GUNS IN AMERICA

ASSESSING THE IMPACT OF STATE FIREARM LEGISLATION ON HOMICIDE AND

SUICIDE RATES IN US METROPOLITAN STATISTICAL AREAS

A Thesis

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Katrina Rose Beedy

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Abstract

of

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In recent years, gun control policy has dominated the American political agenda. Today, political pundits on both sides of the aisle regularly cite gun statistics supporting either a pro or anti-gun control stance. While billions of dollars are spent on firearm policies that either tighten or relax current laws, little is known about the actual effectiveness of many of the firearm laws being implemented. Meanwhile, tens of thousands of Americans continue to die from gun violence each year.

In this thesis, I seek to clarify the gun control debate by assessing the impact of restrictive gun control legislation on various categories of firearm deaths across US metropolitan statistical areas (MSAs) while controlling for a number of relevant variables. More specifically, I conduct a multivariate regression analysis correlating the restrictiveness of state firearm laws with firearm homicide and suicide rates while controlling for age, poverty, education, and location in a Southern state. All homicide and suicide data were drawn from the 2013-2015 Center for Disease Control and Prevention Wonder Database, while all other control variables were drawn from 2010-2014 US Census Bureau statistics. My results reveal that restrictive firearm laws have a small but statistically significant tendency to produce lower suicide rates, but have no

decipherable correlation with homicide rates. The findings for this study underscore the

importance of conducting additional research on this complex but highly salient topic.

_____, Committee Chair
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Chapter 1: Introduction

Over the past few decades, gun violence in America has captured the attention of the media, academics, and policymakers alike. Although overall crime rates have declined and leveled off in the US since the early 1990's (Krogstad, 2015; Sanburn, 2016), the 30 largest US cities experienced a double-digit increase in murder rates in 2016 (Sanburn, 2016). While these types of trends have alerted policymakers on both sides of the political spectrum, consensus is lacking on the best approaches to gun violence overall. Although a number of academics and policymakers believe that imposing more restrictive gun laws is the most effective means of curbing gun violence, others believe that relaxing gun laws is more effective. Meanwhile, others contend that firearm laws themselves have little effect on overall gun violence.

In this thesis, I will shed light on this divisive gun debate by conducting a cross-sectional multivariate regression analysis that **assesses the impact of restrictive firearm laws on two categories of firearm death across US metropolitan statistical areas**.

In the past decade, the politicized nature of the gun violence debate has strongly influenced firearm legislation across the nation. Indeed, in the wake of recent high-profile mass shootings such as the 2012 Sandy Hook Elementary massacre, some states have adopted stricter gun control measures, while many others have relaxed their gun laws (Yourish, Andrews, Buchanan, and McLean, 2013). As noted, these polarized policymaking trends raise the question as to which gun legislation approach is more effective in decreasing gun violence overall. Although it is difficult to give a definitive answer to this question given the complexities of determining causal order through cross-sectional research, my regression analysis will nonetheless offer insight into the partial effect of restrictive firearm laws on firearm death rates.

Conducting comprehensive research on this correlation is important for both general insight and concrete practical purposes. In particular, the Center for Disease Control estimates

that firearms claim the lives of more than 30,000 Americans every year (Xu, Murphy, Kochanek, and Bastian, 2016), while the American Public Health Association (APHA) estimates the independent cost of gun violence is nearly $174 billion a year (American Public Health Association, n.d.). Given the extreme financial and moral stakes surrounding gun violence, effective research on the true impact of restrictive firearm legislation is crucial.

In conducting any regression analysis, selection of relevant control variables is essential. In explaining the root causes of gun violence, many journalists, bloggers, and interest groups have cited factors such as weak gun laws (Law Center to Prevent Gun Violence, 2015), poverty (Stoneman, 2013), mental illness (Jaffe, 2016), and easier access to guns (Mattu, 2015). However, many pundits and interest groups tend to focus on only one particular variable in their analyses while failing to properly control for other factors. For instance, the Law Center to Prevent Gun Violence compares the strength of state gun laws with gun deaths rates among states, noting that states with strong gun laws tended to have lower firearm death rates (Law Center to Prevent Gun Violence, 2015). However, the analyses do not control for any other potential explanatory variables that may account for the difference in firearm death rates between states. In my analysis, I will control for a variety of relevant independent variables to provide a more sophisticated explanation of the partial effects of different variables on firearm death rates.

More specifically, this analysis will control for the following independent variables: the percentage of an MSA population between ages 15-34, the percentage of an adult MSA population with less than a high school diploma, and the percentage of an MSA population living below the poverty line. In addition, I will code states as "Southern" or "Non-Southern" based on US Census Bureau definitions.

With regards to gun laws, my analysis will take into account a wide array of firearm laws implemented across the nation in recent years. More specifically, I will provide a "legislative

strength score" for each MSA that takes into account the presence of 37 different state gun laws from 2013 that either inhibit or encourage the use of firearms. My scorecard and scoring process is adapted from the Brady Campaign 2013 State Scorecard, and accounts for laws that require mental health reporting to the NCIS, prohibit gun possession by domestic abusers and other "at risk" individuals, mandate adequate child access prevention, impose background checks and waiting periods before gun transfers and private gun sales, allow discretion for the issuance of concealed carry permits, and prohibit open carry of all firearms, among other regulations.

In the next chapter of this thesis, I will provide a comprehensive literature review that assesses the existing body of academic research on gun violence. Next, I will include a methodology section with a description and explanation of all variables included in my analysis, testing methods used, and the predicted effects of each variable. I will follow with a data section that will provide descriptive statistics and sources for all variables included. Following the data section, I will include a regression analysis section that will note regression results in table form, summarize regression diagnostic techniques used, and evaluate the results for multiple regression models. Finally, my conclusion will reflect on policy lessons learned and make recommendations for future research on gun violence.

Through a comprehensive analysis of the impact of firearm legislation on firearm death rates across US urban areas, this thesis will offer objective insight into America's polarizing gun debate. By providing a greater understanding of the effects of gun legislation on firearm violence, this research can help inform policy decisions that may ultimately save thousands of American lives each year.

Chapter 2: Literature Review

Today, there is a plethora of gun research within the academic sphere that appears to both confirm and deny the effectiveness of gun control legislation in curbing firearm violence. This contradiction reveals an inherent lack of consensus among researchers and policymakers regarding the role of firearms in violent encounters (Dezhbakhsh and Rubin, 1998). While a number of researchers argue that increased access to firearms has a facilitating effect on gun violence, other researchers claim that increased access by non-criminals has a deterrent effect on gun violence (Dezhbakhsh and Rubin, 1998*).*

I will begin this section with an examination of various academic publications that conclude that stricter gun control legislation is largely ineffective in curbing firearm deaths, violence, or general crime rates. This section will also cover publications that conclude that lax gun legislation is effective in curbing firearm deaths, violence, or general crime rates. I will then examine various academic publications that find a correlation between stricter gun control legislation and lower firearm deaths, violence, or general crime rates. Next, I will review publications with no conclusive findings with regards to the effectiveness of gun control legislation. Finally, based on an evaluation of the current literature, I will provide recommendations for future research on this topic.

<u>Part 1: Fewer Firearm Restrictions are Correlated with Lower Firearm Death Rates</u>

While the topic of gun control has captured the attention of modern researchers, a large body of the original high-profile gun violence research was first published during the 1980's and 1990's. Although these earlier publications make use of old data sets, they often continue to serve as notable references for researchers. Moreover, although these earlier publications' conclusions vary depending on research question, methodology, and sample size, many of these high-profile

early works conclude that most gun control measures are ineffective in curbing violence (Kleck, 1991; Kleck & Patterson, 1993; Lott and Mustard, 1997).

For instance, when examining the net impact of gun prevalence on violent crimes, Kleck and Patterson (1993) conclude that, "gun prevalence levels generally have no net positive effect on total violence rates," "gun control restrictions have no net effect on gun prevalence levels," and "most gun control restrictions generally have no net effect on violence rates." They note a few exceptions to this last conclusion, indicating support for stricter gun licensing and permitting and stronger penalties for illegal possession (Kleck and Patterson, 1993). In conducting their research, Kleck and Patterson (1993) collected data from 170 US cities with a 1980 population of at least 100,000. They coded the cities for "the presence of 19 major categories of firearm restrictions, including both state and city level restrictions" (Kleck and Patterson, 1993). They then measured gun prevalence levels and estimated city violence rates over three years using two-stage least-square methods. Their models covered all major areas of intentional violence and crime frequently committed with guns, including "homicide, suicide, fatal gun accidents, robbery, aggravated assault, and rape" (Kleck and Patterson, 1993).

In the years since Kleck and Pattersons' works have been published, some academics have taken issue with their methodology and conclusions. In particular, Alba and Messner (1995) cite concerns with findings in both Kleck and Patterson (1993) and Kleck (1991), a precursor to Kleck and Patterson that used similar methodology and arrived at similar conclusions. In both publications, the authors use a variety of proxy indicators to determine gun prevalence at the city level. The majority of these proxy indicators involve calculating the percentage of different crimes committed with a gun, and then extrapolating these percentages to the general city population to determine an estimate for overall gun prevalence. By using gun violence rates as a proxy for gun prevalence, Alba and Messner note that the authors inadvertently imply that high

gun ownership is strongly correlated with high criminal usage of guns, thereby weakening the authors' primary assertions that guns are most often used by non-criminals for defensive purposes (Alba and Messner, 1995). In addition, Alba and Messner note that these gun prevalence variables switch from one model to another and have questionable validity. Kleck (1991) also uses data from the National Crime Survey (NCS) to determine that violent encounters with guns are not necessarily more lethal than violent encounters without guns. Alba and Messner (1995) note some selectivity bias arising from use of this data, as non-gun incidences reported to the NCS are more likely to be especially violent, thereby over-estimating the non-violent nature of gun encounters. More generally, Alba and Messner take issue with erroneous interpretations of data as well as the authors' tendency to construct a paradigm touting the benefits of an armed society (Alba and Messner, 1995).

Despite the controversy, other authors have drawn similar conclusions with regards to the efficacy of defensive gun use. In particular, Kleck and Gertz (1998) conducted a nationwide telephone survey to assess the deterrent effects of guns within the United States (Kleck and Gertz, 1998). They extrapolated their findings to determine that guns are used as many as 2.5 times a year to defend against crime. Vizzard (2015) takes issue with Kleck as well as Kleck and Gertz' findings, noting that their results would "translate into about 600,000 lives saved per year, or 300 times the total reported murders in the United States."

Other researchers have noted a correlation between concealed carry laws and lower crime rates. For instance, Gius (2013) assessed the effect of state-level assault weapon bans and concealed weapon laws on state-level murder rates. Using data from 1980-2009 while controlling for state and year fixed effects, Gius concludes that states with restrictions on the carrying of concealed weapons had higher gun-related murder rates than other states, while assault weapons

bans did not affect murder rates at the state level, although these latter results did not reach significance at the .05 level (Gius, 2013).

Similarly, in a highly publicized and controversial article, Lott and Mustard (1997) conclude that that allowing citizens to carry concealed weapons deters violent crimes, without increasing accidental deaths. The authors reach their conclusions through a cross-sectional time series multivariate regression analysis that assesses the impact of concealed carry laws, arrest rates, and a variety of demographic variables on crime rates across US counties from 1977 to 1992 (Lott and Mustard, 1997). In general, Lott and Mustard found that the implementation of concealed carry laws was correlated with a decrease in violent crime such as homicide, rape, and aggravated assaults. At the same time, they found that concealed carry laws were associated with an increase in robbery rates and auto theft (Lott and Mustard, 1997). The authors attribute their findings to a "deterrent" effect in which increased concealed carrying of firearms by non-criminals deters criminals from carrying out violent crimes. Criminals then substitute these violent crimes for less violent crimes like robbery or auto theft (Lott and Mustard, 1997).

Due to its high-profile and controversial nature, Lott and Mustard (1997) has received significant attention from the academic community. In particular, Olson and Maltz (2001) attempted to replicate Lott and Mustard's study while further disaggregating the analysis by weapon type, victim characteristics, and victim-offender relationships (Olson and Maltz, 2001). In general, the authors support Lott and Mustard's conclusions regarding the deterrent effects of concealed carry laws on crime. Specifically, Olson and Maltz re-ran Lott and Mustard's multivariate regressions using only large counties to avoid sample bias resulting from small counties (Olson and Maltz, 2001). They found that implementation of concealed carry laws were associated with lower gun homicide rates, with a much larger effect than found by Lott and Mustard, and higher non-gun homicide rates (Olson and Maltz, 2001). However, the authors did

note some issues with Lott and Mustard's data, including some missing offense and arrest data as well as some variation in the quality of reporting across counties (Olson and Maltz, 2001). Lott and Mustard also cited and attempted to rectify many of these data-related issues (Lott and Mustard, 1997).

Dezhbakhsh and Rubin (1998) take issue with Lott and Mustard's statistical modeling techniques and assumptions, as well as the assumption that the effect of the law is fixed across all counties (Dezhbakhsh and Rubin, 1998). By estimating two separate crime equations and running a two-stage least-square regression, the authors determined that concealed carry laws would generally have a very small crime-reducing effect on murder rates, little effect on rape, and would be generally correlated with an increase in robbery rates. In addition, the authors found that the law more often correlates with crime reductions among counties that spend more on police protection, and note there is little evidence that potential criminals use these laws to obtain guns and commit crimes. Other related findings are further discussed throughout the paper (Dezhbakhsh and Rubin, 1998).

Other researchers have been slightly more critical of Lott and Mustard's findings. For instance, Black and Nagin (1998) determine that Lott and Mustard's results were "highly sensitive to small changes in model and sampling" (Black and Nagin, 1998), and when these changes were made, they found "no basis for Lott and Mustard's confident conclusions about the impact of right to carry laws on violent crime" (Black and Nagin, 1998). In reaching their conclusions, Black and Nagin re-run Lott and Mustard's regressions but make several substantive changes, including dropping small counties to correct for selection bias, examining states separately, and adjusting the model to reflect year-by-year differences before and after the concealed carry law's implementation (Black and Nagin, 1998). When Florida was dropped from

the equation, the new regression results revealed that Lott and Mustard's assault effect remained unchanged but the homicide and rape rate effect disappeared (Black and Nagin, 1998).

Similarly, Webster et al (1997) cite a number of flaws with Lott and Mustard's modeling, including the authors' assumptions that all concealed carry permits are issued equally in states that implement the law, the use of the "arrest ratio" as an independent variable when it is highly correlated with the dependent variable (crime rates), and Lott and Mustard's failure to take into account the cyclical nature of crime trends in their conclusions.

Lott (1998) rebuts a number of the critiques described above. Specifically, he claims that his publication did in fact include individual state and county time trend variables as well as individual-year dummy variables to account for possible cyclical trends over the years examined. In addition, he notes that his results were in fact reported with arrest rates excluded, that the majority of his findings remained significant or became more statistically significant even when Florida was dropped from the analysis, and that his study did in fact take into account the impacts of differentiated implementation of permit laws across counties (Lott, 1998).

Part 2: More Firearm Restrictions are Correlated with Lower Firearm Death Rates

While a number of high-profile publications have noted a correlation between lax firearm laws and lower crime rates, a number of others have noted a correlation between stricter firearm laws/fewer firearms and lower firearm violence.

Similar to Lott and Mustard (1997), McDowall et al (1995) evaluated monthly homicide counts in urban areas within Florida, Mississippi, and Oregon before and after implementation of concealed carry permit laws. After using multiple methods to control for potential exogenous impacts on homicide counts, the authors found that firearm homicides increased in the aftermath of concealed carry laws. They conclude that concealed carry laws do not reduce homicides in

urban areas, but caution against strongly concluding that concealed carry laws raise levels of firearm murders (McDowall et al, 1995).

In addition, a number of academics have found associations between high gun ownership and higher homicide rates. In particular, Cook and Ludwig (2006) examined new estimates of the effect of household gun prevalence on homicide rates by estimating the elasticity of homicide with respect to gun ownership (Cook and Ludwig, 2006). The authors' findings suggest that, "gun prevalence is positively associated with overall homicide rates but not systematically related to assault or other types of crime" (Cook and Ludwig, 2006). Similarly, both Siegel et al (2013) and Miller et al (2002) determined that states with higher rates of gun ownership had disproportionately higher rates of homicide. In determining gun prevalence, all three publications used the percentages of suicides committed with a gun as a proxy measure (Cook et al, 1995; Siegel et al, 2013; Miller, et al, 2002). Although these and similar proxies were also used by Kleck (1991) and Kleck and Patterson (1993), the use of these proxies have drawn criticism from some academics (Alba and Messner, 1995). In addition, Westphal (2013) takes issue with Cook and Ludwig's conclusions, attributing their elasticity findings to a ratio fallacy as opposed to a true effect.

Other studies have determined a relationship between higher gun prevalence and increased suicide rates. For instance, Kellermann et al (1993) interviewed police and control subjects of suicide victims in two counties over a 32-month period. Using crude and adjusted odds ratios with matched pair methods as well as conditional logistic regression, the presence of one or more guns in the home was found to be associated with an increased risk of suicide (Kellermann et al, 1993).

Similarly, Conner and Zhong (2003) evaluated the associations of restrictive state firearm laws with the incidence of state suicide rates among men and women in 1999 and 2000. States

were divided into three categories based on the restrictiveness of their firearm laws, and "state suicide incidence rates stratified by gender were compared using Poisson regression analyses that controlled for measures of race/ethnicity, income, and urbanization" (Conner and Zhong, 2003). In analyses of both men and women, the authors found that suicide rates were higher in states with modest and unrestrictive laws as compared with states with restrictive laws (Conner and Zhong, 2003). However, some researchers have taken issue with the practice of dividing states into categories based on aggregate gun laws (Kovandzic, 1998).

Other researchers have examined the impact of firearm legislation on non-fatal firearm injuries. For instance, Simonetti et al (2015) assessed whether stricter state-level firearm legislation was associated with lower hospital discharge rates for nonfatal firearm injuries. In conducting their study, the authors estimated discharge rates for hospitalized and emergency department-treated nonfatal firearm injuries across 18 states in 2010. They then used "negative binomial regression to determine whether strength of state firearm legislation was independently associated with total nonfatal firearm injury discharge rates" (Simonetti et al, 2015). The authors conclude that, "there is significant variation in state-level hospital discharge rates for nonfatal firearm injuries, and stricter state firearm legislation is associated with lower discharge rates for such injuries" (Simonetti et al, 2015).

In addition, a number of researchers have evaluated the correlation between the restrictiveness of state gun laws and overall firearm deaths per capita. For instance, Fleegler et al (2013) ran a clustered Poisson regression analysis comparing 2007-2010 firearm-related state mortality rates to state-level firearm legislation while controlling for a variety of salient variables. States were divided into quartiles based on their "legislative strength score" across five categories of gun control laws. The researchers found that states in the highest quartile of legislative strength had a lower overall firearm fatality rate, suicide rate, and homicide rate than those in the lower

quartiles (Fleegler et al. 2013). Similarly, Kwon, et al 1997 conducted multiple linear regression models to evaluate the relationship between the number of per capita firearm related deaths and the restrictiveness of state gun laws while controlling for a variety of relevant variables. To assess restrictiveness, states were divided into two groups: states with no gun restrictions and states with gun restrictions. The researchers found that gun control laws had a very mild impact on firearm deaths while socioeconomic variables such as a state's poverty rate, unemployment rate, and increased urbanization had a more significant impact on firearm related deaths (Kwon et al, 1997).

Kovandzic (1998) takes issue with Kwon et. al's work. In particular, he argues that aggregating states into two categories based on gun law restrictiveness overlooks important differences within the analysis. Moreover, he notes that it is problematic to use states as a unit of analysis because states are composed of "vastly different ecological units" that may disrupt the findings. Moreover, Kovandzic notes that the most restrictive gun laws in the US are at the local level, so state level analysis fails to capture these important differences within states (Kovandzic, 1998).

Part 3: Inconclusive Results on Effectiveness of Firearm Restrictions

While individual studies often reveal a positive or negative correlation between gun legislation and firearm violence rates, as a whole many of these studies tend to yield inconclusive results.

In particular, Haun et al (2003) conduct an aggregate meta-analysis of 51 existing studies assessing the effectiveness of firearm laws in preventing gun violence. A variety of gun laws were evaluated, and a "median was calculated as a summary effect measure for each outcome of interest" (Haun et al, 2003). Haun et al concluded there was insufficient evidence to determine the effectiveness of any of the firearm laws or combination of laws on violent outcomes due to

small sample sizes, "inconsistent evidence of effectiveness, and limitations in design and execution of available studies" (Haun, et. al, 2003).

<center>Part 4: General Directions for Future Research</center>

The publications reviewed in this section adopt a range of methodological approaches and utilize a variety of independent and dependent variables. Yet academic consensus remains elusive on nearly every aspect of this important topic. Nevertheless, a number of highly salient variables still appear to be missing from these existing analyses. By conducting a new analysis with additional variables and refined methodology, I will rectify many of the errors committed in previous research while offering deeper insight into the efficacy of firearm legislation in the United States.

In the next chapter I will describe my data and methods, drawing on several "best practices" identified from my literature review. Specifically, I will use MSAs as my unit of analysis, independently analyze two categories of firearm death, take into account a variety of firearm legislation categories, and control for a number of unique independent variables across metropolitan statistical areas.

Chapter 3: Methods

In this chapter I summarize the data I used for my study and the methodology I followed for my regression analyses. More specifically, I provide a summary of the unit of analysis and variables I used, how I set up my regression analyses, and the methods I employed in running my regressions.

As noted in the prior chapter, previous studies assessing firearm restrictions or firearm prevalence often use either counties or states as a unit of analysis. However, as Kovandzic (1998) notes, using states as a unit of analysis is highly problematic since states are heterogeneous units that may promote biased results. In addition, a number of academics have cited issues with the indiscriminate use of counties as a unit of analysis, as smaller counties also often produce biased estimates (Olson and Maltz, 2001; Black and Nagin, 1997). In my own research, I used metropolitan statistical areas (MSAs) as my unit of analysis. MSAs are large geographic entities across the United States that contain a core urban area of 50,000 or more in a population. Each MSA consists of one or more counties in a core urban area, as well as any adjacent counties that have a high degree of social and economic integration with the urban core. MSAs fall neatly within county boundaries, and are frequently used by federal statistical agencies in collecting, tabulating, and publishing federal statistics (US Census Bureau, n.d.). Through the observation of all MSAs throughout the United States, I better ensure homogeneity while helping to avoid sample bias across my analysis. A caveat should be noted, however. Due to the fact that Canadian and Mexican firearm laws vary significantly from US firearm laws, I dropped MSAs bordering Canada and Mexico from the analysis.

In terms of dependent variables, many academics evaluating gun control and gun prevalence choose to test the following variables related to firearm violence: crime rates (consisting of gun and non-gun homicide, suicides, fatal accidents, rape, aggravated assault, and

robbery), gun and non-gun homicide rates only, non-fatal firearm injuries only, suicides only, and overall firearm deaths per-capita. However, few academics adequately separate this last category of firearm deaths. Within my own study, I ran multiple multivariate regressions specifying both firearm homicide and suicide rates as dependent variables. Running separate analyses for both homicides and suicides allowed me to better isolate and evaluate the impacts of firearm legislation on individual types of firearm death.

I collected my firearm death data from the Center for Disease Control and Prevention Wonder dataset, specifically examining county firearm death data from 2013-2015 (Center for Disease Control and Prevention, n.d.). Data from this dataset are reported as numerical death counts per county as well as by crude death rates per 100,000 residents in a county. I used this latter category for my regression functions. For confidentiality purposes, the CDC suppresses county death counts when there are fewer than ten deaths in a county. In addition, the CDC marks crude death rates per 100,000 as "unreliable" when there are fewer than 20 deaths in a county. In justifying this policy, the CDC notes that, "a death rate based on fewer than 20 deaths has a relative standard error (RSE(R)) of 23 percent or more. A RES(R) of 23 percent is considered statistically unreliable" (Center for Disease Control and Prevention, 2016). For the sake of increasing my sample size, however, I nevertheless included these "unreliable" crude death rates in my regression analyses.

In terms of firearm legislation, my primary independent variable, I also ensured that individual laws and types of laws were taken into consideration. Specifically, I adopted a methodology first designated by the 2013 Brady Campaign Scorecard. In particular, I assigned a positive or negative point to each MSA for the presence of a particular restrictive or lax state firearm law, with positive point values assigned to restrictive state laws and negative point values assigned to lax state laws. I considered a total of 37 state laws spanning the following categories:

Background Checks and Access to Firearms (23 positive points possible), Other Regulations of

Sales and Transfers (24 positive points possible), Gun Owner Accessibility (18 positive points

possible), Firearms in Public Places (6 positive points and 9 negative points possible), Classes of

Weapons and Ammunition/Magazines (13 positive points possible), Consumer and Child Safety

(7 positive points possible), Investigating Gun Crimes (2 positive points possible), Local

Authority to Regulate (6 positive points possible), and Other (3 negative points possible).

In reality, gun laws are not neatly bounded by state borders. Indeed, gun policies from

states with less restrictive firearm laws have been found to "spillover" into bordering or nearby

states (Coates and Pearson-Merkowitz, Forthcoming). To account for this possibility, I first

assigned a "1" dummy variable to MSAs that border or span one or more states. I then identified

12 state firearm laws that I determined to be especially susceptible to spillover effects. When

assigning positive or negative point values to bordering MSAs, I first assigned the legislative

score of the least restrictive bordering state for these particular 12 state firearm laws. For the

remaining 25 firearm laws that I did not mark as particularly susceptible to spillover effects, I

assigned the laws of the MSA's home state. For MSAs that span multiple states, I first assigned

the 12 "spillover" laws based on the laws of the MSA's least restrictive bordering state. I then

assigned the remaining 25 "home state" laws based on the "home state" of each individual county

within the MSA. I then averaged all county legislative scores within each MSA to determine an

aggregate MSA score for these special overlapping MSAs.

It should be noted that identification of these 12 "susceptible laws" is somewhat arbitrary

and may be open to revision in future studies. In addition, it could be argued that compensating

for spillover effects in bordering MSAs alone is insufficient to account for the potential spillover

effects in MSAs across the nation. For instance, it could be argued that many MSAs within the

Northeastern region of the United States are susceptible to spillover effects from multiple non-

bordering states, since Northeastern states are often small and in close proximity to each other. These considerations were not accounted for in this particular study, but may be potential areas to account for in future research.

In terms of control variables, I incorporated several salient variables identified in previous studies. More specifically, I wanted to include a handful of variables that would adequately capture the general population make-up and socioeconomic status of an MSA. Previous studies have incorporated socioeconomic variables relating to age, poverty, education, unemployment, race, alcoholism, number of law enforcement employees, and population density (Kleck & Patterson, 1993; Lott, 1998; Fleegler, et al, 2013; Kwon et. al, 1997; Kwon et al, 2005). In terms of age, I wanted to capture the percentage of the MSA "youth" population that may be more likely to engage in gun violence. For socioeconomic status, I wanted to capture the percentage of the MSA population that was highly impoverished and had very low education levels, also likely correlates of higher rates of gun violence. In addition, I coded MSAs for location in a Southern state, as previous studies have identified a correlation between Southern states and higher homicide rates (Pinker, 2011). I determined that other variables relating to alcoholism, race, law enforcement levels, and population density were not feasible for this particular study.

Ultimately, I incorporated the following variables into each of my regression analyses: the percentage of the MSA population between ages 15-34, the percentage of the adult MSA population with less than a high school diploma, and the percentage of the MSA population living below the poverty line. Data from all of these variables was drawn from the US Census Bureau's 2010-2014 American Community Survey Five Year Estimates. In addition, I assigned a dummy variable to MSAs that are within "Southern states" as defined by the US Census Bureau.

It may be argued that the most salient variables impacting gun violence are gun ownership and guns already in circulation. However, these variables are incredibly difficult to track, as no national firearms registry currently exists, and many of the nation's firearms are currently unregistered (Law Center to Prevent Gun Violence, 2013). In addition, any national survey on self-reported gun ownership is likely prone to inaccuracies (Farley, et al., 2012). As such, these particular variables will be excluded from this analysis.

Below, I provide my expected regression equation, a summary table of all variable sources included in the analysis, and a descriptive statistics table for all variables. In addition, I provide a copy of the 2013 Brady Campaign Legislative Scores table with the "susceptible to spillover" laws highlighted.

<div align="center">Regression Equation:</div>

Firearm Suicide/Homicide Rates Per Capita = Constant Value + B_1(Strength of state firearm laws) + B_2(Percent of MSA population between ages 15-34) + B_3(Percent of MSA population with less than high school diploma) + B_4(Percentage of county population living below poverty line) + B_5 (Southern MSA) + Error Term

Table 3.1: Predicted Direction of Effect for Homicide and Suicide Models

Independent Variable Name/Description	Predicted Direction of Effect on Firearm Death Rates
Restrictive Firearm Legislation Score	-
Percent of MSA Between Ages 15-34	+
Percent of MSA with Less Than High School Diploma	+
Percent of MSA Below Poverty Line	+
Presence of Southern state	+

Table 3.2: Variable Description and Source

Variable Name	Variable Type	Variable Description	Variable Source
Firearm Suicides Per Capita	Dependent Variable	Number of firearm suicides per 100,000 residents in an MSA in 2013-2015	Center for Disease Control and Prevention Wonder Dataset, 2013-2015
Firearm Suicides Per Capita	Dependent Variable	Number of firearm homicides per 100,000 residents in an MSA in 2013-2015	Center for Disease Control and Prevention Wonder Dataset, 2013-2015
Firearm Legislation Score	Independent Variable	Numerical score ranging from -9 to 75 gauging the strength of an MSA's firearm legislation at the state level	2013 Brady Campaign Scorecard
MSA Age	Independent Control Variable	Percentage of MSA population between ages 15-34	US Census Bureau's 2010-2014 American Community Survey Five Year Estimates
MSA Education	Independent Control Variable	Percentage of adult MSA population with less than a high school diploma	US Census Bureau's 2010-2014 American Community Survey Five Year Estimates
MSA Poverty	Independent Control Variable	Percentage of adult MSA population living below the poverty line	US Census Bureau's 2010-2014 American Community Survey Five Year Estimates
Southern State	Independent Control Variable	0-1 dummy variable indicating whether MSA is located in a Southern state	US Census Bureau

Table 3.3: Descriptive Statistics for All Variables:

Variable Name	Number of Observations	Variable Mean	Variable Standard Deviation	Variable Minimum	Variable Maximum
Firearm Suicides Per Capita	375	8.75	3.58	1.26	27.80
Firearm Homicides Per Capita	253	4.56	2.77	.66	18
Firearm Legislation Score	362	14.82	23.19	-9	75
MSA Age	381	27.95	4.57	11.2	47.4
MSA Education	381	14.82	5.41	1.6	36.6
MSA Poverty	381	15.83	4.28	6.2	29.9
Southern MSA	381	.41	.49	0	1

Chapter 4: Analysis

In this chapter, I sought to determine a statistical correlation between the restrictiveness of state-level firearm laws and homicide and suicide rates across US metropolitan statistical areas. I did so by running two separate multivariate regression models: one examining the correlation between restrictive gun laws and suicide rates per capita, the other examining the correlation between restrictive gun laws and homicide rates per capita. I then applied a series of model specification changes and regression diagnostic techniques to each respective model.

In the sections below, I first provide a statistical analysis of the suicide model. Within this analysis, I respectively provide a scatter plot of the suicide residuals, a table summary of the regression outputs for multiple suicide regression models, a discussion of the substantive impact of the suicide regression findings, and a discussion of regression diagnostic techniques employed in the suicide model. In the following section, I provide an identical analysis for the homicide model.

Part 1: Suicide Regression Analysis

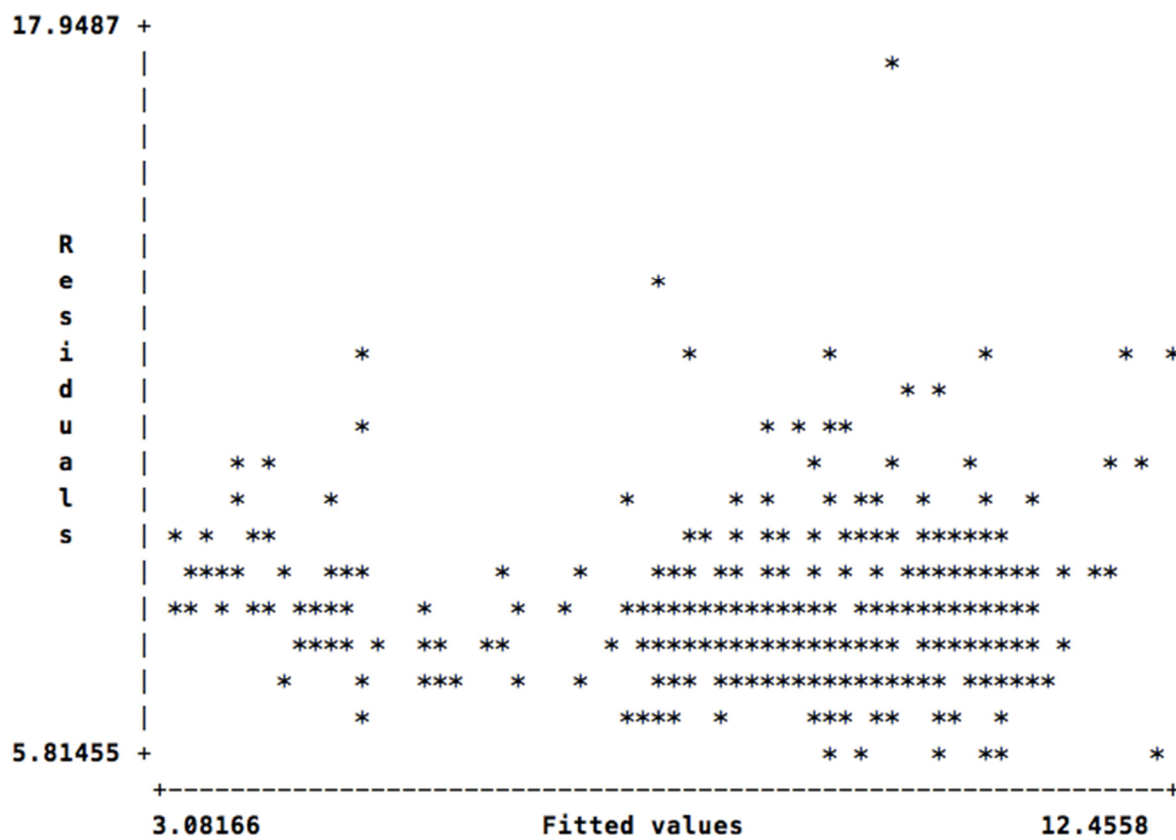Figure 4.1: Suicide Regression Residuals

Table 4.1: Regression Output for Suicide Model:

Variable Name/Description	Model 1: Coefficients and Standard Errors for Regular Suicide Rate	Model 2: Coefficients and Standard Errors for Regular Suicide Rate	Model 3: Coefficients and Standard Errors for Regular Suicide Rate	Model 4: Coefficients and Standard Errors for Logged Suicide Rate	Model 5: Coefficients and Standard Errors for Logged Suicide Rate
Constant	12.05 (1.58)	12.77 (2.03)	13.02 (1.72)	2.46 (.18)	2.46 (.18)
Firearm Legislation (legscore)	-.08* (.01)	-.08* (.01)	-.11* (.02)	-.01* (.00)	-.01* (.00)
Percent of MSA Between 15-34 (age1534)	-.16* (.05)	-.15* (.05)	-.17* (.05)	-.02* (.01)	-.02* (.01)
Percent of MSA Adults with Less Than High School Diploma (educ)	-.01 (.04)	-.08 (.13)	-.02 (.04)	.00 (.01)	.00 (.00)
Percent of MSA Below Poverty Line (poverty)	.14* (.04)	.09 (.11)	.11* (.05)	.01* (.00)	.02* (.00)
Presence of Southern State (southern)	.45 (.36)	.44 (36)	.54 (.37)	.07 (.04)	
Interaction Between Education and Poverty (interedpov)		.00 (.01)			
Interaction Between Leg Score and Poverty (interlegscorepov)			.00 (.08)		
Observations	355	355	355	355	355
F-Statistic	38.63*	32.18*	32.63*	60.43*	74.35*
R-Squared	.36	.36	.36	.46	.46
Adjusted R-Squared	.35	.35	.35	.46	.45

Regression Discussion: Suicide Model:

Overall, the suicide regression results indicated that restrictive firearm legislation had a very small but statistically significant effect on suicide rates per capita across US metropolitan statistical areas. As noted above, this effect remained constant despite alterations to the regression model. Substantively, the results reveal that a one-point increase in the restrictiveness of an MSA's firearm laws translates to eight fewer suicides within a population of ten million people. The direction of this effect is in accordance with my predictions. Although the size of this effect may seem extremely small, it makes sense to put this statistic into context with other highly prioritized threats in modern society. For instance, the US has spent an estimated $4.79 trillion on military expenditures in Iraq, Syria, Afghanistan, and Pakistan since 2001 (Crawford, 2016), yet the likelihood of an American perishing in a terrorist attack committed by a foreigner on US soil is approximately 1 in 3.6 million a year. Moreover, the likelihood of an American being killed by a refugee is approximately 1 in 3.64 billion a year, while the likelihood of an American being killed by an illegal immigrant is roughly 1 in 10.9 billion a year (Nowrasteh, 2016).

In addition to the legislation variable reaching significance, the regression results reveal that the age variable reached significance at the 0.05 level. More specifically, the results reveal that a one percentage point increase in the percentage of MSA residents between the ages of 15-34 translates to sixteen fewer suicides within a population of ten million people. The direction of these results seems counter-intuitive; I predicted that there would be a positive correlation between the percentage of young people in an MSA and the rate of suicide among the MSA population.

Among the other control variables in the suicide model 1 regression results, the poverty variable was the only other one that reached significance at a 0.05 level. Specifically, the model suggests that a one-percentage point increase in the percentage of MSA residents under the

poverty line translates to 14 additional suicides in a population of ten million people. Although the direction of this effect aligns with my predictions that poverty would be positively correlated with suicide, the direction of the effect was smaller than I expected.

For each of the three models, the R-squared value was equal to 0.36. As such, it can be said that the combination of variables in each respective regression model explains 36 percent of the variation in the dependent variable, respectively. Moreover, for each of the three models, the adjusted R-Square value was equal to .0;35. Thus, a more conservative estimate of R-Square tells us that the combination of variables in each respective regression model explains 35 percent of the variation in the dependent variable, respectively. As these rates are not very high, additional variables should be included in future models to better explain variation in the dependent variable.

<div align="center">Regression Diagnostics: Suicide Model</div>

After running my first suicide regression model, I conducted a series of diagnostic tests to determine the internal validity of my analysis. I began by running a residuals vs. predicted values plot of my first model to determine the distribution of my error terms (see figure 4.1 above). I found some heteroskedasticity, signifying a non-random distribution of error in my analysis. Furthermore, when I ran a Breusch-Pagan/Cook-Weisberg test for heteroskedasticity, I found significance (p = .01), indicating that my error variances were not necessarily evenly distributed and heteroskedasticity may be present.

Next, I tested for multicollinearity within my variables. I ran a variable inflation factor (VIF) test for my model, and found no variables had a VIF over 2.1. These results indicate that no multicollinearity was present among the variables. To confirm, I ran correlations for all variables and found no collinearity of concern.

I also tested for outliers among my variables. I ran a Cook's distance histogram and found minimal concern for outliers. Although Blacksburg-Christiansburg-Radford, VA had a high suicide rate of 27, this was not enough of a concern to drop this particular MSA from the analysis.

To test additional variations on my model, I logged the suicide rate, dropped the Southern variable, and generated several new interaction variables. I then ran additional regressions to test the respective effect of each of these changes on the model. In general, these alterations did not have substantial effects on the regression output. However, logging the data did substantially decrease the effect size of the legislative score, age, poverty, and especially the Southern variable.

Overall, the results from these suicide regression models imply that restrictive state-level firearm laws and higher poverty rates are correlated with a very small but significant impact on firearm suicides across US urban areas. Surprisingly, an increase in the MSA young adult population was also correlated with lower firearm suicide rates across US urban areas. However, as implied above, additional research is needed to verify these findings.

Part 2: Homicide Regression Analysis

Figure 4.2: Homicide R Residuals

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        2.52893                  Fitted values                 7.59565
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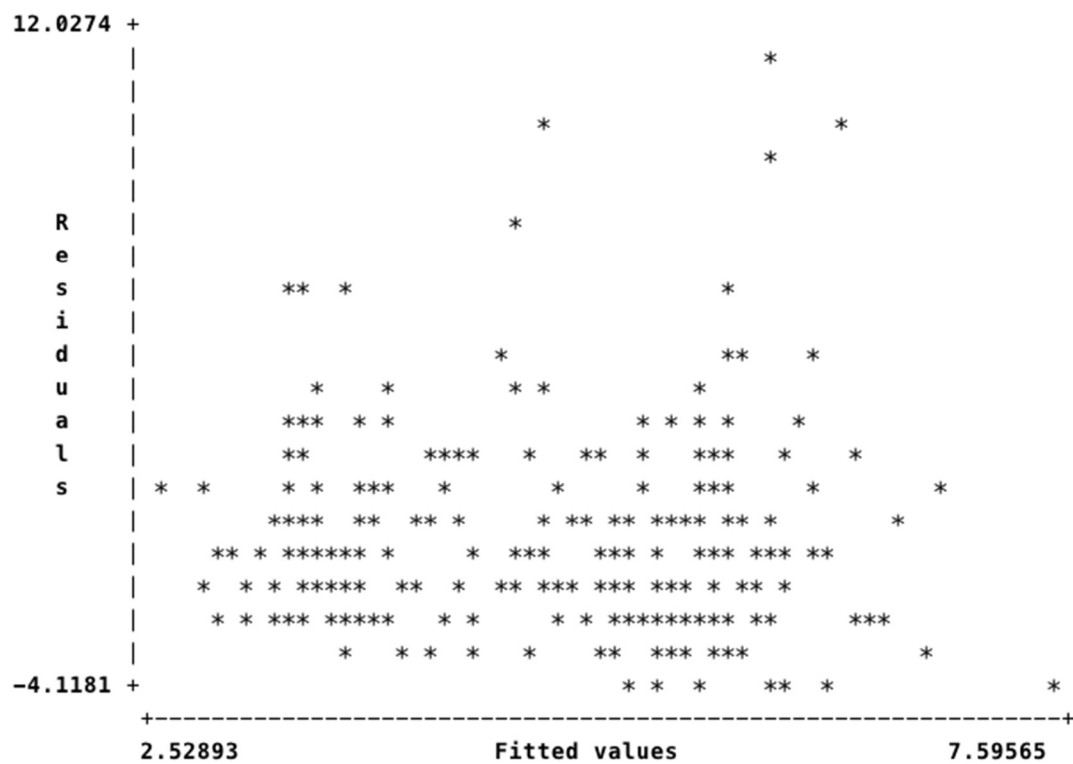
Table 4.2: Regression Output for Homicide Model:

Variable Name/Description	Model 1: Coefficients and Standard Errors for Regular Homicide Rate Variable	Model 2: Coefficients and Standard Errors for Regular Homicide Variable	Model 3: Coefficients and Standard Errors for Logged Homicide Rate Variable	Model 4: Coefficients and Standard Errors for Logged Homicide Rate Variable
Firearm Legislation (legscore)	.00 (.01)	-.01* (.01)	.00 (.00)	-.00* (.00)
Percent of MSA Between 15-34 (age1534)	.10* (.06)	.11* (.03)	.02 (.01)	.02 (.01)
Percent of MSA Adults with Less Than High School Diploma (educ)	.14* (.04)	.15* (.00)	.03* (.01)	.03* (.01)
Percent of MSA Below Poverty Line (poverty)	.05 (.05)	.09* (.03)	.01 (.01)	.03* (.01)
Presence of Southern State (southern)	1.46* (.42)		.35* (.08)	
Observations	237	237	237	237
F-Statistic	8.01*	6.66*	11.19*	9.07*
R-Squared	.15	.10	.20	.14
Adjusted R-Squared	.13	.09	.18	.12

Regression Discussion: Homicide Model:

Overall, regression results from all four homicide models indicate that restrictive firearm laws have no substantive impact on homicide rates per capita across US metropolitan statistical areas. Although the legislative score variable did reach significance on the two logged homicide models, the effect size was too small and too susceptible to model specification to signify a true impact on homicide rates per capita.

Among the other control variables in the homicide models, the "less than high school diploma" education variable is the only variable that remained consistently significant among all four models. However, the size of the effect varied substantially between the logged and unlogged models, indicating that the effect was also highly subject to specification error.

In addition, the Southern variable did remain significant within both the logged and unlogged models. Moreover, it registered a substantially larger effect size than the other two models. In the logged model, the results indicate that Southern states have on average 35 more homicides per ten million people than non-Southern states. This may be a variable to explore more deeply in future research. In particular, it may be interesting to determine what characteristics of Southern states render them particularly more likely to have higher homicide rates, even while controlling for the prevalence of youth, poverty, limited adult education, and laxer firearm laws. Pinker (2011) attributes the South's tendency towards increased violence to a greater reliance on "self-help justice" and a "culture of honor" that justifies rightful retaliation in the wake of mistreatment. He demonstrates that these particular mindsets have continued to pervade Southern laws, politics, and attitudes (Pinker, 2011). It may also be interesting to incorporate these Southern culture theories into future firearm research.

The control variables in all four models yielded the predicted direction of effect. That is, greater percentages of youth, impoverished, and uneducated residents in a population were

positively correlated with higher homicide rates. Moreover, as noted above, Southern states were also positively correlated with higher homicide rates. However, the legislative score variable did change direction and become significant when the Southern variable was dropped. However, as noted, this particular variable had a very small effect size and was highly subject to model specification.

Overall, the R-squared and adjusted R-square rates were very small for all four homicide models, ranging from 0.09 - 0.18. Substantively, this means that in all four models, the variables specified only explain 9 to 18 percent of the variation in the model. As with the suicide model, future research should attempt to incorporate additional salient variables in the homicide regression model.

<u>Regression Diagnostics: Homicide Model</u>

As with the suicide model, I ran a variety of diagnostic tests on my homicide model in order to determine the internal validity of my analysis. I again ran a residuals vs. predicted values plot of my first homicide model to determine the distribution of my error terms (see embedded figure and figure 4.3). I found some heteroskedasticity, signifying some non-random distribution of error in my analysis. To confirm, I again ran a Breusch-Pagan/Cook-Weisberg test for heteroskedasticity. The test yielded significance ($p = .00$), indicating that my error variances were not necessarily evenly distributed and heteroskedasticity was indeed present. From these diagnostics tests, I determined that logging the data might be beneficial.

Next, I tested for multicollinearity within my variables. I ran a variable inflation factor (VIF) test for my model, and found no variables had a VIF over 1.6. These results indicate that no multicollinearity was present among the variables. To confirm, I ran correlations for all variables and found no collinearity of concern.

Like my suicide model, I also tested for outliers among my variables. I ran a Cook's distance histogram and found no concern for outliers. I confirmed this by listing all variable values.

As noted, my initial regression diagnostics indicated that logging the model may be beneficial. While the logged homicide model did rectify the original model's heteroskedasticity, it did not substantially affect the legislative score variable's effect size or significance. It did, however, reduce the constant and Southern variables' effect size.

In my third model, I dropped the Southern variable and re-ran the regression with the regular unlogged homicide variable. Dropping the Southern variable made the legislative score variable barely significant with a very small effect size. It also rendered the poverty variable significant.

In my fourth model, I re-ran the regression with the logged homicide variable and without the Southern variable. In this model, the legislative score and poverty again reached significance, although the effect size was incredibly small. Otherwise, the model did not deviate substantially from the other logged model.

Overall, the regression outputs from the homicide model do not provide clear evidence of a correlation between more restrictive firearm laws and lower homicide rates across US urban areas. However, both Southern states and lower education levels were weakly but significantly correlated with increased homicide rates. In the final chapter of this study, I provide further reflections on both the homicide and suicide regression findings.

Chapter 5: Conclusion

The regression results produced in the above analyses imply that restrictive state-level firearm laws have a small but statistically significant tendency to reduce urban suicide rates. At the same times, such laws generally had no significant or decipherable effects on urban homicide rates. These effects remained consistent throughout various model specifications for both regression analyses.

These results should give pause to staunchly pro-gun or anti-gun activists advocating indiscriminately for laxer or more restrictive firearm laws. As the regression outputs imply, the impact of firearm legislation on gun violence is much more nuanced and complex than political pundits may infer. As much as policymakers and the general electorate may like to be able to synthesize a gun law's impact into a definitive statistic, this is simply not feasible given the complexities of this particular multi-variate analysis.

Nevertheless, the results from this analysis do raise questions regarding the validity of pro-gun arguments that attest to the harmful effects of restrictive gun legislation on public safety. In addition, the consistent findings for the suicide model should raise questions for policymakers in regions with especially lax firearm laws, especially those with high levels of poverty.

However, additional research is needed to affirm the findings of both the suicide and homicide model. In particular, future research correlating restrictive firearm laws with lower suicide rates would be significantly bolstered by the inclusion of variables measuring mental health status, depression, and drug and alcohol dependence within a population. In addition, future studies should run additional regressions isolating the effect of each individual firearm law with firearm death rates. While the regressions included in this study provide a fully comprehensive measure of the correlation between overall firearm law strength and firearm

homicide and suicide rates, further research should delve more deeply into the individual

correlations between each law type and its respective impact on gun violence.

Other improvements to quantitative analysis of the impact of firearm laws should also be

considered. Firearm injury rates should be incorporated into future regression models. Doing so

would substantially increase the model's sample size and validity, as well as provide a more

comprehensive picture of the impacts of firearms on gun violence. Unfortunately, non-fatal

firearm injury data by county or MSA was not readily available for this study. However, this

particular variable may be feasible for future studies adopting a different methodology or unit of

analysis. In addition, future research should take into account additional policy spillover effects

across state borders, as well as the level of discretion local authorities have in enforcing gun laws

at the city and county level. Moreover, future studies might incorporate longitudinal analyses

observing the change in gun violence in a region before and after a new firearm law takes effect.

Ideally, all these analyses would track statistics over more than a decade to better account for

potential random fluctuations in the data. Finally, and perhaps most importantly, future research

should take into account an accurate estimate of existing firearm ownership levels and firearm

circulation within a population. As noted, there is currently no accurate method of accounting for

these variables, as no national firearm registry exists and self-reporting surveys are often prone to

error (Law Center to Prevent Gun Violence, 2013; Farley, et al., 2012). Nevertheless, such

variables are crucial for truly comprehensive analyses of firearm legislation and gun violence.

Despite these limitations, this study ultimately accomplishes several goals. First, as noted

above, the findings raise questions for gun law advocates who tend to correlate restrictive firearm

laws with diminished public safety. Although valid research may attest to the "deterrence" effect

of laxer gun laws, (Lott, 1998; Kleck, 1991, Kleck and Patterson, 1993), one cannot assert the

opposite conclusion. That is, my research casts doubt on the argument that restrictive gun laws

increase crime rates. Indeed, the findings from both regression models provide no support for this conclusion. Second, the findings from the suicide model raise valid concerns regarding access to firearms among suicide-prone populations. Finally, the ambiguous findings from the homicide model highlight the complexities of this highly contentious topic.

As many policy makers have discovered, the "right approach" to gun violence is rarely obvious. The findings from my research only underscore the importance of conducting further comprehensive, objective research with new salient variables and refined methodologies. Ultimately, the hope is that such research will move us closer to sparing thousands of victims of gun violence for decades to come.

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