Guns and Homicide: Is the Instrument-Focused Approach to Deterrence Efficacious?

James M. La Valle*

Volume 5 – No. 2 – Fall 2008

* *My deepest gratitude is expressed to Dr. Robert Hanneman from the Sociology Department at the University of California, Riverside, without whose patience, collegiality and expertise this paper would not have been possible*

Abstract

Recent spree-killings have strengthened criminological commitment to resolving the ever-looming question of how best to prevent criminal gun-fatalities (i.e., effective "gun control"?), but the U.S. Supreme Court held in the case of "Heller v. The District of Columbia" that the 2nd amendment guarantees the individual right to "keep and bear arms", a ruling which renders traditional gun-regulation strategies (i.e. the handgun bans, "assault weapons" bans, etc.) both legally tenuous and politically sensitive, but which also then leave sentencing enhancements for gun-crimes as perhaps the most probable alternative gun-intervention. However, a recent National Academy of Science ("NAS") Report has raised technical concerns about much of the most widely cited and extensively reviewed gun-intervention outcome estimates, and it concludes moreover that both the validity and reliability of the results are variously questionable overall. This study, then, adjusts for those technical concerns to re-estimate accordingly the effects that sentencing enhancements for gun crimes may exert on homicide rates and gun-homicide rates, respectively, in 20 major cities across the U.S. between 1970 and 2005, and it finds that one type significantly reduces gun-homicide rates, but that none detectably reduce total homicide rates.

About the Author

Dr, La Valle holds a B.A. in Sociology from California State University, San Bernardino, and both an M.A. in Sociology (1998) and a PhD in Sociology (2004) from the University of California, Riverside. Prior to arriving at Murray State University as an Assistant Professor of Sociology in 2005, he variously taught as an adjunct part-time faculty for California State University, San Bernardino, California Poly-Technic State University, Pomona and University of California, Riverside; all regularly from 1999 - 2005. His areas of specialization are Criminology and Social Theory, and he completed his dissertation under the invaluable guidance of Dr. Austin T. Turk in May, 2004.

Guns and Homicide: Is the Instrument-Focused Approach to Deterrence Efficacious?

Introduction

Recent spree-killings have understandably renewed criminological interest in the persistent question of how best to prevent lethal gun-crime (Wellford, Pepper & Petrie, 2005; Wellford, 2005). As violent and saddening as any of these attacks was the Amish school shooting of October 2, 2006, which claimed the lives of 6 children as well as the life of the assailant. Multiply more innocent lives were lost in the Virginia Tech massacre of April 16, 2007, which claimed 32 students, as well as the life of the mentally disturbed shooter, Seung-Hui Cho, 23. Less than 6 months later on October 7, 2007, an off-duty deputy Sheriff launched a shooting spree in Crandon, Wisconsin, which left 6 innocents dead; and, just 3 days afterwards, 14-year old student Asa Coon shot 2 teachers and 2 students in Cleveland, Ohio before turning the weapon on himself fatally (Imrie, 2007). More recently, Robert Hawkins, 19, opened fire in a Nebraska mall as it was opening for business on the morning of December 6th, 2007 – tragically killing 6, and then himself (Garcia, 2007). Finally, on St. Valentines day, 2008, Stephen Kazmierczak, 27, leapt from behind a stage during a course lecture at Northern Illinois University wielding a shotgun and two handguns, from which he reportedly fired a total of 54 rounds to kill 5, and then himself (Rosseau, 2008).

An especially hopeful legislative response to these and other recent spree killings was a bill proposing to expand the investigational scope of background checks for handgun purchases nation-wide to include the *medical-psychiatric history* of prospective firearm purchasers (Abrams, 2007), which resulted in the expansion of the previous national list of 175,000 "mental defective's" to over 400,000 names, and also in an increase in the list of states contributing mental health data to the FBI from 23 to 32 (Eggen, 2007). Predictably, though, the bill itself was held up in congress for several months (even though it was ironically co-sponsored by gun-policy rivals Chuck Schumer on the one hand, and the NRA on the other) due to a concern by some that it did not adequately provide for legal challenges by those perhaps wrongly classified as "mental defective", a provision which was finally included in the bill in November, 2007, thus clearing the way for its' final passage just before the end of that same year (Eggen, 2007).

However, gun-policy in the U.S. overall appears to be trending increasingly toward successful legal challenges to long-standing gun-regulations. Even one of the oldest and most restrictive standing gun laws in the entire continental U.S., the "DC gun-ban" of 1975, was successfully challenged in the circuit court of appeals in February, 2007. A rather dramatic outcome of this decision is that the U.S. Supreme Court subsequently held that the 2nd amendment guarantee's an *individual* right to "keep and bear arms" (Sherman, 2008). Wellford (2005) somewhat prophetically observed previously that these types of legal decisions are most likely due to the broader perception fact that "the second amendment of our constitution is being interpreted to grant an individual right to possession of firearms" (P. 678). The present study, then, holds accordingly that outcome estimates of *gun-policy interventions should place additional emphasis on the most common alternative to gun-regulations, which are sentencing enhancements for gun crimes* (Cook & Nagin, 1979; Loftin, McDowall & Wierserma, 1981). Supporting this shift in emphasis, Piquero (2005) observes moreover in his independent evaluation of the present state of gun-policy outcome research literature that "*there [remains] a pressing need for outcome evaluations of firearms enhancement laws at the city level*" (p. 784). A National Academy of Sciences Report (Wellford, et. al., 2005), however, recently concluded that the results of the bulk of the past gun-policy outcome analyses in general are variously invalid and unreliable due to methodological limitations within the research designs themselves (Wellford et. al., 2005, p. 120-151, 223-230). Some take the implications of this report one step further to suggest that the technical problems detailed therein are so limiting and extensive that traditional research methods should be discarded altogether in favor of alternate yet still unarticulated methods; or, alternately, that gun-policy research focus overall should shift away from mainly "supply side" (e.g., background checks, licensing, etc.) and toward greater emphasis on "demand side" (e.g., disrupting illegal gun-markets, tracking and investigating "extra-legal" gun-purchasing patterns, etc.) outcome evaluations (Wellford, 2005, p. 673-676).

The methodological critiques presented by the NAS Report are not here disputed, and neither is the need for more "demand side" outcome evaluations; explicitly to the contrary *on both counts*, but perhaps there is no need for alternate research methods since practical refinements can be made to present research methods which can effectively resolve practically all of the technical deficiencies summarized and detailed by the NAS Report (Wellford et. al., 2005, p. 120-151, 223-230). In general outline, the NAS Report (Wellford, et. al., 2005) raised serious concerns about (a) unacceptably high levels of aggregation such as states or counties, (b) reliance upon observably *unreliable* county-level data (see also Maltz and Targonski, 2002), (c) artificial statistical confidence produced by large numbers of necessarily non-independent units contained within larger jurisdictions (i.e., samples containing large numbers of adjoining and necessarily non-independent counties used to evaluate the impact of state laws on county-level outcomes), (d) the sensitivity of gun-policy effects to overall model specification (i.e., the magnitude, direction and significance of beta's frequently change drastically in response to the removal, alteration or introduction of just a few covariates or controls), (e) questionably short post-intervention periods, and finally, (f) wide variation in statistical techniques of estimation among the various gun-policy researchers (Wellford et. al., 2005, p. 120-151, 223-230).

The present study, then, resolves the variously inter-related problems with unacceptably high levels of aggregation, reliance on unreliable county-level data and too many non-independent observations by conducting a city-level analysis which limits as much as possible the total number of non-independent units, and one which also balances as evenly as possible the various "treatment groups" with "non-treatment" groups (table 1; but see Babbie, 2007, p. 359 for support for this approach). The problem with model specification sensitivity is resolved by developing model specifications according to the homicide literature exclusively, and by then estimating the effects of the interventions on rates of homicide and gun-homicide exclusively. To adjust for the problem of questionably short post-intervention periods, the present study extends them well beyond any yet estimated. Rather than attempting to resolve the apparently unresolvable question of which type of statistical technique is most appropriate for gun-policy outcomes estimates (see Wellford, et. al., pp. 223-230), the present study estimates and compares mutually supplementary multi-variate estimates. To be sure, the National Academy of Sciences Report (2005) *"recommends more rigorous study of firearms sentencing enhancement laws at the city-level* [italicized emphasis added]" (p. 229).

<u>Literature Review</u>

Cook and Nagin (1979) were perhaps the first to establish preliminarily, at least, (using actual case information) the need for gun-specific criminal justice enhancement policy; they concluded that criminals once convicted of gun crimes such as robbery and assault were detectably more likely to recidivate with firearms, and that offenders found guilty of committing these types of crimes with firearms should then be eligible for stiffer sentences since the use of a firearm increases substantially the probability of the victim's death (see also Wellford et. al., 2005, p. 223 for a much more comprehensive review).

Somewhat troubling, however, is that over 25 years of methodologically sound sentencing enhancement outcome estimates have failed overall to resolve in the collective mind of academia whether sentencing enhancements actually reduce lethal crime rates (Kleck, 1997; Naigin, 1998; Wellford et. al., 2005). In general, though, early sentencing enhancement estimates generally tended to report significant deterrent effects resultant of these types of strategies, whereas later studies typically reported that they are not effective, thus rendering the results overall both mixed and difficult to interpret (Wellford, et. al., 2005).

Deutsch (1981), for example, expanded an earlier study of the "Bartley Fox" law (Deutsch & Alt, 1977) which mandated stiffer penalties for illegal public gun-carrying, and which was enacted in its fully amended form in Massachusetts in 1975, to report that it significantly reduced homicide, gun-assaults and armed robbery. Similarly, Pierce and Bowers (1981) confirmed the findings of Deutsch et. al. (1977) and Deutsch (1981) by comparing Boston's violent crime rates to an extensive array of other cities across the U.S.

However, a series of equally well done, widely cited and extensively reviewed studies which were conducted by Loftin, McDowall and Wierserma (1981, 1983, 1984, 1992) concluded differently. They first estimated statistically the outcomes of sentencing enhancement strategies in Michigan (Loftin et. al., 1981); their analysis concluded overall that a subsequent drop in homicide rates in Detroit was most likely due to other factors, and their subsequent study using the same data (Loftin et. al., 1983) provisionally reaffirmed their previous (1981) conclusions. Their next study (1984) estimated the effects of a Florida sentencing enhancement law passed in 1975 which mandated a minimum 3-year prison term for anyone committing or attempting to commit a number of different felonies while in possession of a firearm (see also Wellford et. al., 2005:224-225 for a more comprehensive review). They (1984) failed to detect statistically significant reductions in violent crime in Jacksonville and Miami, but in the case of Tampa, they detected (a) decreased rates of firearm-related homicides but (b) increased rates of firearm-related assaults, which rendered interpretation of the results overall understandably difficult.

After pooling all of the time-series analyses from both studies, they (1984) concluded that the laws were not effective overall. The final study in the series (McDowall et. al., 1992) conducted a meta-analysis by combining all of the aforementioned data with additional data from a subsequent study of Pennsylvania Sentencing enhancements which sampled Philadelphia and Pittsburgh, and the results reaffirmed that sentencing enhancement strategies are most likely not effective overall (see Wellford et. al., 2005, p. 224-225; Marvell & Moody, 1995; Kleck, 1997, p. 353 for much more comprehensive reviews). It is possible – as the National Science Academy Report generally suggests – that the variation in results between Deutsch and Alt (1977), Deutsch (1981) and Pierce and Bowers (1981) on the one hand, versus Loftin et. al. (1981, 1983, 1984 and 1992) on the other, may perhaps be a result of variations in the length of the post intervention periods, disparate model specifications, or differences in the statistical techniques of outcome estimation employed by the researchers themselves.

In an attempt to better account for potentially confounding influences on violent crime and also to estimate an unusually large sample, Kleck (1991) developed and executed a cross-sectional design using 1980 data for 170 sample cities. Controlling for an extensive array of other possible causes of violent crime, Kleck (1991) found that sentencing enhancements were not statistically related to assault, robbery or homicide rates. The National Science Academy Report (2005), however, observes that cross-sectional research designs may obscure otherwise statistically detectable effects from interventions because “it is difficult to be confident that the control variables account for the difference between cities that may mask the laws impacts.” (225). Marvell and Moody’s (1995) lagged time-series analysis estimated the effects sentencing enhancements may exert on a wide-range of index crimes and prison sentences, including those for homicide, and their data reported that sentencing enhancements fail to exert statistically detectable effects on either type of outcome. Importantly, however, Kleck’s (1991) research design is exclusively cross-sectional, and therefore does not include a post-intervention period, and Marvell and Moody’s (1995) analysis is conducted at the state-level of aggregation, both evidently reasons for concern according to the NAS Report (Welford, et. al., 2005).

Sentencing Enhancements

Sentencing enhancements for gun crimes variously provide for (a) mandatory minimum sentencing for crimes committed with guns (and sometimes also other weapons), (b) additional jail time “added-on” for gun crimes, or (c) both; and some additionally stipulate (d) a “non-suspendable” clause for mandatory minimums (Marvell and Moody, 1995: 259-260). The present sample consists of 20 of the largest cities across the U.S., which collectively represent 7 discrete configurations of sentencing enhancements. Every city in the sample except New York (see table 1) became subject to some overall configuration of sentencing enhancements at some point in time between June, 1972 and July, 1984. Sentencing enhancements for Los Angeles, San Diego, San Francisco San Jose, Memphis, Nashville, Baltimore, Boston, Columbus and Detroit mandate "gun only", whereas Austin, Dallas, El Paso, Houston, San Antonio, Chicago Milwaukee, Phoenix and Seattle include other weapons such as knives or clubs. Each city stipulates "mandatory" minimums for crimes committed with guns or other weapons, but Memphis, Nashville, Baltimore, Boston, Columbus, Detroit, and Seattle also mandate "add-on" sentences for gun crimes, whereas Los Angeles, San Diego, San Jose, San Francisco, Houston, Dallas, Austin, San Antonio and El Paso additionally impose "non-suspendable" mandatory minimums. The first of these to become subject to sentencing enhancements was Baltimore (6/72), followed then by Phoenix (8/74), Boston (4/75), Los Angeles, San Diego, San Jose and San Francisco (1/76), Memphis and Nashville (7/76), Detroit (1/77), Houston, Dallas, San Antonio, Austin and El Paso (8/77), Chicago (2/78), Milwaukee (3/80), Columbus (1/83), and finally, Seattle (7/84). (Marvell & Moody, 1995, p. 259).

Table 1 – Effective Dates and Sentencing Enhancement Laws for each City (*Sequenced by population size)

City	Effective Date	Gun Only	Mandatory Minimum	Add-On	Non-Suspendable
**New York	**6-13-80	No	No	No	No
Los Angeles	1-1-1976	Yes	Yes	No	Yes
Chicago	2-1-1978	No	Yes	No	No
Houston	8-29-1977	No	Yes	No	Yes
Phoenix	8-9-1974	No	Yes	No	No
San Diego	1-1-1976	Yes	Yes	No	Yes
Dallas	8-29-1977	No	Yes	No	Yes
San Antonio	8-29-1977	No	Yes	No	Yes
Detroit	1-1-1977	Yes	Yes	Yes	No
San Jose	1-1-1976	Yes	Yes	No	Yes
San Francisco	1-1-1976	Yes	Yes	No	Yes
Columbus	1-5-1983	Yes	Yes	Yes	Yes
Austin	8-29-1977	No	Yes	No	Yes
Baltimore	6-1-1972	Yes	Yes	Yes	No
Memphis	7-1-1976	Yes	Yes	Yes	No
Milwaukee	3-1-1980	No	Yes	No	No
Boston	4-1-1975	Yes	Yes	Yes	No
El Paso	8-29-1977	No	Yes	No	Yes
Seattle	7-1-1984	No	Yes	Yes	No
Nashville	7-1-1976	Yes	Yes	Yes	No
Summary	***6-72 / 7-84***	***10Y / 10N***	***19Y / 1N***	***7Y / 13N***	***10Y / 10N***

Source: Marvell & Moody, 1995, p. 259-260 * According to 2000 U.S. Census
** Marvell and Moody (2005:259) observe that while New York does have "sentencing enhancements on the books", they are not interpreted or applied in the true spirit of their underlying principles, thus they should not be analytically or empirically considered as such.

Homicide

Homicide rates vary considerably within the sample, both longitudinally and cross-sectionally. The highest homicide rate reported by any city included in the sample for any year included in the series, for example, was Detroit, which reported exactly 60 homicides per 100,000 of the population in 1989, whereas the lowest was El Paso, which reported 2.1 homicides per 100,000 in 1999. Similarly, the highest gun homicide rate reported by any city in the sample was also Detroit, which reported a gun-homicide rate of 52.3 per 100,000 of the population in 1993, whereas the lowest was San Jose, which reported a practically negligible gun-homicide rate of 0.6 in 2000. Homicide rates in the sample rose precipitously in the early 1970's, generally peaked overall in the late 1980's and early 1990's, and then steadily declined through the end of the century before tapering slightly upward through 2005. The average homicide rate for the overall sample is 19.3, whereas the average gun-homicide rate for the overall sample is 12.9 (*Uniform Crime Reports* / Crime in the United States, 1970-2005).

Homicide is also the index crime for which firearms have the greatest frequency of involvement overall; only about 20% of all robberies annually and only about 10% of all rapes annually, as examples, involve or are otherwise committed with firearms, whereas guns were the type of weapon used to commit approximately 69% of all homicides reported nationally in the U.S. during the final three decades of the twentieth century (Uniform Crime Reports / *Crime in the United States*, 1970- 2005). In addition, Government Crime Reports only record the most serious crime in cases of multiple offenses, which means that official measures of homicide and gun-homicide capture – literally by definition – murders committed during the course of other felonies such as rape, robbery, felony theft etc. ("felony murder"), an incident rate which should theoretically decrease if criminals are more reticent to use firearms to commit such crimes since guns are presumably more lethal than other types of weapons. Thus, sentencing enhancements should theoretically exert stronger, more significant and more reliable effects on homicide rates and gun-homicide rates as compared to other index crimes such as, say, rape , robbery or even burglary.

Data Collection

The data for the present study were obtained from Uniform Crime Reports / Crime in the United States Data (1970- 2005), the National Archives of Criminal Justice Data (henceforth, "NACJD"), the U.S. Census (1970, 1980, 1990, 2000 & 2005) and the U.S. Census of Retail Trade (1967, 1972, 1977, 1982, 1987, 1992, 1997 & 2002).

Population figures, square mile area and the number of law enforcement personnel were all available for each year included in the series, whereas percent of population not highschool graduated, percent of families living in the lowest 20 percent income bracket, percent of population renting, percent of population Black and percent of population Hispanic were available for 1970, 1980, 1990, 2000 and 2005 only; alcohol outlet figures were similarly available for 1967, 1972, 1977, 1982, 1987, 1992, 1997 and 2002 only; the missing data, then, were interpolated according to the direct linear function method detailed by Yaffee (2000, p. 3) who explains that "missing value replacement...applies one-step ahead forecasting from the previous observation." (See appendix).

Table 2 – Univariate Descriptive Statistics for 1970 - 2005 U.S. Census Data *W/O Interpolated Data*

Variable	*Mean*	*Standard Deviation*	*Skewness*
Number of residents per square mile	1.53	.836	.182
Percent of population renting	3.92	.191	.206
Number of patrol officers	7.72	.986	.932
Patrol officers per sq. mile	2.38	1.15	.266
Patrol officers per resident	.870	.389	.186
% of population below poverty line	2.77	.307	-.463
Families in lowest 20% income	3.12	.292	-.416
% of population with less than HS	3.35	.415	-.221
% of population Black	2.81	.877	-.266
Gun homicide rate	2.12	.827	-.399
Total homicide rate	2.61	.727	-.453

All variables logged to normalize distributions (N=100)

Table 3 – Univariate Descriptive Statistics for 1970 - 2005 for Census Data *W/ Interpolated Data*

Variable	*Mean*	*Standard Deviation*	*Skewness*
Number of Bars	5.66	.864	.407
Number of liquor stores	4.95	.891	.652
Number of Bars per square mile	.337	1.19	.002
Liquor stores per square mile	.370	1.05	.103
* Number of residents per sq. mile	6.51	5.72	1.58
* Percent of population renting	51.88	9.94	.537
Number of patrol officers	7.73	.954	1.07
Patrol officers per sq. mile	2.32	1.15	.452
Patrol officers per resident	.882	.374	.224
% of population below poverty line	2.80	.288	-.469
Families in lowest 20% income	3.13	.277	-.561
% of population with less than HS.	3.36	.348	-.390
% of population Black	2.83	.866	-.278
% of population Hispanic	2.29	1.33	-.555
Gun homicide rate	2.18	.605	-.478
Total homicide rate	2.68	.662	-.357

* Non-logged variable (N=720)

Methods and Statistics

The present study estimates two different pairs of pooled "fixed effects" GLS time-series analyses, both incorporating heterogeneous first-order auto-regressive covariance structures (ARH-1)*, one exclusive of the interpolated observations* (see tables 6 & 7), and another *inclusive of the interpolated observations* (see tables 8, 9 & Appendix); all to estimate the effects sentencing enhancements may exert on homicide rates and gun-homicides rates, respectively, from 1970 through 2005 in 20 of the largest cities across the U.S. (see table 1). The pooled analysis is preferred over an interrupted type to (a) maximize the number of observations invested in each parameter estimate (which is important given that data-availability restricted somewhat the size of the present cross-section), (b) the GLS type is preferred over the GEE type due to superior fit statistics; (c) the "fixed effects" technique over "random effects" due to the non-random (quasi-systematic) sample; (d) first order auto-regression to account for observably serial correlated error terms; and, (e) the heterogeneous type over the homogeneous type due to higher "rho" statistics. It should be noted, though, that GEE types and even GLS types with alternate covariance structures – diagonal, homogeneous, etc.– yielded strikingly similar results as compared to those presently reported.

The interpolated estimates are intended to be progressive-supplemental with respect to the former, which means that only the *interventions* that both (a) *maintain their effects directionally and also* (b) *report statistical significance with the introduction of the interpolated observations*, *at least*, evidently meet the methodological standards of robustness set by the present study (Wellford, et. al. p. 139-150; but see importantly Webster, Doob and Zimring, 2006, for support for this general approach). Importantly, then, "ALCOHOL" and population Hispanic are not estimated in the models exclusive of the interpolated observations due to "odd" census years for alcohol outlet data (1967, 1972, 1977, 1982, etc.), and no 1970 census figures for percent of population Hispanic, respectively. The quasi-systematic sample (20 of the largest cities in the U.S.) was chosen over a random type (which would have surely included some number of much smaller cities) to (a) maximize sample cross-section, and to (b) extend the time series over a period of time sufficient to adequately capture all of the requisite pre-intervention trends and patterns; *gun-homicide data* is largely unavailable for smaller cities before the early 1980's, and that which exist is also observably less reliable than those for the cross-section presently employed.

Model Specification

The uni-dimensional variables used to develop and produce the covariates and controls have been provisionally confirmed to exert statistically significant effects on homicide rates at various levels of aggregation (Almgren, Guest, Immerwahr and Spittel, 1998; Cao, Adams and Jensen, 1997; Covington, 1999; Crutchfield, 1989; Grant, Sherman, and Martinez, 1997; Murray, 1975; Parker, 1995), and *all but Percent Hispanic and percent Black have been factor-analyzed* (see tables 4 & 5) to construct combination indexed variables where it was both theoretically probable (e.g., % of population renting + residence per square mile = "DENSITY") and statistically acceptable (factor loadings > .70). The resulting indexed constructs variously measure (a) alcohol availability, (b) population density, (c) police presence, and (d) poverty levels.

<u>Table 4 – * Factor Indexed Compositions</u> *<u>Without Interpolated Data</u>*

Variable	Factor Loading	Alpha Coefficient	Factor Indexed Variable
Residents per square mile	.928		
% of population renting	.928	.838	"DENSITY"
Number of patrol officers	.892		
Patrol officers per resident	.931		
Patrol officers per sq. mile	.944	.912	"POLICE"
% of population below poverty	.981		
Families in lowest 20% income	.981	.960	"POVERTY"

* "Principle components" with verimax rotation method
Alcohol variable(s) excluded due to nonconcurrent census schedule – "odd" vs. "even" years
Percent of population Black not factor analyzed

<u>Table 5 – * Factor Indexed Compositions</u> *<u>With Interpolated Data</u>*

Variable	Factor Loading	Alpha Coefficient	Factor Indexed Variable
Number of Bars	.903		
Number of liquor stores	.814		
Number of Bar's per sq. mile	.873		
Liquor stores per sq. mile	.920	.900	"ALCOHOL"
Residents per sq. mile	.942		
% of population renting	.942	.872	"DENSITY"
Number of patrol officers	.903		
Patrol officers per resident	.929		
Patrol officers per sq. mile	.913	.903	"POLICE"
% of population below poverty	.962		
families in lowest 20% income	.950		
% of pop. with less than HS	.699	.806	"POVERTY"

* "Principle components" with verimax rotation method
Percent of population Hispanic and percent of population Black not factor analyzed

Table 6 – “Fixed” GLS (ARH-1) **Gun Homicide Model** *W/O Interpolated Observations 1970 - 2005*

Variable	Estimate	Standard Error
Intercept	.512	.380
ManMin	.299 *	.174
AddOn	-.594 **	.265
NonSusp	.278	.212
GunOnly	.274	.204
DENSITY	-.070	.133
POLICE	.013	.142
POVERTY	.099	.078
LogBlak	.836 **	.122

** P < . 05 (two-tailed) * P < .05 (one-tailed) *LL* = 151.17 ARH - 1 = .55

Table 7 – “Fixed” GLS (ARH-1) **Total Homicide Model** *W/O Interpolated Observations 1970 - 2005*

Variable	Estimate	Standard Error
Intercept	-.389	.408
ManMin	.320 **	.147
AddOn	-.678 **	.239
NonSusp	.198	.196
GunOnly	.459 **	.202
DENSITY	.231	.140
POLICE	-.317 **	.143
POVERTY	.014	.076
LogBlak	.915 **	.133

** P < . 05 (two-tailed) * P > . 05 (one-tailed) *LL* = 123.06 ARH - 1 = .75

Table 8 – “Fixed” GLS (ARH-1) **Gun Homicide Model** *With Interpolated Observations* 1970 - 2005

Variable	**Estimate**	**Standard Error**
Intercept	.729 **	.141
ManMin	-.069	.062
AddOn	-.229 **	.084
NonSusp	.073	.072
GunOnly	.137 **	.055
ALCOHOL	-.036	.034
DENSITY	-.265 **	.031
POLICE	.358 **	.036
POVERTY	.265 **	.023
LogBlak	.510 **	.039
LogHisp	.013	.019

** P < . 05 (two-tailed) *P < .05 (one-tailed) *LL* = 261.61 ARH - 1 = .85

Table 9 – “Fixed” GLS (ARH-1) **Total Homicide Model** *With Interpolated Observations* 1970 - 2005

Variable	**Estimate**	**Standard Error**
Intercept	2.086 **	.290
ManMin	.065	.090
AddOn	-.181	.128
NonSusp	-.155	.108
GunOnly	.020	.087
ALCOHOL	.110 *	.060
DENSITY	-.163 **	.063
POLICE	.160 **	.058
POVERTY	.213 **	.046
LogBlak	.274 **	.078
LogHisp	-.082	.041

** P < . 05 (two-tailed) * P . < .05 (one-tailed) *LL* = - 318.99 ARH - 1 = .90

The advantages of this approach are that it (a) allows for easy comparisons of beta's without "standardizing" them conventionally, (b) reports statistically the reliability of the underlying constructs, (c) saves degrees of freedom, (d) reduces substantially the total number of beta's, and (e) simplifies interpretation of the results overall. Tables 4 and 5 list (a) which variables are included with which factor-indexed composites, (b) the factor loadings for each, (c) the resulting alpha coefficients, and (d) the various labels assigned to each factor indexed composite. Similarly, (a) mandatory minimums for gun-crimes = "ManMin", (b) add-on sentences for gun-crimes = "AddOn", (c) non-suspendable mandatory minimums = "NonSusp", and (d) enhancements for guns only (but not other types of weapons) = "GunOnly". Finally, ANCOVA reported no interactions among any of the treatments, covariates or controls, thus none are presently estimated.

Discussion

Alcohol outlet availability is, in the main (but 'one tailed' in the total homicide model, see table 9), insignificant statistically, which is perhaps not as surprising as it may first appear since (a) the alcohol-homicide research frequently estimates the effects of alcohol consumption on homicide rates at even lower levels of aggregation ("neighborhoods"), and also (b) typically specifies overall models with somewhat different covariates and controls than the present study (Parker, 1995, p. 80-101). The percent of the population Hispanic, too, is reported to be statistically insignificant, a result which is most likely a function of the perhaps overly-inclusive U.S. census definition of "Hispanic", which problematically subsumes a wide range of "Hispanic" classifications – Puerto Rican, Cuban, Mexican etc.; each of which discretely report somewhat different police contact and incarceration rates (Uniform Crime Reports). It is also possible that recent increases in Hispanic urban populations (which were the largest over time of any of the variables used in the present study, and by a fairly wide margin – see appendix) simply do *not* portend higher violent crimes rates as some may perhaps erroneously presume (Sampson, 2007).

Population density and Police presence are both statistically significant *inclusive of the interpolated observations*, but both in the opposite of the expected direction; negative and positive, respectively. The former is most likely a result of the somewhat restrictive quasi-systematic sample presently employed, whereas the latter is perhaps more likely a result of endogeneity; the present sample includes only rather densely populated (large) cities which, in turn, heavily skewed the distribution of “residence per square mile” upward (table 2 & 3), whereas increasing the size of police forces is a common response to increasing violent crime rates. Importantly, however, both poverty levels and the percent of the population and the percent of the population Black are statistically significant, too, and in the same (generally expected) direction as the reported estimates *inclusive of the interpolated observations* (tables 8 and 9); combined results which are clearly consistent overall with a substantial body of existing homicide research (Almgren, et. al., 1998; Cao, et. al., 1997; Covington, 1999; Crutchfield, 1989).

Sentencing enhancements for the commission of crimes with guns only (and not other types of weapons) is found to be statistically significant in the gun-homicide model, at least, but in the opposite of the expected direction, a result which may also be the result of endogeneity. The most substantively important result overall, however, is that the only statistically significant policy intervention and in the expected direction is additional jail time added-on for gun crimes), but it is only “one-tailed” significant, *and for the gun-homicide model exclusively, results* which overall clearly warrant further discussion, especially with respect to future policy implications.

Conclusions and Policy Implications

The present study conditionally holds that the methodological refinements suggested by the National Science Academy Report (2005) do not *alter substantively* the results of sentencing enhancement estimates, but the results overall do contribute to the present state of scientific knowledge of the matter in at least one important but nonetheless ironic way; one type of sentencing enhancement – jail time added-on for gun crimes – perhaps reduces *gun-homicide rates*, but curiously, *not total homicide rates*. In the narrowest sense, then, adding on jail time for gun-crimes (vs. non-gun crimes) may be considered "successful" policy if the only (limited) goal is to reduce the frequency with which crimes are committed with guns. However, the original assumption underlying the instrument-focused approach to deterrence is that guns are more lethal than other weapons, and are therefore more likely to result in fatal outcomes as compared to crimes committed with other presumably less lethal weapons such as, say, clubs or knives, yet these data unambiguously disconfirm this basic assumption. The present study, then, provisionally disconfirms the underlying assumption behind the instrument-focused approach to deterrence, which is that the presence of a firearm in violent or criminal encounters is actually more likely to lead to fatal outcomes.

Future policy attempts to reduce criminal fatalities should perhaps then consider (a) the possibility that the absence of a gun may actually promote resistance by the victim, thus frequently concluding confrontations with much the same tragic result as if a firearm were otherwise involved, and also (b) the possibility that judicial plea-bargaining, as it is currently practiced in the courts, may effectively nullify the effects of sentencing enhancements for gun-crimes. It is not generally disputed, for example, that courts will sometimes reduce sentences in exchange for guilty pleas, ergo, accelerated disposition of otherwise costly and time-consuming criminal trials. Thus, it is easy to see how judicial plea bargaining may readily dismiss firearm-use charges against defendants, thereby canceling the deterrent effects of sentencing enhancements for gun-crimes. Relatedly, Marvell and Moody (1995) reported that sentencing enhancements for gun-crimes are not significantly related to the overall length of prison sentences, which combines with the present study to suggest that the manner in which the criminal justice system works to expedite the adjudication of criminal cases through plea-bargaining may perhaps nullify the effects of sentencing enhancements for gun crimes. It is also possible that the absence of a gun in criminal confrontations actually promotes resistance by the victim due to a (evidently wrong) perception by said victim that, say, "mere" knives and clubs can be effectively overcome through physical resistance, thus increasing the probability of "real" violence, ergo, tragically fatal outcomes. The present study provisionally holds, then, that perhaps mere "instrumentality" is less pivotal to the outcome of criminal or violent encounters than the methods, attitudes and pre-dispositions of the "principles" involved.

Appendix

The straight linear function method of interpolation is presently acceptable for at least three reasons. First, all five of the component variables for which only five data points – 1970, 1980, 1990, 2000 & 2005 – are available reported relatively little variation between any two consecutive census years covered by the series. The cities in the sample reporting the highest rate of variation between any two consecutive census years included in the series, for example, were the percent of the population Black in Detroit, Michigan, which rose by a total of 15.5%, from 44.5% to 60%, or an average of 1.6% annually between 1970 and 1980, followed by the percent of the population Hispanic in Phoenix, Arizona, which rose by a total of 14.4% between 1990 and 2000, from 19.7 to 34.1%, or an average of 1.4% annually, whereas all but 2 other longitudinal lines of observations reported increases or decreases of less than 10% between any two consecutive census years covered by the series. *The bulk of the total variability for all of the specified demographic and structural variables, then, is cross-sectional rather than longitudinal.* Second, there is no theoretical reason to expect that demographic *or* structural variables measured in annual percentages could have unpredictably or abruptly spiked or dropped at any point during the period covered by the series, or between one observation and the next therein. Overall trends in the percent of a population Hispanic, or the percent of a population renting, as examples, would not have abruptly spiked unless a city is subject to, say, a mass exodus or influx of specific populations resultant of, perhaps, an unanticipated socio-economic or natural catastrophe (e.g., hurricane “Katrina”).

Finally, the directional trends for all of the census variables were also cross-sectionally similar, thus suggesting *directional consistency with known and demonstrably stable regional and* *national-level trends.* The percent of the population with less than a highschool education, as an example, declined between 1970 and 2005 for all 20 cities included in the sample, whereas the percent of the population Hispanic rose between 1970 and 2005 for all 20 cities included in the sample. Similarly, the percent of the population in the national lowest 20% income bracket increased in 15 out of the 20 cities included in the sample, whereas the percent of the population living below the poverty line increased in 14 out of the 20 cities included in the sample.

Alcohol outlet figures were similarly projected for the missing years by averaging the unit differences between the reported years and estimating the missing observations accordingly, and the characteristics of these variables were similar to those of the aforementioned: The number of bars declined markedly between 1970 and 2005 for all but one city in the sample – Austin, Texas, but that number rose by only 17 outlets during the same period, and the number of liquor stores declined overall between 1970 and 2005 for all but two cities – Austin, Texas, which saw a mere 15 outlet increase overall between 1970 and 2005, and Seattle, Washington which saw a virtually negligible 1 outlet overall increase during that same period. Both alcohol outlet density and the number of patrol officers per square mile were calculated by dividing the number of patrol officers and the number of alcohol outlets, respectively, by the square mile area of the city. The percent of population Hispanic, however, was not censussed in 1970, thus these values were estimated and assigned by retro-projecting the missing values according to the subsequent census report figures included in the series – 1980, 1990, 2000 and 2005.

References

Almgren, Gunar, Guest, Avery., Immerwahr, George & Michael Spittel (1998). Joblessness, family disruption, and violent death in Chicago, 1979-1990. *Social Forces, 76 (4).* pp. 1465-1494.

Babbie, Earl. (2007). *The Practice of Social Research*. Wadsworth: U.S.A.

Cao, Liquin, Adams, Anthony & Vicki Jensen (1997). A test of the Black subculture of violence thesis: A research note. *Criminology, 35 (2),* pp. 367-379.

Census of Retail Trade. (1967, 1972, 1977, 1982, 1987, 1992, 1997, 2002). Retrieved April, 2006 *from http://www.columbia.edu/cu/lweb/indiv/usgd/ecococensus/html*

Cook, Phillip & David Nagin (1979). Does the weapon matter? An evaluation of weapons-emphasis policy in the prosecution of violent offenders. Washington, D.C.: INSLAW.

Covington, Jeanette (1999). African American communities and violent crime: The construction of race differences. *Sociological Focus, 32 (1),* pp. 7-24.

Crutchfield, Robert (1989). Stratification and violent crime. *Social Forces, 68 (2),* pp. 489-512. Deutsch, Stuart J. (1981). Intervention modeling: Analysis of changes in crime rates in James A. Fox (Ed.) *Methods in Quantitative Criminology* (pp. 171-193).

Deutsch, Stuart & Francis Alt (1977). The effect of Massachusetts' gun control on gun-related crimes in the city of Boston. *Evaluation Quarterly, (1),* pp. 543-568

Eggen, David (2007). FBI's gun ban listing swells: Thousands added to file marked 'mental defective'. (November 30) Washington Post.

FBI (1970-2005). Uniform Crime Reports: Crime in the United States. Washington, DC: U.S. Department of Justice.

Garcia, Oskar (2007). 6 employees among dead in mall shooting. (December 7) Associated Press. Grant, David, Sherman, Don & Ramiro Martinez, R. (1997). Crime and restructuring of the U.S. economy. *Social Forces, 75 (3),* pp. 769-798.

Imrie, Robert (2007). Off-duty Wis. deputy sheriff kills 6. (October 7) Associated Press.

Kleck, Gary (1997). *Targeting guns: firearms and their control*. New York: Adlyn De Gruter.

Loftin, Colin & David McDowall (1981). One with a gun gets you two: Mandatory sentencing and firearms violence in Detroit. *Annals, 455,* pp. 150-167.

Loftin, Colin, McDowall, David & Brian Wierserma (1983). Mandatory sentencing and firearm violence: Evaluating an alternative to gun control. *Law & Society Review, 12,* pp. 287-318.

Loftin, Colin & David McDowall (1984). The deterrent effects of the Florida felony firearm law. *Journal of Criminal Law and Criminology, 75,* pp. 250-259.

Ludwig, Jens (2005). Better gun enforcement, less crime. *Criminology and Public Policy, 4 (4),* pp. 677-716.

Maltz, Michael & Joseph Targonski (2002). A note on the use of County-level UCR data. *Journal of Quantitative Criminology, 18 (2), pp.* 297-318.

Marvell, Thomas & Carlisle Moody (1995). The impact of enhanced prison terms on felonies committed with handguns. *Criminology, 33,* pp. 247-281

McDowall, David, Loftin, C & Brian Wierserma (1992). A comparative study of the preventative effects of mandatory sentencing laws for gun crimes. *Journal of Criminal Law and Criminology, 83*, pp. 378-394

Nagin, David, S (1998). Criminal deterrence at the outset of the twenty-first century. Volume 23 *Crime and justice: A review of research.* In Michael Tonry (Ed.). Chicago: University of Chicago Press.

National Archives of Criminal Justice Data. Retrieved March, 2003 *from http://www.icpsr.umich.edu/NACJD/bjs_pubs*.

Parker, Robert, Nash (1995). *Alcohol & Homicide: A deadly combination of two American traditions*. New York: State University Press.

Pierce, Glenn & William Bowers (1981). The Bartley-Fox laws short-term impact on crime in Boston. *Annals*, *455,* pp. 120-37.

Rousseau, Crystal (2008). Man kills self, 5 at Northern Illinois. (November 14) Associated Press.

Sampson, Robert (2007). Does Mexican immigration reduce crime? Retrieved March, 2006 *http://www.marginalrevolution.com/marginalrevolution/2006/03/does_mexican_im.html*

Sherman, Michael (2007). Supreme Court will hear D.C. gun case. Retrieved November, 2007 *from http://apnews.myway.com//article/20071120D8TIJ9K80.html*

Webster, Cheryl, Doob, Anthony & Franklin Zimring (2006). Proposition 8 and crime rates in California: The case of the disappearing deterrent. *Criminology and Public Policy, 5 (3),* pp. 417-442

Wellford, Charles (2005). Special issue on gun policy. *Criminology and Public Policy, 4 (4),* pp. 673-676

Wellford, Charles, Pepper, John & Carol Petrie (2005). *Firearms and violence: A critical review*. Washington, DC.: National Science Academies Press.

Yaffee, R. (2000). *Time Series Analysis and Forecasting*. San Diego: Academic Press.