

“Gun Control” vs. “Self-Protection”: A Case against the Ideological Divide



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Justice Policy Journal • Volume 10, Number 1 (Spring)

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Abstract

A recent string of vicious, senseless and tragic mass spree killings have propelled an intense re-appraisal of U.S. gun laws, but the ensuing dialogue amply demonstrates that the opposing sides of the gun policy debate are as firmly entrenched in their mutual opposition to one another as ever before (Washington, 2012). Those who favor stricter “gun control” axiomatically oppose “personal protection” (e.g., “right to carry”) strategies, whereas those who favor “personal protection” measures stridently oppose “gun control”. The present study compares statistically (N=1736) these two contrasting approaches according to the methodological recommendations published by a National Academy of Sciences Research Panel (Wellford, Pepper & Petrie, 2005), and the results provisionally suggest that “personal protection” (“right to carry”; henceforth “RTC”) laws may reduce both gun homicide rates and total homicide rates, whereas traditional “gun control” policies do not detectably effect either outcome. However, the present study also observes that neither type of measure was originally designed to protect the public against the presently emerging and more deadly threat posed by armed and mentally

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disturbed mass spree killers. Suggestions for the modification of existing gun measures to more effectively prevent future mass spree-killing are offered.

Introduction

Recent mass spree-killing have caused elected officials, policy makers, policy researchers, behavioral scientists, and ordinary citizens all to thoughtfully re-appraise U.S. public gun policy, and this rightfully includes both "gun control" laws and "personal protection" measures (e.g., "right to carry" laws). "Gun advocates" hold presently that traditional "gun control" policies have variously failed to prevent gun killings of all sorts, that they only prevent law-abiding citizens from protecting themselves, and therefore that they should be abandoned. "Gun control" supporters alternately hold that the restrictiveness of present gun control measures should be increased, that "self-protection" measures only increase the overall "body count", and therefore that the latter should be summarily rejected (Washington, 2012). However, the present study, the evident nature and source of presently emerging and more deadly armed threats, and current public opinion all point toward reasonable modifications to existing gun measures that would more effectively curtail future mass spree killings.

Schools, theatres, temples, malls and even academic tenure reviews in the U.S. were once relatively safe from deadly gun-violence, but this has frighteningly changed in recent years, and that is because the deadliest threat to public safety is no longer the violent career criminal or armed robber; rather, it is the mentally deranged psychopath bent on massacring innocents. On April 16th 2007, a mentally disturbed student used handguns he purchased "legally" to kill 31 innocents and then himself on the campus of Virginia Tech University. Just over 5 years later, James Egan Holmes killed 12 and injured 58 during a midnight premier showing of *The Dark Knight Rises* at the Aurora Colorado Movie Theatre using an "assault rifle" he had purchased "legally." On December 14th 2012, Adam Lanza killed his mother in her sleep, and then 20 young children and 6 adults at the Newtown Connecticut primary school using an "assault rifle" he "legally" obtained from his mother, which he finally turned on himself as police approached.

Gun polls taken in the wake of Sandy Hook understandably and predictably report that public support for stricter gun control has generally risen since the incident, but support for more specific types of policies is less clear, and that there is little support for outright bans of any specific type of weapon such as handguns or "assault weapons" (Washington, 2012). In general outline, Gallup public opinion polls report (a) that the public prefers stricter enforcement of existing gun laws over the enactment of new ones 47% to 46%, (b) that most do not favor an assault weapons ban 51% to 44%, (c) that most do not favor a ban of handguns for all but

law enforcement 74% to 24%, (d) that most favor closing the gun-show loophole 92% to 7% , and finally, (e) that most favor a ban on “feeding devices” (magazines) capable of holding more than 10 rounds of ammunition 62% to 35% (Washington, 2012)²³.

These poll results defy the deep ideological divisions (a) from which many policy researchers have historically conceived gun-policy research, (b) from which they have reviewed the work of others, and (c) from which gun-policy has traditionally been enacted. Moreover, the poll results suggest a somewhat less ideologically-grounded and more research-based approach to future gun-policy research and implementation, and all in step with public opinion.

Research Standards: Political Ideology as “Methodological Concern”

Scientific efforts to help resolve the question of which gun-measures are most efficacious have been methodologically sound, well-focused and certainly extensive, but the results have been indisputably mixed nonetheless (e.g., Ayers & Donahue, 2003a, 2003b; Black & Nagin, 1998; Duggan, 2001, Lott & Mustard, 1997, Lott, 2000; Ludwig, 1998; Olson & Maltz, 2001; Vernick & Hepburn, 2003), and the ideologically charged nature of the debate is continuously highlighted by the inconsistent methodological critiques that follow published research (see especially Cook & Ludwig, 2003 vs. Kleck, 1997 for clear examples).⁴ Unfortunately, little has changed since Wright, Rossi and Daly (1983) astutely observed:

Both “guns” and “crime” are emotionally laden symbols that evoke strongly held and not always rational feelings, emotions and concerns, and researchers are not exempt from such evocations (p.3). In the minds of many, the gun

² No estimates of the effects of “assault weapon” bans, magazine capacity limitations, or ammunition-type restrictions are included in the present study primarily because the overlap in time and location between the federal bans and restrictions on the one hand, and local, city or state bans on the other are statistically redundant, and therefore inestimable. In addition, there exists wide variability in state versus federal conceptualizations (definitions) of “assault weapons”, and the available information about which states or cities were or were not subject to local assault weapons bans at different points in the series is insufficient to accurately code the data. Finally, local, city, and state “assault weapons” variously include or exclude different types of ammunition and magazine capacity restrictions further confounding efforts to accurately classify and code these policies, too.

³ +/-0.04 margin of error

⁴ These citations are not comprehensive and should not be taken as favoring either side of the gun-debate; only that each represents opposing ideological viewpoints and interpretations of the existing gun policy research literature.

symbolizes all that is wrong in American culture: it symbolizes male dominance, sexual frustration, aggression, violence, and a host of other pathologies that are offensive to a civilized society (p.4). But in the minds of many others, the same gun symbolizes all that is right in the culture: it symbolizes manliness, independence, self-sufficiency, outdoorsmanship, and a willingness to die for one's beliefs. In this view, the gun is the virtual embodiment of traditional American values (p.8).

As a National Science Academy Panel Report (Wellford, Pepper & Petrie, 2005) similarly laments, "Little can be decided through argumentation over a-priori beliefs and expectations (p.121)". For these and other reasons, the present study relies exclusively on the methodological recommendations published by the aforementioned National Science Academy Panel Report (Wellford, et. al, 2005) to support its general methodology, and not on independent methodological critiques of previously published work, or on other existing critiques.

The Present Study and the National Academy of Sciences Panel Report

The Panel Report (Wellford, et. al., 2005) issued a refreshingly non-partisan, comprehensive and timely methodological critique of the existing gun policy research that provisionally resolves many of the existing methodological disputes (pp. 120-151). The Research Panel chiefly raises concerns about (a) unacceptably high levels of aggregation such as states or counties, (b) analytical dependence upon observably unreliable county-level data (see Maltz & Targonski, 2002 for a review), (c) artificial statistical confidence produced by excessively large numbers of non-independent sample units, (d) the sensitivity of policy effects to seemingly minor changes in overall model specification, (e) questionably short or overly extended post-intervention periods, (f) differences among the various statistical techniques of gun policy outcome estimation, and finally, (g) the need for more data overall (Wellford, et. al., 2005, pp. 120-151, 223-230; see also McPhedran, Samara & Baker, 2008).

The problem with state or county levels of aggregation is that they are too internally heterogeneous to be inferentially reliable levels of analysis; in states such as Texas or California, for example, the cities of Dallas or Los Angeles are very different places than, say, El Paso or Oakland, and these internal dissimilarities may produce different effects in one city versus another thus obscuring overall policy effects where states or counties are the units of analysis. Sample unit non-independence reduces standard errors, ergo, produces artificial statistical

significance, which means that in such cases, one must be especially cautious in accepting policy effects as authentically significant where they are reported. With respect to model specification sensitivity, the research panel (Wellford, Pepper & Petrie, 2005) found that adding or removing just a single covariate or control frequently changes the magnitude, direction or statistical significance of the policy effects, thus leaving questionable whether the effects are actually negative, positive, or if they even exist at all. The period of time allotted to observe policy effects is critical, too, since there is no clear answer to the question of how long it should take for a gun policy to exert statistically detectable effects. The research panel (Wellford, Pepper & Petrie, 2005:120-151, 223-230) found that different statistical equations and different estimation models produce different results, which also leaves open the question of which models and which statistical approaches are the most valid. Finally, the Panel Report (2005) observed that the available data as of 2005 was insufficient to properly evaluate the results, and that substantially more data would be required in the future to do so.

The Panel Report (2005) summarizes their overall findings to conclude that present gun-policy research methods should be abandoned altogether in favor of presumably superior yet still unarticulated research methods, but only in curiously vague terms:

If further headway is to be made on this question, new analytical approaches and data sets will need to be used. For example, studies that more carefully analyze changes in gun-carrying behavior at the county or even the local level in response to these laws may have greater power in identifying the impact of such laws. Surveys of criminals or quantitative measures of criminal behavior might also shed light on the extent to which crime is affected by such laws (p. 151).

Rather than awaiting “new analytical approaches” with no arrival in sight, the present study follows the methodological path carved by previously published gun-policy research which was similarly guided by the Panel Report (La Valle, 2007, 2008, 2010; La Valle & Glover, 2012). Moreover, the present study (a) revises existing research methods, (b) employs newly (since 2005) available data, and (c) develops new data sets to estimate the effects of gun-policy, and all in accord with the methodological concerns raised by the Panel Report (Wellford, et. al., 2005).⁵

⁵ The Panel Report (2005) also suggests that perhaps too much time has passed since the original enactment of many existing gun laws, and thus post-intervention periods are untenably long to accurately evaluate the effects of existing gun policy. This assertion is also rejected on the grounds that research in this vital area cannot cease until new gun-laws are enacted, and that evaluation of existing gun-laws is critical to the development of new and more effective gun-laws.

The present study adjusts for the interrelated matters of problematically high levels of aggregation, reliance on county-level data, and sample unit non-independence by conducting a city level analysis that includes parameter estimates to measure the effects of sample unit clustering (see Babbie, 2007, p. 367 for methodological support, and also Maltz & Targonski, 2002). Second, model specification is improved by including the widest possible range of covariates and controls from the homicide literature,⁶ by then factor indexing them to correct for over specification and collinearity, and by accordingly estimating gun-homicide rates and total homicides rates exclusively to maximize the validity and reliability of the response variable with respect to the overall model specification. Third, the present post intervention periods naturally vary considerably among the sample units and are extended as far as present data availability will allow. Fourth, the present study compares the results of multiple pairs of model estimates to more rigorously evaluate the robustness and stability of the policy effects. Finally, the present study minimally adds five years of data that did not literally by definition exist at the time of the Panel Report (Wellford, et. al., 2005; see also McPhedran, Samara & Baker, 2009). In general outline, the present study estimates the effects of four different “gun control” measures and RTC laws on gun homicide rates and total homicide rates for 56 U.S. cities over 31 years with statistical corrections for variation in sample unit independence and serial correlated error terms.

Gun Policy Conceptualizations: “Gun Control” vs. “Personal Protection”

“Gun control” is presently conceptualized by four categorical variables that include (a) “BradyLaw”, (b) “License”, (c) “Permit” and (d) “Register”, and common to all four types of measures is that they each require criminal background checks to determine applicant eligibility. “BradyLaw” indicates whether a city was subject to state mandated background checks for gun purchases prior to February 28, 1994 when the law was federally enacted, or not until that date. “License” indicates whether there exists a legal requirement to possess a license to own firearms, “Permit” indicates whether there exists a legal requirement to have a permit to purchase a handgun, and “Register” indicates whether there exists a legal requirement to register firearms, usually handguns, with local government registries (NRA.org; table 1).⁷ When the Brady Law was federally enacted in early

⁶ See “Known Factorial Determinants of Homicide” on p 16 herein for a comprehensive review.

⁷ All coded ‘1’ for “yes” and ‘0’ for “no.”

1994, 18 states referred to as “Brady States” already had equally restrictive provisions in place, whereas 32 referred to as “Non-Brady States” did not.

RTC laws allow legally qualified residents of a state to carry a concealed handgun for personal protection, but to be eligible, applicants must (a) also submit to a criminal background check, (b) pass a gun-range test, (c) have been a resident of the state for at least 1 year prior, (d) demonstrate firearm safety, and (e) pass classroom courses that detail the legalities of carrying and using firearms for personal protection. At the beginning of the present series, 1980, approximately 14 states had RTC laws in place. By the end of the present time-series, 2010, all but a few states had such laws in place (NRA.org; table 1).⁸ RTC laws are also the lone representation of the “personal protection” approach to gun-policy for the present study.

⁸ Examples of states without RTC laws as of 2010 include New York, New Jersey, Massachusetts, and Connecticut.

Table 1: Gun Laws

State	Brady Law	RTC Law	License	Register	Permit
Alabama	1994	*	No	No	No
Arizona	1994	*	No	No	No
California	*	No	*	No	No
Colorado	1994	1981	No	No	No
Georgia	1994	*	No	No	No
Hawaii	*	1988	No	*	*
Illinois	*	No	*	No	*
Kentucky	1994	1996	No	*	No
Maryland	*	*	*	No	*
Massachusetts	*	*	*	No	*
Minnesota	1994	2003	No	*	No
Michigan	*	2001	No	*	*
Missouri	*	2003	No	No	No
Nevada	1994	1995	No	No	No
New Jersey	*	No	*	No	*
New Mexico	*	2003	No	No	No
New York	*	No	*	*	*
North Carolina	1994	1996	No	No	*
Ohio	1994	2004	No	No	No
Oklahoma	1994	1995	No	No	No
Oregon	1994	1989	No	No	No
Pennsylvania	1994	*	No	No	No
South Carolina	1994	1996	No	No	No
Tennessee	1994	1996	No	No	No
Texas	1994	1996	No	No	No
Utah	1994	1986	No	No	No
Virginia	*	*	No	No	No
Washington	1994	*	No	No	No
Wisconsin	*	No	No	No	No

*Enacted prior to beginning of series—1980

Data Collection, Model Specification and Factor Analysis

The data for homicide, gun-homicide, police personnel and city square mile coverage were obtained from the Uniform Crime Reports (1980-1985) and Crime in the United States (1986-2010). The poverty, ethnic composition and regional data were obtained from the U.S. Census (1980-2010) and the U.S. Census Supplements (1974-2007). The alcohol availability data were obtained from the U.S. Census of Retail Trade (1977-2002). The Brady Law information was taken from Jacobs (2002),

the RTC statutes from La Valle & Glover (2012), and "License", "Permit" and "Register" all from NRA.org⁹ (Table 1).

The requisite U.S census data is only collected at the end of each decade, so the missing observations have been interpolated according to the straight linear function method (Yafee, 2000:3)¹⁰¹¹. Table 2 reports the logged descriptive statistics for the census year only data, table 3 reports the logged interpolated distributions, and table 4 reports the variables included in each factor analyzed combination, factor loadings, ALPHA (reliability) coefficients, and names of each factor indexed variable included in the final models.¹²

⁹ The present study acknowledges that there is substantial disagreement among gun-policy researchers about which states have true RTC laws and which do not, but holds that the present choice is the most up-to-date and thoroughly researched to date (see appendix, La Valle & Glover, 2012).

¹⁰ This approach is also a response to Ayers & Donahue's (2003a & 2003b) critical objection to "spline" models.

¹¹ Approximately 65% of the total observations are not interpolated in the interpolated models, and no D.V's (homicide rates) were interpolated except in rare cases where no figures were reported to the BJS by a city for a year. The U.S census of retail trade ceased reporting alcohol outlet counts after 2002, so the 2003 through 2010 data are identical to the reported 2002 alcohol outlet figures.

¹² The advantages of this approach are that it (a) automatically pre-standardizes the coefficients, it (b) reduces substantially the total number of parameter estimates, it (c) resolves collinearity problems, it (d) reports the reliability of the underlying construct, and (e) it allows for more refined and valid model specification.

Table 2: Logged Descriptive Statistics for Census Years Only (N=224)

Variable	Mean	Std. Dev.	Skew	Name
Population	6.20	.803	.790	Pop
Number of liquor stores	4.78	1.04	-.142	Alc1
Number of bars	4.02	1.01	.379	Alc2
Liquor stores per square mile	-.143	1.23	.111	Alc3
Bars per square mile	-.835	1.12	.249	Alc4
Persons per square mile	1.26	.811	.036	PopDen
Percent of population renting	3.84	.185	.118	PopRent
Median family income	3.64	.447	-.213	FamInc
Median household income	3.35	.460	-.274	HouseInc
Number of patrol officers	7.05	.980	.765	PolOff
Patrol officers per resident	850	.363	.245	PolPres
Patrol officers per square mile	2.12	1.05	.303	PolDens
Less than high school education	3.07	.437	-.228	PopLTHS
Percent households female head	3.32	.305	-.350	FemHead
Individual poverty rate	2.81	.327	-.543	IndPov
Family poverty rate	2.54	.393	-.349	FamPov
Percent of population black	2.76	.998	-.517	PopBlack
Percent of population hispanic	2.11	1.23	-.212	PopHisp
Gun homicide rate	1.95	.956	-.623	GunHom
Total homicide rate	2.52	.797	-.358	TotalHom

Table 3: Logged Descriptive Statistics for Interpolated Data (N=1732)

Variable	Mean	Std. Dev.	Skew	Name
Population	6.22	.791	.801	Pop
Number of liquor stores	4.76	1.03	-.136	Alc1
Number of bars	4.00	1.00	.368	Alc2
Liquor stores per square mile	-.180	1.21	.073	Alc3
Bars per square mile	-.852	1.11	.299	Alc4
Persons per square mile	1.28	.804	.044	PopDen
Percent of population renting	3.84	.179	.333	PopRent
Median family income	3.57	.362	-.142	FamInc
Median household income	3.39	.372	-.184	HouseInc
Number of patrol officers	7.06	.960	.896	PolOff
Patrol officers per resident	843	.352	.318	PolPres
Patrol officers per square mile	2.12	1.04	.316	PolDens
Less than high school education	3.04	.417	-.247	PopLTHS
Percent households female head	3.33	.291	-.278	FemHead
Individual poverty rate	2.81	.324	-.573	IndPov
Family poverty rate	2.56	.390	-.365	FamPov
Percent of population black	2.76	.998	-.493	PopBlack
Percent of population hispanic	2.14	1.22	-.203	PopHisp
Gun homicide rate	1.98	.906	-.414	GunHom
Total homicide rate	2.52	.761	-.380	TotalHom

Known Factorial Determinants of Homicide

Each of the controls and covariates included in the present study were taken from the homicide literature where they have been variously identified as factorial determinants of homicide. The main categorical control is the U.S. census region within which each city is located—(a) Northeast, (b) Midwest, (c) South, and (d) West.

Messner (1983) tested the “southern sub-culture of violence” hypothesis and found that southern regions may exert statistically significant effects on homicide rates due to higher firearm ownership levels even when other important factors such as race are controlled. In response, Dixon and Lizotte (1987) found that violent crimes such as homicide may be a result of a “subculture of violence”, but that this relationship is not necessarily specific to the south and is most likely unrelated to firearm ownership levels.

Parker (1995) found that levels of neighborhood alcohol consumption are positively associated with overall homicide rates. Alaniz, Luisa, Cartmill and Parker (1998) similarly found that homicide rates are higher in neighborhoods where alcohol availability is higher controlling for other important factors such as income inequality. In addition, La Valle (2007) found that alcohol availability is positively and significantly associated with homicide rates.

Population density is equally prevalent in the homicide literature, but is almost invariably employed as a control, which seems appropriate since denser forms of social organization such as prison populations and juvenile facilities frequently suffer from greater internal social tensions and greater social disorganization which may lead to increased incidents of lethal interpersonal violence (see Tartato & Levy, 2007 for a thorough review of the relevant literature). La Valle’s (2007) city level study of RTC laws and the Brady law also found population density to be statistically significant controlling for fewer but similar other determinants of homicide.

Numerous homicide studies have reported that income inequality is detectably associated with homicide rates in a community. Almgren, Guest, Immerwhar and Spittel (1998) found that joblessness is positively related to homicide rates controlling for both race and gender. Crutchfield (1989), too, found that income inequality is associated with homicide rates controlling for unemployment and poverty rates. Harrer and Steffenshmeir (1992) also reported that income inequality strongly affects the relationship between inter-racial homicide rates. Messner and Tardiff (1983) similarly found that economic opportunities significantly influence rates of interracial homicide.

Police presence has been controlled in previous multivariate estimates of the effects of other important determinants of violent death, as a number of homicide studies demonstrate. Grant, Sherman & Martinez (1997) found police expenditures to be a statistically significant correlate of homicide controlling for population size, inflation index, unemployment rate, and unionization. Harer and Steffensmeier (1992) actually found police presence to be positively correlated with violent crime rates, which are consistent with La Valle's (2007, 2008 & 2010) studies of the effects of gun policies on homicide rates in 20 of the largest U.S. cities.

Few variables are as widely accepted as poverty as a determinant of homicide, and the research confirming so stretches back over 30 years. Parker and Smith (1979) found that poverty rates controlling for age structure are robustly and positively correlated with incidents of primary homicide. For a sample of 125 statistical metropolitan areas, Williams (1984) similarly found that poverty is positively correlated to homicide, but that region exerts an additional effect. Bailey (194), too, found that poverty exerts a statistically significant effect on most violent crimes, including murder. In addition, the most recent city-level estimates of the effects of gun policies on homicide rates consistently find that poverty exerts an especially robust effect on both outcomes controlling for alcohol availability, population density, and police presence (La Valle, 2007, 2008 & 2010).

Ethnic composition has also been found to be a factorial determinant of homicide controlling for other factors. Analyzing data for Columbus, Ohio in 1990, Kirvo and Peterson (1996) reported that racial differences reflected in structural inequality accounts for black-white differences in violent criminality. Krivo, Peterson, Rizzo and Reynolds (1998), too, found that racial inequality produces racially segregated concentrations of community disadvantage which, in turn, led to increased rates of violent crime including homicide. Harer et. al (1992) also found that the effect of economic inequality is different for Blacks than for Whites.¹³

¹³ Sex ratio and age are also commonly found in the homicide literature, but both were discarded from the present analysis due to no association and statistical insignificance throughout.

Table 4: Factor Analyzed Variable Coefficients and Combinations

Census Years Only				With Interpolated Data			
Variable	Loading	ALPHA	Name	Variable	Loading	ALPHA	Name
Alc 1	.917			Alc 1	.915		
Alc 2	.829			Alc 2	.829		
Alc 3	.853			Alc 3	.858		
Alc 4	.885	.894	ALCOHOL	Alc 4	.882	.894	ALCOHOL
Pop	.766			Pop	.762		
PopDen	.900			PopDen	.900		
PopRent	.739	.723	DENSITY	PopRent	.756	.731	DENSITY
FamInc	.998			FamInc	.996		
HouseInc	.998	.995	INCOME	HouseInc	.996	.993	INCOME
PolOff	.879			PolOff	.871		
PolPres	.888			PolPres	.886		
PolDens	.928	.875	POLICE	PolDens	.931	.877	POLICE
PopLTHS	.680			PopLTHS	.726		
FemHead	.817			FemHead	.833		
IndPov	.909			IndPov	.920		
FamPov	.936	.858	POVERTY	FamPov	.938	.878	POVERTY

Verimax rotation method

Cronbach's ALPHA based on standardized items

Specified Outcomes: Homicide and Gun Homicide

There are five important methodological reasons why gun homicide rates and total homicide rates are the exclusively measured outcomes for the present study, and they are variously reliability, validity and/or generalizability based. First, the validity of the present model specification has been maximized by including the widest possible range of variables from the homicide literature (review previous section) in the final multivariate analyses; the variables included in the final estimates are those pre-identified scientifically to be factorial determinants of homicide.

Second, since the dependent variable is regressed on the independent variables, the reliability and validity of the outcomes are most critical to the validity of multiple regression analyses. Homicide is the most reliably identified and reported of all index crimes; whereas other violent crimes such as, say, rape, assault or even burglary are frequently subject to various situational ambiguities or investigational contingencies, the victim is always found deceased or at least permanently missing in cases of a homicide.

Third, the Uniform Crime Reports only record the most serious crime in cases of multiple offenses, which means that official measures of homicide and gun-homicide necessarily capture incidences of both “felony murder” and multiple homicides from mass spree-killings), an overall rate which should decrease if criminals are deterred by potentially armed victims, but increase if gun policies do not deter homicide. Moreover, then, gun laws should exert stronger, more significant and more reliable effects, regardless of direction, on homicide rates and gun-homicide rates as compared to other measured index crimes.

Fourth, the present study does not disaggregate homicide rates into general type or offender-victim relationship in the interest of generalizability. True, gun laws are more likely to affect some types of homicide more than others (see Ludwig, 1998 for a thorough review), and more likely to affect adult rates more than juvenile or childhood rates, but the larger question is whether the net effect of these laws are lives saved, people killed, or neither, especially in the wake of the Sandy Hook massacre of 20 innocent children and 6 adults.

Fifth and finally, gun homicide rates and total homicide rates are the most common outcomes measured by previous gun-policy analyses (see Black & Nagin, 1998; La Valle, 2007, 2008, 2010; La Valle & Glover, 2012; McDowall, Loftin & Wierserma, 1995; Olson & Maltz, 2001; Plassman & Tideman, 2001; Plassman & Whitely, 2003 for more comprehensive reviews), and the critical importance of these outcomes are implied contextually by the Research Panel Report (2005).

Sample and Statistics

The present study is a naturalistic quasi-experimental multivariate pooled time-series research design. The analytic sample consists of 56 cities (table 2), and the series spans 31 years—1980 through 2010.¹⁴ It employs multiple estimation models, fixed effects for each unit observation and year, correction for serial correlation of error and controls for sample unit non-independence. Three pairs of models are estimated; each (a) employs “fixed effects” for each city and year, each (b) includes a

¹⁴ This is the widest sample cross-section the available *city-level* data will allow: Only 69 cities regularly reported gun homicide rates or numbers over the entire period covered by the series, but some of those were eliminated due to missing more than two consecutive years of gun homicide figures. These include mostly those that had suffered natural disasters sometime between 1980 and 2010 – Miami, Tampa, Jacksonville, New Orleans, etc. Additional cities were also eliminated due to missing U.S census data. These cities included Indianapolis, Louisville and a few others. The end result is the 56 cities that presently comprise the analytic sample.

homogeneous first-order auto-regressive covariance structure,¹⁵ each (c) estimates the same analytic sample over the same overall time period, each (d) includes the same covariates and controls, and each (e) estimates the effects the specified gun policies may or may not exert on both gun homicide rates and total homicide rates. The first pair estimate the effects of RTC laws using the available census data only, the second pair includes the interpolated data, and the third also includes the interpolated data but are also weighted according to population size and the effects are lagged one-year.

A comparison of the first pair of estimates with the second provides a means of determining if the inclusion of interpolated data alters the direction of the policy effects. Such changes would suggest that the interpolated observations are somehow problematic, whereas no such change would provisionally confirm that they are not. Comparing un-weighted estimates with weighted and lagged estimates provides a means of comparing between city differences (un-weighted) on the one hand, with proportionally disaggregated population differences (weighted) on the other. The policy effects that (a) hold their direction throughout, (b) hold or gain statistical significance with the addition of the interpolated observations, and (c) remain accordingly stable across all models are presently considered the most robust and reliable for the present analysis.

Results

The present research design addresses the main concerns raised by the Panel Report (Wellford, et. al., 2005), but in so doing, it is forced into two methodological choices that should be evaluated before examining the intervention effects. First, the imputation of interpolated observations increase both the sample size and the power of the current design (the observations on homicide and gun homicide rates are not interpolated), but in so doing, could overstate the statistical significance of the policy variables. Second, there is the question of whether the weighted models or the un-weighted models are more valid. For the present study, reporting both sets of results provides a method of assessing the robustness of the policy intervention effects, but it should be recognized that weighted and lagged criminal justice policy estimates are appropriate for inferentially sound conclusions and valid and reliable policy recommendations (see especially Marvell & Moody, 2008).

¹⁵ The “fixed effects” approach is due to the semi-systematic sample, general linear models were chosen over general estimating equations due to improved fit statistics, and the homogeneous auto-regressive covariance structure was chosen over the heterogeneous type due to stronger “rho” statistics.

Table 5: Estimates for Census Years Only

<u>"Fixed Effects" Gun Homicide Model</u>			<u>"Fixed Effects" Total Homicide Model</u>		
Parameter	Estimate	Std. Error	Parameter	Estimate	Std. Error
RTCLaw	-.101	.106	RTCLaw	-.127	.084
BradyLaw	-.241*	.118	BradyLaw	-.199*	.094
License	.055	.238	License	.083	.194
Permit	.031	.263	Permit	.064	.218
Register	.211	.292	Register	.123	.241
Nested	.083	.149	Nested	.037	.124
Region1	-.726*	.258	Region1	-.582*	.213
Region2	-.430*	.206	Region2	-.318	.170
Region3	-.168	.167	Region3	-.176	.138
Region4	0a	0a	Region4	0a	0a
ALCOHOL	.091	.112	ALCOHOL	.087	.092
DENSITY	-.254*	.120	DENSITY	-.250*	.097
INCOME	-.253*	.077	INCOME	-.210	.046
POLICE	.289*	.119	POLICE	.257*	.096
POVERTY	.136*	.068	POVERTY	.141	.055
LogBlack	.672*	.084	LogBlack	.510*	.068
LogHispan	.197*	.059	LogHispan	.138*	.049
*p<.05			*p<.05		
AR-1=.44			AR-1=.47		
-2LL=348.99			-2LL=255.37		
a=Redundant			a=Redundant		

The census-year only data report that AR-1 is a reasonable correction for serial auto-correlation, that all of the continuous controls and covariates are statistically significant save "ALCOHOL", all are in the expected direction save "DENSITY", and that the significance of "Region" seems to depend upon which census region a city is located within. It also reports that the only statistically significant policy intervention variable in the expected direction is "BradyLaw", which is consistent with earlier published studies that use similar samples and research methods (see especially La Valle 2007 & 2010). "Nested" reports that whether cities are clustered within the same state makes no statistically detectable difference.

Table 6: Estimates with Interpolated Data

<u>"Fixed Effects" Gun Homicide Model</u>			<u>"Fixed Effects" Total Homicide Model</u>		
Parameter	Estimate	Std. Error	Parameter	Estimate	Std. Error
RTCLaw	-.181*	.055	RTCLaw	-.134	.045
BradyLaw	.033	.055	BradyLaw	-.008	.045
License	-.096	.131	License	.015	.109
Permit	-.069	.151	Permit	-.046	.124
Register	.138	.168	Register	.101	.138
Nested	.048	.185	Nested	-.012	.069
Region1	-.815*	.146	Region1	-.639*	.120
Region2	-.561*	.118	Region2	-.400*	.069
Region3	-.233*	.096	Region3	-.199*	.079
Region4	0a	0a	Region4	0a	0a
ALCOHOL	.118	.065	ALCOHOL	.075	.055
DENSITY	-.234*	.069	DENSITY	-.184*	.056
INCOME	-.180*	.057	INCOME	-.176*	.031
POLICE	.226*	.034	POLICE	.218*	.096
POVERTY	.154*	.046	POVERTY	.148*	.038
LogBlack	.646*	.051	LogBlack	.486*	.042
LogHispan	.149*	.035	LogHispan	.101*	.029
*p<.05			*p<.05		
AR-1=.71			AR-1=.78		
-2LL=1818.10			-2LL=1727.24		
a=Redundant			a=Redundant		

The imputation of the interpolated observations predictably increases the AR-1 coefficients considerably (.44/.71 for gun homicide, and .47/.78 for total homicide), and also produced statistical significance in dichotomous control variables that were previously insignificant—"Region 3" for gun homicide, and "Region 2" and Region 3" for total homicide, not an unexpected outcome. The addition of the interpolated data also caused the direction of three policy variables to reverse; both "License" and "Permit" for the gun homicide model, and just "Permit" for the total homicide model. Only these three estimates change direction with the inclusion of the interpolated data, the magnitude of the coefficients change only slightly overall, and none were significant for any of the three pairs of estimates, thus it is

concluded presently that the interpolated observations are not problematic in this general regard.

By far the most important outcome for this pair of estimates is that “BradyLaw” is no longer significant for either gun homicide or total homicide, but “RTCLaw” becomes statistically significant and in the expected direction for both, an explicable result. The census year (only) estimates model just four observations over time, 1980, 1990, 2000 and 2010, the Brady Law was adopted federally in 1994, and the bulk of the RTC laws were adopted at some point during the late-eighties, mid-nineties and early two-thousands, which means that nearly all of these two policies were coded for either 1990 or 2000. These modeling limitations altogether produced artificial collinearity between “BradyLaw” and “RTCLaw”, a problem that is naturally resolved with the imputation of the interpolated data where the actual years of passage are modeled correspondingly to the actual years of adoption.

Table 7: Weighted and Lagged (1-year) Estimates with Interpolated Data

<u>"Fixed Effects" Gun Homicide Model</u>			<u>"Fixed Effects" Total Homicide Model</u>		
Parameter	Estimate	Std. Error	Parameter	Estimate	Std. Error
RTCLaw	-.166*	.073	RTCLaw	-.137*	.062
BradyLaw	.022	.071	BradyLaw	.003	.060
License	-.276	.175	License	-.238	.154
Permit	.071	.229	Permit	.129	.202
Register	-.117	.198	Register	-.194	.174
Nested	.087	.150	Nested	.014	.133
Region1	-.809*	.215	Region1	-.671*	.190
Region2	-.586*	.191	Region2	-.469*	.168
Region3	-.386*	.096	Region3	-.322*	.140
Region4	0a	0a	Region4	0a	0a
ALCOHOL	.038	.096	ALCOHOL	-.018	.084
DENSITY	-.011	.082	DENSITY	.021	.071
INCOME	-.034	.046	INCOME	.011	.040
POLICE	.068	.078	POLICE	.030	.061
POVERTY	.477*	.068	POVERTY	.415*	.059
LogBlack	.404*	.051	LogBlack	.258*	.063
LogHispan	.028	.054	LogHispan	.027	.047
*p<.05			*p<.05		
AR-1=.78			AR-1=.80		
-2LL=2325.14			-2LL=1717.04		
a=Redundant			a=Redundant		

Lagging the effects 1-year and treating populations as the units of analysis rather than cities (weighting) further increases the magnitude of the AR-1 coefficients for both gun homicide and total homicide, but curiously eliminates the statistical significance of all but two of the continuous controls and covariates—"POVERTY" and "LogBlack", which suggests that the effects of population density, income inequality and police presence vary considerably according to population size.¹⁶ "Nested" remains statistically insignificant throughout. Critical to the present analysis are (a) that all the "gun control" treatments, including "BradyLaw" remain insignificant in both interpolated estimates, but (b) that "RTCLaw" reports a

¹⁶ A thorough discussion of this particular result is ancillary and beyond the scope of the present study

statistically significant reduction in homicide of approximately seventeen percent for gun homicide and roughly fourteen percent for total homicide (tables 7&8), and that RTC effects remain directionally stable, statistically significant and effectively robust weather the units of analysis are disaggregated according to population size or cities^{17,18}.

The present study suggests that “personal protection” approaches probably do serve some sort of deterrent or preventative function, whereas “gun-control” measures most likely have not exerted the desired effect, at least not up to the present. If the observed national decline in homicide rates over the past 20-years (Uniform Crime Reports 1980-1985 & Crime In The United States, 1986-2010) may be attributed in any increment to any pre-existing gun-policy, the present study suggests that it is “personal protection” measures and not “gun control” policies.¹⁹

Methodological Limitations

There are two important limitations of the present study, one substantive and the other statistical. The primary substantive limitation of the present study (and all similar RTC studies) is that it does not link RTC laws directly to (a) crimes committed with actual permit holders, (b) overall patterns of gun carrying, (c) guns owned by permit holders, (d) guns stolen from permit holders, or (e) actual incidents of permit holders defending themselves with guns (Wellford, et. al., 2005, p. 151). It would be ecologically fallacious, then, to take the present study to suggest that guns necessarily protect citizens against criminals more frequently than guns are used to commit deadly gun crimes.²⁰ The primary statistical limitation of the present study is that the observed RTC effects may be endogenous. The vast majority of state RTC laws were enacted sometime during the national 20-year decline in homicide rates, which may perhaps mean that the observed effects are

¹⁷ These effects also remained relatively stable and significant throughout the collinearity diagnostic process

¹⁸ The difference between these results vs. previous studies employing similar methods (esp. La Valle, 2007, 2010) may be due to (a) the expanded sample cross-section, (b) 5 years of additional data, and (c) the extended post-intervention period.

¹⁹ **NOT** to be interpreted as a firm conclusion.

²⁰ Supplementary estimates of the comparative frequency of defensive gun use (henceforth “DGU”) vary by literally millions per year (e.g., Kleck & Gertz, 1995 vs. Hemenway, 1997), and the DGU literature overall cannot even collectively agree on an operational definition of “defensive gun use”. The Panel Report (Wellford, et. al., 2005) concurrently observes that “much of the confusion surrounding the [DGU] debate seems to center on what is meant by ‘defensive gun use’. Whether one is a defender (of oneself or others), or a perpetrator, for example, may depend on perspective” (p.106).

due to presently unspecified factors and not from the presently observed RTC effects.

Conclusions: Policy Recommendations

The present study provisionally combines with the facts surrounding recent mass spree-killings and public opinion to suggest that each side of the current gun-debate must concede in roughly equal measure to the other. "Gun advocates" should acknowledge that at least some modifications to present "gun control" measures would be generally preventative, and "gun control" supporters should concede that legally owned firearms may also serve a deterrent or protective function. Present "personal protection" measures should not be rejected, and neither should present "gun control" policies, but both should be similarly revised. Moreover, U.S gun policy should be modified to (a) include psychiatric histories along with existing criminal background checks for all gun purchases and transfers, (b) prevent secondary access to legally owned firearms by legally disqualified individuals, and (c) both revisions should be effected to both "gun control" measures and "personal protection" measures alike.

A number of recent studies very similar to the present one (esp. La Valle, 2007 & 2010) reported statistically significant reductions in both total homicide rates and gun homicide rates resultant of the Brady Law, whereas the present study does not, but a probable reason why is that traditional "gun control" measures were never geared to curtail the presently emerging and more deadly types of armed threats. Whereas criminals may have access to underground gun-markets, the mentally disturbed most likely do not, which means that modifications to existing gun-measures may more effectively prevent the mentally disturbed from getting guns than they do convicted criminals. Virtually every gun-toting spree-killer in recent memory has been found to have suffered from some sort of severe mental illness that should have disqualified them from purchasing guns "legally", but there was no such legal prohibitions in place at the time, and neither were there any legally imposed firearm storage requirements that could have prevented any from simply "borrowing" guns from acquaintances, friends or relatives

Right to privacy laws are, in most cases, appropriate and well-intended, but those looking to purchase firearms should nonetheless be required to provide their psychiatric history going back at least 5-years before being allowed to complete a gun-purchase or transfer. There should be a formal list of diagnosed mental illnesses that disqualify individuals from purchasing guns, just as criminal convictions disqualify some from legally buying or possessing guns presently.

Those who have been involuntarily admitted to psychiatric wards over the previous 5-years should automatically be prohibited from owning or purchasing guns at least until they are declared “well”, a judge terminates the prohibition early, or the prohibition naturally expires.

There should also be a list of less serious mental illnesses that do not disqualify applicants from purchasing or possessing firearms, those minor afflictions we know are not associated with violent or aggressive behavior. To be fair, there should also be legal mechanisms that allow for challenging a purchase denial, just as there are avenues to challenge a purchase denial due to false or misrepresented criminal histories.

A government approved locking container should be required for all gun-purchases, and secondary access by individuals under the age of 21 or with disqualifying legal or psychiatric histories must be prohibited. Prospective gun buyers should also be required to provide a full list of all who will have secondary access to the guns, and each individual listed should be subject to the same background checks as the purchaser. If a secondary gun-users are disqualified and /or there are residence under the age of 21, the buyer must provide proof that the guns are to be kept in a government approved locking gun container, and that there will be no secondary access whatsoever to the guns by anyone. To better assure the effectiveness of all of the presently proposed gun-policy revisions, the gun show loophole should be closed without delay.

The same mandates should also apply to RTC permit holders and applicants. Just as “gun control” polices should include psychiatric histories along with criminal records, so, too, should gun-carry permit applicants and holders. Just as involuntary admittance to a psychiatric ward at any time over the previous 5-years should automatically prohibit prospective gun-purchasers from completing a gun-purchase, so, too, should such be the case for concealed gun-carry permit holders and applicants. Just as prospective gun-purchases should prohibit secondary access to legally owned firearms, so, too, should such be the case for gun-carry permit applicants and holders. Just as prospective gun-purchasers should be required to store their guns in government approved locking containers, so, too, should such be the case for gun-carry permit holders. Just as there should be legal consequences for violating these provisions, so, too, should such be the case for permit applicants and holders.

Common to both sides of the gun policy debate is that (a) the mentally disturbed should not possess firearms and that (b) there is a need for greater responsibility on the part of gun owners. The present results combine with the evident facts surrounding recent mass spree killings and public opinion to suggest that there is

ground common to both sides of the gun-debate, and that now is the time to capitalize a-politically on this common ground to more effectively curb future mass spree killings.

For such measures to finally manifest, however, both sides of the gun debate must demonstrate some ideological and political flexibility in roughly equal measure, a price all should be willing to pay in the interest of protecting all from future mass spree killings. If not, an opportunity to prevent at least some future mass spree killings will have tragically passed, and there will be no end in sight to what currently promises to be a never-ending cycle of ideological disputes over the efficacy of all types of gun measures.

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La Valle extends his sincerest and most heart-felt gratitude to Dr. Robert Hanneman in the Department of Sociology at the University of California, Riverside, without whose help this paper would not have been possible.