handgun murders increased significantly during the study period.** The total firearm homicide data used by WVH balances the increase in handgun murders partially by significant decreases in long gun use during the study period. Either WVH have made a naïve mistake in choosing contaminated data or they have engaged in data shopping to prove their preconceptions. In either case, their blunder invalidates their results as we shall explain. The MSP data showing murder counts is given in Table 1 below (earliest date of 1981 limited by availability of data from CDC web site) and compares CDC homicide counts.

Table 1. Various Counts of Maryland Homicides from the MSP Uniform Crime Report (UCR) and CDC Sources

YEAR	MSP ALL FIREARM COUNT	CDC ALL FIREARM COUNT	CDC/MSP DIFFERENCE COUNT	CDC/MSP FIREARM DIFFERENCE	MSP ALL HANDGUN COUNT	CDC ALL/ MSP HG ONLY DIFFERENCE
1981	248	272	24	9.7%	186	46.2%
1982	257	259	2	0.8%	200	29.5%
1983	213	242	29	13.6%	179	35.2%
1984	199	233	34	17.1%	162	43.8%
1985	206	228	22	10.7%	177	28.8%
1986	235	266	31	13.2%	204	30.4%
1987	250	281	31	12.4%	214	31.3%
1988	283	316	33	11.7%	241	31.1%
1989	336	373	37	11.0%	282	32.3%
1990	368	428	60	16.3%	339	26.3%
1991	372	426	54	14.5%	342	24.6%
1992	422	469	47	11.1%	393	19.3%
1993	457	481	24	5.3%	432	11.3%
1994	407	452	45	11.1%	384	17.7%
1995	421	459	38	9.0%	394	16.5%
1996	421	436	15	3.6%	405	7.7%
1997	377	417	40	10.6%	356	17.1%
1998	332	389	57	17.2%	313	24.3%

Long guns averaged 17.3% of the firearms used for murder (and 10.2% of all murders) during the eight years 1981 to 1988 according to the MSP. Long gun use for murder decreased to nearly 3.6% of firearms in 1996 (2.7% of all murders) and that decrease hides part of the increase use of handguns in WVH's data. That decrease cannot be attributed to a SNS ban but suggests the ban was counter productive since criminals did not shift from handguns to long guns – rather they shifted the other way. That decrease accounts for a reduction in gun related homicides of 7.5% (= 10.2 – 2.7) -- nearly all of the 9% decrease WVH claim from banning SNSs. **Shifts away from using long guns created a distortion in WVH's study (which they ignored) nearly as large as the effect they claim from the SNS ban.**

The MSP handgun data differs from CDC firearm data by between 7.7% and 46% of MSP handgun murder counts. Using CDC firearm homicide data in the WVH study gives Maryland 30% more homicides in 1988 and 7.7% more homicides in 1996 than handgun murders shown by the MSP. The CDC data shows Maryland as having a much smaller increase in firearm homicide rate than occurred with Maryland's handgun murder rate -- 38% increase versus 65% from 1988 to 1996. The CDC data doesn't differentiate between self-defense shootings by private citizens, justified shootings by law-enforcement officers, and other deaths involving firearms. By contrast the MSP Uniform Crime Report (UCR) gives the same murder counts and rates as published in the FBI UCR, and the MSP reports murders using handguns.

WVH appear unaware that handgun use for murder grew as a fraction of total murders from 54% (of the MSP total) in 1988 to 69% in 1996 while long gun use decreased (from 9.4% to 2.7% of total homicides). WVH say (page 407):

Trends for firearm and nonfirearm homicide rates were analyzed separately to determine whether changes were specific to gun homicides and to examine possible weapon substitution effects.

By looking at the wrong data WVH completely missed the 65% increase in murder rates from 1988 to 1996 and the increased proportional of murders with handguns. WVH ignored the effect of long guns homicides in their analysis and overlooked a large weapons substitution effect in the wrong direction for their conclusions (from long guns to handguns). **In short – WVH missed or ignored important weapon substitution effects that show their conclusions are false.**

We plot Maryland handgun homicide rates in Figure 1 for 1975 through 1999 as large dots to show a visual comparison to the WVH model and “observed” data. The MSP handgun murder rates increased from 5.2 (per 100,000) in 1988 to 8.6 in 1993 and then stay at a level near 8 through 1996 (these rates are the “crude” rates reported by the FBI, not the “age adjusted rates” favored by WVH). WVH may see a benefit from a SNS ban on handgun homicides, but a 65% increase in the rate of murder with handguns makes it hard to argue that benefit.

In fact WVH say (page 409):

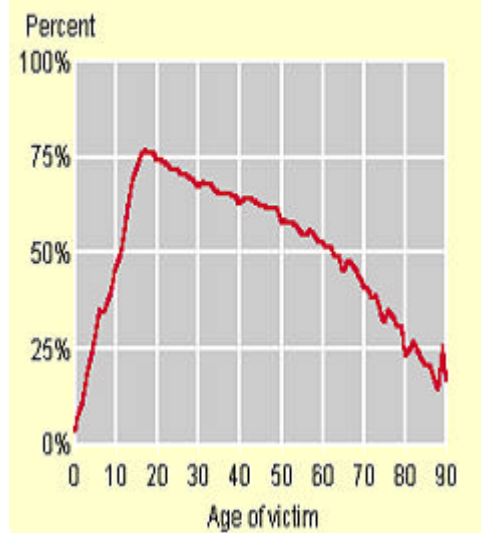
The magnitude of these effect estimates is consistent with our prior research that indicated crime guns recovered in 1996 and 1997 in Baltimore, Maryland, were much less likely to be banned Saturday night specials than was the case in 15 other cities without a Saturday night special ban (8.7 percent vs. 19.7 percent of crime guns).

and (page 409):

*Opponents of the law predicted that it would leave low-income citizens more vulnerable to crime by increasing the price of handguns and would increase the lethality of shootings, because criminals would acquire higher caliber substitutes for Saturday night specials. We could not examine these possible intermediate effects with the data available. **Absent such data,** the net effects of the law suggest that any negative consequences for homicide rates were apparently outweighed by the law's benefits.*

Figure 2 Percent of homicides involving guns by age of victim, 1976-99

<http://www.ojp.usdoj.gov/bjs/homicide/weapons.htm#weapons>



If WVH are correct about the reduction of SNSs in Baltimore as a result of the ban, the 65% increase in handgun murder rates would seem to support fears of increased lethality of shootings as criminals shift to higher caliber (and more reliable) substitutes. **Increased lethality may also be argued from Maryland's aggravated assault rates (FBI UCR) which grew only 6.5% between 1988 and 1996. That is, handgun lethality rate increase is a factor of 10 larger than the increase in assaults (65% increase in handgun homicide rates vice 6.5% increase in overall assaults). But WVH dismiss the increased lethality by their claim “Absent such data,” the law has a “net benefit”.** The fears of increased vulnerability of decent citizens and increased lethality seem fully justified by WVH's data.

2. By using age-adjusted normalization (to a 1980 standard population), WVH have created a bias in murder by handguns. Applying age adjustments to Maryland increases homicide rates in the 1990's and decreases the rate in 1981. By example, in 1995 Maryland's FBI UCR rate of 9.1 becomes age adjusted to 10.9 and from 6.4 to 6.1 in 1981. The CDC explains age adjustment for their 2000 population at <http://www.cdc.gov/nchs/datawh/nchsdefs/ageadjustment.htm>.

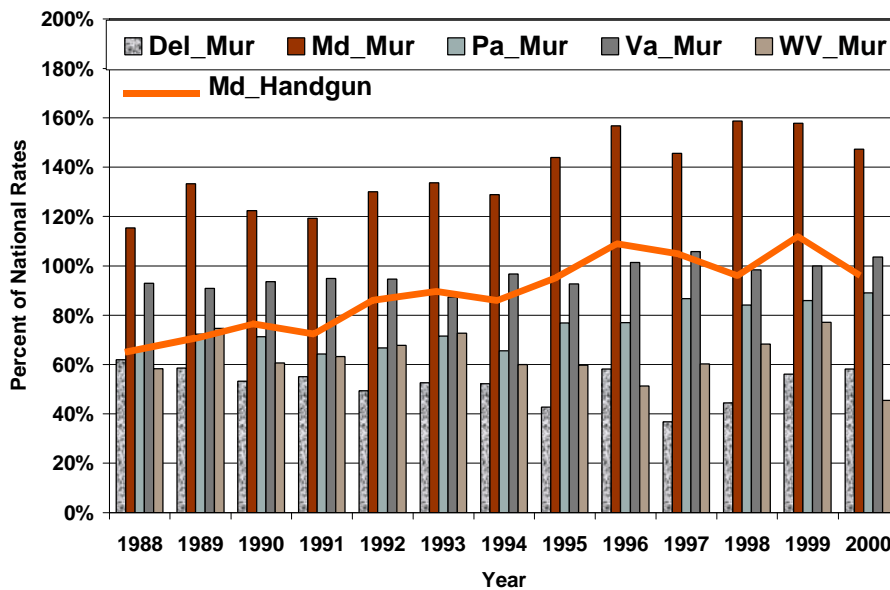
Age adjustment has the effect of weighting victims differently according to their age and weighting weapons used as illustrated in Figure 2. If the standard population were the 2000 population shown in the CDC referenced document, the weighting would have the effect of **counting people who are killed in the age range of 15-24 by 58%**

more than those in the range 55-64 and the weapons used for 15-24 are 75% firearms vice 50% for 55-64. As you might guess a state like Florida with a different age distribution of people would end up with different “age adjustments” than Maryland. For example, Florida’s 1981 crude rate of 11 is age adjusted to 11.6 and 5.8 to 6.7 for 1995 (compare with the Maryland age adjustments above). **Considering that firearms are preferred to kill young adults as shown in Figure 2 (from the Department of Justice, see <http://www.ojp.usdoj.gov/bjs/homicide/weapons.htm>), age adjusted weighting tends to inflate firearm homicide rates where gang activity is large.**

Age adjusting is commonly used in health studies to analyze effects of treatments of age related illnesses (i.e., cancer, heart disease) but victim age is not a factor in murder the way it is in cancer and the effects of a gun control policy aimed at a part of the population defined by age (e.g., denying juveniles access to guns) cannot be determined by adjusting homicide victim population to a standard population. Rather, effects must be determined based on changes in perpetrator populations (e.g., fewer juvenile killers).

3. WHV have not accounted for national trends in their analysis. Violence peaked nationwide near 1992 and some national effects are to be expected in the Maryland homicide data between 1988 and 1992 as well as the trend after 1992. But the WHV study ignores Maryland’s connection to the United States by not controlling for national effects. That is, the study attributes all “reductions” in homicides to the SNS ban and does not credit national effects for contributing to any reduction. **To see what effect national policy might have in Maryland we construct Figure 3 with annual murder rates and handgun murder rates (as a line graph on the same chart) for Maryland and annual murder rates for near-by states plotted as a percent of the US rates.**

Figure 3 Murder Rates in Maryland



national policy might have in Maryland we construct Figure 3 with annual murder rates and handgun murder rates (as a line graph on the same chart) for Maryland and annual murder rates for near-by states plotted as a percent of the US rates.

Figure 3 shows **Maryland’s murder control policy is less effective than the nation as a whole and all of its neighbors.** Maryland’s murder rate has climbed from around 120% of the national rate in the 1988 time frame to 150% of the national rate near 2000 and handgun murder rates for Maryland are 100% of the national total murder rates. Neighboring state murder rates are near or lower than the national rates and substantially lower than Maryland’s. The line graph of handgun murders shows a continued upward trend after the SNS ban with handgun rates 60% of US in 1988. By 1996 the Maryland handgun murder rate

equaled (was 100% of) the national murder rate for all weapons. This trend in handgun murder rates, now controlled for national factors and Maryland’s relatively poor performance compared to its neighbors without similar SNS bans, makes the claimed SNS ban benefit extremely doubtful.

Maryland’s peak reported murder rate (12.7 FBI UCR) occurred in 1993 and exceeded the previous peak rate of 12.5 in 1972. There is an old joke which goes:

Two people are standing on Wall Street in New York City and one is snapping his fingers. The second person says, “why are you snapping your fingers?” The finger snapper replies, “I’m keeping elephants away from Wall Street.” The first person snorts with derision and says, “There are no elephants anywhere near Wall Street!” The finger snapper replies, “See, its working!”

Given that Maryland’s murder rate exceeded the previous peak, it is easy to believe that claims for Maryland’s SNS ban are as substantial as our finger-snapper’s. Unfortunately, people are dying because Maryland politicians are devoted to finger-snapping solutions for violence and the WVH study has contributed to the deception that finger-snapping works.

4. WVH should understand that correlation does not prove causality. To prove causality, controlled experiments are needed.

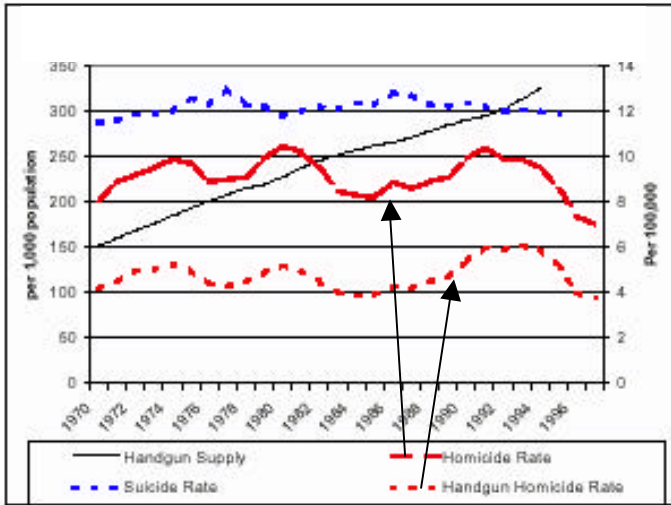
A simple example demonstrates the problem (from http://www.kellogg.nwu.edu/faculty/weber/decs-437/Week_5/Correlation.htm):

In the late 1940s, a nationwide study conducted over several years found a high correlation between the incidence rate of new cases of polio among children in a community, and per capita ice cream consumption in the community. (Equivalently, a simple regression model, using ice cream consumption to predict the rate of occurrence of new polio cases, had a high coefficient of determination.)

Fortunately for those of us who like ice cream, a re-examination of the data showed that the high values of both variables occurred in communities where the study collected data in the summertime, and the low values of both occurred in communities where the data was collected during the winter.

Polio - which we now know to be a communicable viral infection -- spreads more easily when children gather in heterogeneous groups in relatively unsanitary conditions, i.e., it spreads more easily during summer vacation than when the children are in school. The high correlation in no way provided evidence that ice cream consumption causes or promotes polio epidemics.

WVH assert a correlation between SNS handgun availability and homicides through their model and its predictions. It is not the first time ideologically motivated “scientists” have attempted such a connection. Figure 4 (Source **Gun Facts**, Version 3.0, Guy Smith) shows the national supply of handguns per 1,000 people have more than doubled from 150 in 1970 to 325 in 1995 while homicide,



suicide and handgun homicide rates have essentially been constant with fluctuation on the order of 20% and smaller fluctuations for suicides. A positive correlation between gun availability and violence would require increases in violence rates to reflect the growth of gun ownership.

WVH have failed in the most elemental steps of science since they have proposed no controlled study to verify their “correlations”. WVH could have used Great Britain’s ban on handguns as a controlled study, but that would have shown large increases in handgun and non-handgun violence since the ban took effect. In the year (1996) before the total handgun ban Britain had 72,266 robberies. By 2001, there were 95,154 robberies -- an increase of more than 30% (ref. Recorded Crime, England and Wales, 12 Months to March 2001, Dave Povey and colleagues, 19 July 2001). Handguns were used in about two-thirds of these robberies showing criminals don’t obey handgun bans (ref. *Illegal Firearms in the United Kingdom* Report – Overall Executive Summary,

July 2001, King's College London Centre for Defence Studies). While the British have lower levels of homicide than in America, handgun murders in Britain have increased since the ban (in London handgun murders have increased more than 80% in 2001 from the prior year) and by all other measures the British have more violence than Americans. A resident of Britain is more likely to be burglarized, almost twice as likely to be robbed, and two-and-a-half times more likely to be assaulted than a resident of the US.

5. WVH use a statistical forecasting model to predict the trend in firearm murders without a SNS ban. WVH take the model prediction as giving the number of homicides that would have happened absent the passage of the 1988 SNS ban and use that result to determine the lives saved by the ban. All statistical forecasting models have an error structure, where error is defined as the difference between the model prediction and reality. **The fundamental understanding that models are not reality has confounded the WVH authors.** To see the nature of the difference between reality and prediction from WVH’s own data, note Figure 1 for 1978 where their model predicted 5.5 but reality was 4.8. In this case model error is 15% of the observed homicides. Model error arises from a mismatch between model assumptions and reality and from random effects. Model error is not discussed by the WVH and ignoring it is a serious technical deficiency.

Model results deviate from reality for other than random effects, bias error, and in some cases models are just wrong. The existence of bias error in any statistical model is particularly a concern in this case since, as a rule of thumb, Box-Jenkins ARIMA models require 40 to 50 equally spaced periods of data to determine model parameters accurately. And that rule applies when the data is a good fit to the model – the model might not be a good fit. The accuracy of the WVH model fit should be questioned given that it had only 16 time values to determine the model parameters (the model construction had to be limited to 1975 through 1990 for parameter determination step since the model does not fit the observations after 1990). Additionally, the suitability of the model (whether it could fit the data at all) should be question.

WVH claim to have “predicted” age-adjusted firearm homicide rates in Maryland in the absence of the SNS ban². If the WVH model prediction (error) was too high in 1993 as it was in 1978, the 9% decrease from “predicted” to “observed” shown by Figure 1 in 1993 could be due to error. So, can WVH assert that there is not such an error? It is doubtful, since they would require God-like insight into what would have happened without the law’s passage. Since a 15% error happened previously in 1978, it is possible, even likely, for a significant error to have occurred in 1993.

6. The WVH study frequently includes “speculation” without supporting facts. In some cases, the speculations are misleading about the real conditions of homicides. As examples, we see statements like. (p 411):

Many homicides stem from spontaneous altercations that end in gunfire. Ready access to a firearm can increase the lethality of violent altercations because firearms are much more lethal than other personal weapons. Prohibiting the sale of Saturday night special handguns could reduce the likelihood of these fatal encounters by either decreasing handgun ownership (particularly among high-risk persons) by making handguns more expensive or decreasing the incidence of concealed gun carrying.

There are four speculations stated by WVH not supported in their study with data and contradicted by other real world data. These are:

A) *Many homicides stem from spontaneous altercations that end in gunfire.*

"Many" is vague, but Baltimore law enforcement officials estimate that 50 to 100 percent of the city's homicides are related to drug dealing, including violent clashes among competing dealers and buyers and sellers. [Bar Association of Baltimore City. *The Drug Crisis and Underfunding of the Justice System in Baltimore City: Report of the Russell Committee*. 1990. The Russell Committee reported that 55 percent of the city's homicides were drug-related. A higher estimate was given by Baltimore police in 2001, "Almost every one of these people [murder victims] had a criminal record. That's a fact," Detective Carew said. "The bulk of their charges were related to drugs" (see "*Most Baltimore murder victims have criminal records*," John Biemer, ASSOCIATED PRESS, *Washington Times* 12/18/2001)] Drug dealing is not a spontaneous activity such as, say, traffic accidents. Drug transactions and other criminal activities carry risks of violence. While some drug altercations may not be planned, it is hardly fair to characterize resulting violence as spontaneous.

B) *Prohibiting the sale of Saturday night special handguns could reduce the likelihood of ... fatal encounters ...*

Fatal handgun murders have actually increased in Maryland over the studied period from 1988 to 1996 by 68% in numbers and 65% in rates so WVH are speculating contrary to the real world data.

C) *Handgun ownership has been decreased by the SNS ban among high-risk persons because handguns were made more expensive.*

No data is offered concerning handgun prices in the criminal market. No data is shown that ownership of firearms by high-risk persons was decreased in Maryland. Contrary indicators stem from the BATF's Youth Crime Gun Interdiction Initiative (YCGII) Gun Trace Reports for Baltimore where the crime gun trace requests continued to increase 1.8% from 1998 to 1999 (3717 in 1998, 3783 in 1999).

D) *Carrying concealed handguns has been decreased by the SNS ban among high-risk persons.*

WVH have offered no data showing reductions concealed handguns carrying by individuals in Maryland. Increases in Maryland rates of violence using firearms suggest high-risk individuals have increased carrying of concealed handguns.

7. We observed several instances of carelessness in statements by WVH.

WVH say (page 407):

Homicide data for the years 1975-1998 were obtained from the National Center for Health Statistics' multiple cause of death data files using International Classification of Diseases, Ninth Revision, external cause of death codes E960-969.

While it is hard to know what WVH actually used, it is doubtful they included:

Assault by corrosive or caustic substance, except poisoning (E961), assault by poisoning (E962), assault by hanging and strangulation (E963), and assault by submersion (drowning) (E964)

WVH are careless in stating they included these causes.

In discussing handgun sales after the ban went into effect and the bulge in purchases before the ban, WVH say (page 408):

The annual change in per capita handgun sales dropped sharply in the first year the Saturday night special ban was in effect and was 15 percent lower during the entire postlaw period of 1990-1998 than would have been expected with no Saturday night special ban. However, this difference was not statistically significant.

If the difference was not statistically significant, why mention it?

Notice that a 15% decrease in actual handgun sales (relative to predicted) is stated to be not statistically significant, but a 9% decrease in homicide (relative to predicted) is claimed as statistically significant and used to proclaim the SNS ban a success.

8. The WVH modeling approach shows signs of instability that the authors have not recognized. By ignoring the instabilities, WVH have not recognized their impact. This lack of analysis is just one more reason we should have a low confidence in the modeling methodology employed. The instability is identified in their statements (page 409):

An analysis of model residuals revealed the presence of an outlier (year 1976) that, when excluded, had a significant effect on one of the four Saturday night special ban estimates. When the Saturday night special ban effect was assumed to be immediate and constant and the 1976 observation was excluded from the analysis, the ban was associated with a 15.1 percent increase in firearm homicide rates (95 percent confidence interval: 7.1, 23.1), and the preban effect was associated with a 12.1 percent increase (95 percent confidence interval: 5.9, 18.3).

The idea that an event occurring 12 years earlier could somehow affect the SNS ban results by a significant amount in the period 1988 to 1992 confirms the unsuitability of the model being used. Yet, WVH don't realize the instability of their methodology. Rather they attribute the strange results obtained to an outlier in the 1976 data. While outliers can cause problems in data fitting, outlier determination involves more than a model's failure to achieve a desired result. There should be some analysis or visual inspection to convince us that the data is truly an outlier. In this case, the 1976 value nearly equals the 1977 and 1978 values. Examination of the FBI UCR total homicide results show a decrease from 1975 to 1976 followed by two more years of similar homicide numbers. So, it appears that WVH label 1976 as an outlier because it causes their model not to fit the data and not because the 1976 data are sufficiently different enough to qualify as an outlier by objective statistical tests. WVH should have questioned whether the model was suitable given the instability observed, especially given the effect produced so many (12) years later.

¹ And this is not the first time JHU researchers have failed with moral issues in research. Maryland Court of Appeals Judge Dale R. Cathell recently accused JHU officials with purposely misleading participants about the dangers involved in exposing children to lead paint (see **Md. Appeals Court Slams Researchers, Participants in Study on Lead Paint Weren't Informed of Risks, Judge Says**, Manuel Roig-Franzia and Rick Weiss, Washington Post August 21, 2001; Page B01).

Sloppy medical review practice at Johns Hopkins led to the death of a healthy volunteer (Ellen Roche) in a Medical experiment investigating asthma (**Johns Hopkins Admits Fault in Fatal Experiment**, Gina Kolata, New York Times, July 17, 2001 and **Human Research halted in wake of volunteer's death**, Sheila Hotchkin, Associated Press, reported in St. Paul Pioneer Press, July 20, 2001).

In this asthma case the sloppy practice, including not obtaining FDA approval for the drug used, was blamed in part on "Funding Concerns." Bluntly, Johns Hopkins neglects its moral responsibility for the people it uses in experiments and prefers to spend its money generating publishable papers. The moral blindness shown by the quote from WVH continues a pattern of practice by Johns Hopkins employees of using people as Laboratory Rats in social experiments with little concern for the consequences.

² *"Figure 1 contrasts trends in age-adjusted firearm homicide rates in Maryland with the rates predicted by the model that assumed a gradual Saturday night special ban effect beginning in 1990. In the postlaw period, the figure shows the observed rates in relation to those expected in the absence of the Saturday night special ban."* (page 409)